Quantum States: What the Hell Are They?

and

Darwinism All the Way Down (Probabilism All the Way Back Up)

(Both Expanded and Now In One Volume)

Selected Correspondence, 2001–2006

Christopher A. Fuchs

Quantum Information and Optics Research
Bell Labs, Lucent Technologies
Room 1D-426, 600–700 Mountain Avenue
Murray Hill, New Jersey 07974, U.S.A.

10 May 2006

Introduction

This document is the second installment of three in the *Cerro Grande Fire Series*. It is a collection of emails to do with quantum mechanics, much in the spirit of my previous installment `quant-ph/0105039`, *Notes on a Paulian Idea*. This time, however, many of the emails are more technical in flavor and the layout of the book is more continuous in thread. Much of what is recorded here concerns a fairly major transition in my thought. [MUCH MORE TO COME.]

DISCLAIMERS:

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

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

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

ACKNOWLEDGEMENTS:

I thank Scott Aaronson, Paul Benioff, Adan Cabello, Richard Campos, Carlton Caves, Robert Garisto, Osamu Hirota, Adrian Kent, Eugen Merzbacher, Klaus Moelmer, Ulrich Mohrhoff, Michael Nielsen, Jack Ng, Itamar Pitowsky, Arkady Plotnitsky, Damian Pope, Ruediger Schack, Ben Schumacher, Bill Wootters [AND SEVERAL MORE STILL TO BE INSERTED] for helping me believe that I am not crazy (or at least too crazy). I especially thank Hans Christian von Baeyer, Greg Comer, Steven van Enk, Andrei Khrennikov, David Mermin, Asher Peres, Huw Price, John Preskill, ...

Contents

Chapter

Quantum States: What the Hell Are They?

> Now my own suspicion is that the Universe is not only queerer than we suppose, but queerer than we can suppose.
>
> — J. B. S. Haldane

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

Merminition 1 : *You never replied to my comment on it: that you were getting close to Schrödingerian mysticism. I suspect the only difference (if it is a difference) is that you say the observer is in the world while Schrödinger — this is a cartoon version of what he says but is hard to resist in the current context — says that the world is in the observer. Both of you say (with Pauli) that they cannot be separated.*

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

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

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

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

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

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

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

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

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

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

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

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

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

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

> While Whitehead and classical physics are in sympathy with regard to experience as the starting point for natural philosophy, there is a dramatic difference with respect to the manner in which the two define "nature." In the materialistic world-view of classical physics, nature is the *object* that *causes* our experiences; what we experience are observables correlated with the primary and secondary properties of material substances that exist without the aid of experience. For Whitehead, however, "nature is that which we observe in perception through the senses." Nature is not the cause of experience; it is the field of experience itself. On this point Whitehead's philosophy insists: we are not concerned with discovering the concrete object which is the objective cause of experience, the nature of which is known in abstract concepts; we are instead concerned with explicating the origins of our abstractions by reference to the concrete factors nature revealed an experience. Thus Whitehead claims that
>
>> we have nothing to do with the ultimate character of reality. It is quite possible that in the true philosophy of reality there are only individual substances with attributes, or that there are only relations with pairs of relata .... Our theme is Nature ... we confine ourselves to the factors posited in the sense-awareness of nature ....

Against the traditional view of science, the Copenhagen Interpretation finds complete agreement with Whitehead's intention:

> As a final consequence, the natural laws formulated mathematically in quantum theory no longer deal with the elementary particles themselves but with our knowledge of them ... we can no longer view "in themselves" the building blocks a matter which were originally thought of as the last objective reality ... basically we can only make our knowledge of these particles the object of science.

It is important to note that when Heisenberg refers to "knowledge" he is not calling upon a conceptual abstraction but rather the experiences of investigators in their experiments. Since what is involved in this agreement between Whitehead and quantum theory is a redefinition of the concept of "nature" and since physics seeks to "explain" nature, it is not surprising that Bohr should call for "a reconsideration of our attitude towards the problem of physical explanation." Such a reconsideration is precisely the goal of Whitehead's philosophy.

and

In undertaking a metaphysics which would take the professed empiricism of science seriously and yet offer a conceptual elaboration of nature, Whitehead offers us an ontology of "events" as the real termini of our experiences in sense-awareness. Each event is an "actual entity" which is a concresence of prehensions of all previous events. In a "prehension" the prehending event brings within itself with a certain determinate "subjective form" the prehended event as an objective datum entering into the process that makes up the life of the prehending event. In this process the prehending event "becomes" while the prehended event achieves "objective immortality" and "perishes." There is no hylomorphic structure to the actual entities of this ontology; there is not "something that endures," material substance, and something which the enduring thing "has," properties, that can change. What is, is events, and events do not endure, they happen. In a prehension an event reaches out to "feel" other events; thus an actual entity "acts." Activity, not endurance, is the basic ontological status of entities in the philosophy of organism.

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

and

If we endorse the materialistic ontology, then Bohr's position is totally unacceptable, but on the Whiteheadian view his ideas follow quite naturally. The common point, as has been stressed, is that both positions are interested in a reformulation of the doctrine of physical explanation by an appeal to direct experience. However, where Whitehead analyzes experience in general, Bohr, as a physicist, is interested in only a small set of experiences, namely those observations in which a measurement of a microsystem takes

place. Whitehead protested against the idea of experience as experience of an impassive object "out there" related at each precise instance in time to a subject that in no way reached out to modify the object experienced. Since at each instance the subject is allegedly related to the object in a perfectly determinate way, there is no becoming *within* an experience; becoming can only be the succession of experiences, one following, instant after instance in a continuum, upon the other. If nothing becomes within the instant, nothing happens; there is no activity in experience. Experience, on this view, is quite literally, a point instant in time in which nothing happens. Against this notion Whitehead's process view holds that the essence of an experience is the *activity* of the prehending or experiencing entity.

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

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

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

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

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

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

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

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

and

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

and

> This was justified on its view because "observation" in the Cartesian framework refers to an event in the cognitive domain, *i.e.*, the human "mind," and thus even

though the careers of spatio-temporal substances are the "cause" of this observation, it cannot be described as a *physical* interaction. The Cartesian ideal which pictures what the universe would look like even if no one was there to look at it is the viewpoint of a ghost spectator who pilots without physical effort his corporeal submarine through a space-time sea.

In a quantum-era philosophy of nature this dualist approach to observation must be thrown out. The empirical starting point of science is the description of a phenomenon through a very specialized set of concepts in which that phenomenon is described as an observation of a neutrino or a quasar. Thus the description of observation as interaction which was exactly what had to be *left out* in the classical account now becomes exactly *what it is* that is described in the quantum description of microphysical reality.

For this reason the realistic interpretation of quantum physics requires not only that we discard the spectator account of knowledge, but it also denies the presupposition that "truth" refers to a property of statements and exists in virtue of a reference relationship between terms in these statements and the properties of an independent reality to which these terms correspond. Now we learn that in physics we characterize an independent reality *not* by attributing properties to some subtantial entity which is imagined to possess those properties apart from our interaction. Instead we characterize it through the phenomena which occur in our interactions with it. Truth, then, is not a property of statements but a property of the whole theoretical structure which allows us to predict those sorts of phenomena, and such a theoretical structure has that "truth" in virtue of its power to predict successfully precisely those phenomena in which we are said to observe these objects.

The collapse of the hope for a hidden-variables theory which would preserve the separability of the states of mechanically isolated systems, and the dispelling of the myth that quantum physics was conceived in an anti-realist spirit now make it necessary to take seriously a philosophy of nature which represents real microentities as the seats of objective potentials for interaction. Such a philosophy of nature will no longer characterize as ontologically fundamental those primary properties which characterized the classical body. Material objects are not vast collections of tiny extended bodies, Democritean atoms or Cartesian *rei extensae*. In breaking the presumed link between the primary properties of the classical mechanistic framework and those properties which are conceived to be ontologically fundamental, the quantum revolution point towards a philosophy of nature which "atomizes" not bits of matter, but elementary processes of interaction.

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

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

and

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

and

> Assuming that there are a plurality of individuals in nature, an ontology must provide a means for individuating them from each other. This need for a principle of individuation entails that ontological discourse requires that at least some "fundamental" properties must be *possessed* absolutely by the entities of which they are predicated. To see why this is so, consider that if no properties were possessed, then all properties would be relational. But in this case there would be no way to individuate the relata as distinct entities between which the relation is said to hold. Indeed, the supposed relation could not be external between the relata, but becomes a relation "internal" to the complex whole of the supposed relata. But to say that a relation is internal to a nonreducible whole, is in effect to say that the whole, treated as an individual, possesses a certain property. So we are back to the need for possessed properties.

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

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

Merminition 2: *The real issue is nothing less than how you and I can each construct a representation of the manifold aspects of our individual experiences (loosely known as a world), and the constraints that my representation imposes on yours, and vice-versa. By focusing explicitly on the strange information-processing capabilities inherent in the quantum mechanical description of physical reality, the new discipline of quantum information offers an opportunity to put on a sound foundation what was only hinted at in the convoluted prose of Bohr, the facile sensationalism of Heisenberg, the aphorisms of Pauli, and the poetic mysticism of Schrödinger. If it hasn't occurred to you that this is the real justification for your quantum information-theoretic pursuits, then you owe it to yourself to pause and peruse these pages.*

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

Merminition 3 :

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

I detect irony.

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

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

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

Merminition 4 : *I know Fuchsian Genesis and its variations almost by heart at this point, but I didn't think that was what I was saying. I wasn't thinking of the constraints in terms of your information gathering requiring my information loss. I had in mind something more like a static situation. Not very well defined (or I'd be done). But maybe it's the same thing. Will think about it.*

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

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

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

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

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

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

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

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

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

The DIMACS Meeting Axioms

Quantum Axiom 1

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

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

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

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

$$\Pi = \text{"the quantum state"}$$

Quantum Axiom 2

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

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

where U is unitary.

Quantum Axiom 3

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

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

The index j labels the outcomes.

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

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

Quantum Axiom 4

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

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

where

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

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

Quantum Axiom 5

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

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

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

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

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

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

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

Merminition 5 : *The way you state it would lead most readers to think that it holds only for special states of the composite system.*

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

Merminition 6 : *I've never seen an argument that it's implied by 3, and am glad to see that you too state 5 as an independent assumption. Does anybody else? Or has anybody showed that it is implied by 3? Or produced a counterexample that satisfies 3 but not 5?*

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

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

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

14-05-01 *Today's Meeting Cancelled* (to N. D. Mermin)

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

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

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

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

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

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

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

The NATO paper has—of course!—gotten delayed again by this little outing.[1] I know that Gonis views me as one of the most evil men alive. But if it's not too arrogant, I think it might turn out pretty good over all. There is a fine line between trying to shock people into action and going too far, and I know I'm just playing it by ear. The manuscript just reached the 32 page mark. Once I get to the stage of having the "collapse rule argument" completely written down, I'll send the preliminary version your way: you seemed the most interested in that part of it.

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

> And now comes the last great surprise: it turns out that in describing atomic phenomena one particular picture cannot suffice: according to the circumstances we are obliged to make use of several quite different pictures; one can only describe the atomic world, as it were, from one point of view at a time, and the prospects it offers from

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

various angles are so different that they cannot be fused into one single picture in the usual sense.

This situation has given atomic physicists occasion for fascinating reflections upon the very essence of human cognition, which I shall not enter upon here. I just wanted to mention it as a concluding touch in the picture of the atomic physicist that I have attempted to outline. I scarcely venture to hope that I have succeeded in giving an adequate insight into his methods of work, but perhaps I have managed to persuade you that the secret weapons that have secured him such brilliant triumphs are persevering toil and common sense.

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

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

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

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

Merminition 7: *But if it happened to get two readers who appreciated its literary merits, do you think its subsequent appearance in PRL would make me a laughing stock in the quantum-info community? This worries me. Please advise.*

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

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

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

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

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

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

Bennettism 2: *Were you a Green? Did you really believe there was no difference between Gore and Bush?*

No, I wasn't a Green. And, I didn't believe that there would be no difference in the *consequence* of having Gore versus Bush in the White House.

But I do believe it of the quantum campaigns. And I do believe the reason there continues to be money in quantum foundations research is because the arguments all center around almost-invisible variations of the same thing.

Bennettism 3: *Nice introduction and motivation of the problem, especially the math vs physics versions of special relativity. However, in your table on Quantum Axioms and Imperatives, the one with the redundant right column, it seems to me you beg the question with "give an information theoretic reason".*

If I'm on the right track, the two columns really will be redundant. So, that's a good sign.

Bennettism 4: *Isn't information theory, no less than linear algebra, a branch of mathematics, rather than a branch of physics?*

But rarely is a branch of mathematics laid down without a more fuzzy kind of motivation working in the background. The fuzziness comes first, the formalization comes next. But the meaning and the value of the final construct—i.e., the final formal system—remains in the fuzziness. What I'm really asking us to do in that paper is to lay open the motivation for why we have the particular structure we do in quantum mechanics. If I had to place a bet, it would be that the answer will be found along information-theoretic or decision-theoretic lines. You undersell those fields in not realizing that they have a significantly firmer grip on their origin than quantum mechanics does. (Remember Rüdiger's nice presentation of the "dutch book" argument for the probability calculus in Montréal last year? As I recall, you appeared, at least mildly, impressed. That is the flavor of things I'm hoping for for quantum mechanics.)

Bennettism 5: *And when you ask for a physical explanation, what do you mean by "physical"? It's like pornography—you can't define it but you know it when you see it.*

I know you meant to be sarcastic with this, but, roughly, yes. Yes, we will know it when we see it: the nagging, nasty feeling that something is missing from our worldview will disappear (at least in this avenue of science). Funding for quantum foundations meetings will—by community choice—dry up. One could argue that one's pet interpretation—like the Everett program, for instance—is already virtually there ... if the rest of the community would just stop being stubborn! But I just don't think the rest of the community will stop being stubborn until the essence of the theory can be taught to a junior high student. (Your Everett interpretation is not there, except in the trivial sense of saying "all possibilities equally exist" ... which one could have just as well have said of classical physics, asserting that every initial condition is equally valid.)

Bennettism 6: *You could say that the special relativity example, despite its patent physicality, is really a geometric axiom asserting that spacetime behaves like a Minkowski space rather than a Euclidean one. So how much worse is it to have a bunch of axioms saying that states behave like rays in a Hilbert space? To me quantum mechanics is a more essentially mathematical and less physical part of physics than special relativity is. In that regard it is more like thermodynamics, which concerns the macroscopic consequences of microscopic reversibility, and would apply in any world, for example a 2 dimensional classical-mechanical world, a discrete cellular automaton world, or a 5-dimensional special-relativistic world, as long as the underlying dynamics was reversible.*

You will find little disagreement with me about part of this: i.e., that quantum theory shares a lot with thermodynamics in its range of applicability. But that is precisely why I would call quantum mechanics more a "law of thought" than a "law of nature." Just as Boole did of probability theory. And just as Jaynes did with (classical) statistical mechanics. (In fact, either of you two—i.e., you or Jaynes—might just as well have written your quote above.)

You might say, and I suspect you will say, the distinction I draw between "nature" and "thought" is only a semantic one. But I don't believe that: I think it is precisely in making that distinction clear (and operational) that we have a chance of closing the quantum foundations debate and moving on.

Physical theory is about two things: what is and what we know (or what we believe). It's the process of putting the two things together that gives a prediction in any practical setting. Quantum theory, interestingly, seems to be a nontrivial jumble of those two things. I think that is a rather deep statement about the world, and one that we have not yet come to grips with.

I hope you'll read the whole paper from beginning to end when I finally post it. You know that I crave your respect. But I know that I have to earn it.

29-05-01 *'Typo in Note 6' and 'Worst of All'* (to N. D. Mermin)

Wootters! Wootters!

There are two t's.

29-05-01 *Bayes and Lorentz: Never the Twain Shall Meet* (to A. Peres)

I still owe you some comments on your paper. I guess in the end, I don't have much to say. I agree with its content of course. Not least of all, because you taught me long ago that the correct analogy is between quantum states and Liouville distributions, and not between quantum states and phase space points. For me, that means that both objects—quantum state and Liouville distribution—are subjective entities, and can therefore be changed by the whims and the fancies of the observer. Their valuations are not tied to physics per se. The learning of information always presents just such an opportunity: there is no law of physics that says that all observers must agree in their Liouville distributions for a given system. Measurement can lead to very nice inequities in that regard: an application of Bayes' rule is not Lorentz invariant, but then again it is not even *observer* invariant. We apply it when we gather information; we don't apply it when we don't.

29-05-01 *No Miracles* (to A. Peres)

Asherism 1 : *Exophysical agents are essential, and most people tend to sweep them under the rug.*

Yeah, you might say my *Notes on a Paulian Idea* is about looking under the rug.

29-05-01 *Substantiation* (to N. D. Mermin)

What particular piece in the paper directly substantiates the sentence: "It is possible to eliminate all couplings between the source and the destination because quantum qubits have a richer range of logical capabilities than do classical bits."

Have you tried explicitly walking through all your circuits with a classical Liouville distribution rather than a quantum state? That is to say, in Fig. 2 for instance, let $|\psi\rangle$ stand for a (Bayesian) probability distribution over $|0\rangle$ and $|1\rangle$, rather than a pure quantum state. Then you would have a classical state-swapping circuit: where the word state now means without doubt "state of knowledge."

What goes break and where goes break in getting from figure 2 to figure 10 in such a classical setting? Well, nothing of course, because we know that teleportation also works for mixtures. But it does probably mean that some components in the circuit are probably more "quantum" than they need to be to carry out such a classical project. (I.e., the project of teleporting a classical state of knowledge, via purely classical resources.)

Can you pinpoint where that occurs? It has now struck me that your view of teleportation may be a good laboratory for sussing that out. I'm warming up as a referee.

Today Renes arrives, and this evening Caves arrives, and ... and next Tuesday I have committed myself to lecturing on Holevo's channel capacity result, and—between all of that—somehow, someway, some pig (Charlotte's web), I must complete the NATO paper. I keep hoping for a miracle: none occurs.

You've never read it, I know, but you should read it: de Finetti's paper "Probabilismo," written in his youth while in a fascist fervor. Have the mental strength to divorce it from the fascism, and you will find it is an absolutely wonderful piece: B. de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989). [See also R. Jeffrey, "Reading *Probabilismo*," Erkenntnis **31**, 225–237 (1989).]

I've come to think of the NATO paper as my own Probabilismo. I just wish I had the youthful fervor.

06-06-01 *Comments and Answers* (to N. D. Mermin)

Wow. Thanks again for the very thorough reading! (And concern over my welfare.)

I reply to all your comments below. You've certainly left me hungry for more.

Merminition 8 : *I didn't really get the epigraph ("imprimatur") and the apparent explanation of it on p. 2. Is that a coy reference to the bad-boy tone?*

Which meaning of coy did you have in mind?

1. Tending to avoid people and social situations; reserved.
2. Affectedly and usually flirtatiously shy or modest.
3. Annoyingly unwilling to make a commitment.

Merminition 9 : *What is an Einselectionst? (p. 2.) Everything else is familiar, but, irritatingly, it's the one school you don't explain in footnote 2. Is this a (bad) joke you're playing on the reader?*

I'm letting that one stand: it was meant to be a little insulting. I can't figure out what their views are.

Merminition 10 : *p. 2. You should think twice about sentences like "If I am ever going to get . . . someone else's war." I know it's true, that physics would be a lot more fun if people weren't trained to hide such sentiments, etc. But it does have a certain self-indulgent quality.*

Stripped away with surgical precision . . .

Merminition 11 : *p. 4. Say the whole thing: 1) the speed of light IN EMPTY SPACE is [constant,] independent of the speed of its source. (Einstein did not say "is constant" — it's not necessary.) Keep "is constant" if you like, but why leave out his "in empty space" — that's crucial.*

Oh, alright!

Merminition 12 : *Bottom of p. 4. Can you explain classical E&M to a junior high-school student giving the essence, not the mathematics? Classical mechanics? Thermodynamics? I've always thought relativity was special that way.*

No. Not yet. Maybe. Me too. I.e., I consider E&M much more like the setting of a particular the Hamiltonian within QM: And I wouldn't expect a high-school student to be in a position to understand why we have chosen the particular Hamiltonians that we have in any practical situation. Since the overarching belief here is that "quantum mechanics is a law of thought" (with an almost trivial side-input of honest-to-god physics), it strikes me as being in a category much more like relativity. In the words of J. Bub, it is a "framework theory."

Merminition 13 : *Also p. 5, I've been meaning to ask you: why is H a vector space if the scalars are real or complex, but a module if they are quaternions? As I remember "module" has a precise meaning — maybe having to do with the scalars lacking inverses — which is not satisfied if they're quaternions. Isn't H still an ordinary vector space?*

According to Adler's book, it is a module. Why one makes such a distinction, I don't know: I just followed convention.

Merminition 14 : *Is footnote 5 on p. 7 a little too cute? [Just asking.]*

Let me think about this one. I added the footnote first as a joke to myself. But then I decided I sort of liked it. The motivation came from Bennett having no clue about what imagery I was trying to convey with zing.

Merminition 15 : *p. 11. I find your brief knee-fortifying rejoinder to Penrose somewhat of an anticlimax. If you had added explicitly that the answer to Penrose is that it's all about the RELATION between Alice and the qubit (even without invoking correlations without correlata) I would have thrown away my Ace bandage.*

Yeah, I thought it was a bit of an anti-climax too. I continue to search for ways to strengthen my rejoinder to Penrose and Jozsa (the z comes first).

But indeed you and I differ on the climaxes we seek. As far as I can tell, you seem to seek a predominantly static worldview. I want to see true becoming ... I think.

Perhaps I said something closer to what you'd like me to say presently in the Samizdat, pages 407–408 (in a note titled Penrose Tiles). Have a read of that, and tell me whether that meshes with what you would have liked to hear. Who knows, I may well reconsider (based on the old text).

Merminition 16 : *p. 11. I'd never seen so direct a statement of correlations (between Man and Nature) without correlata in Heisenberg before. Nice quote. Where's it from?*

W. Heisenberg, "The Development of the Interpretation of the Quantum Theory," in *Niels Bohr and the Development of Physics: Essays Dedicated to Niels Bohr on the Occassion of His Seventieth Birthday*, edited by W. Pauli with the assistance of L. Rosenfeld and V. Weisskopf (McGraw-Hill, New York, 1955), pp. 12–29.

> The criticism of the Copenhagen interpretation of the quantum theory rests quite generally on the anxiety that, with this interpretation, the concept of "objective reality" which forms the basis of classical physics might be driven out of physics. As we have here exhaustively shown, this anxiety is groundless, since the "actual" plays the same decisive part in quantum theory as it does in classical physics. The Copenhagen interpretation is indeed based upon the existence of processes which can be simply described in terms of space and time, i.e. in terms of classical concepts, and which thus compose our "reality" in the proper sense. If we attempt to penetrate behind this reality into the details of atomic events, the contours of this "objectively real" world dissolve—not in the mist of a new and yet unclear idea of reality, but in the transparent clarity of mathematics whose laws govern the possible and not the actual. It is of course not by chance that "objective reality" is limited to the realm of what Man can describe simply in terms of space and time. At this point we realize the simple fact that natural science is not Nature itself but a part of the relation between Man and Nature, and therefore dependent on Man. The idealistic argument that certain ideas are *a priori* ideas, i.e. in particular come before all natural science, is here correct. The ontology of materialism rested upon the illusion that the kind of existence, the direct "actuality" of the world around us, can be extrapolated into the atomic range. This extrapolation, however, is impossible.

Merminition 17 : *p. 11. "wake and dream"? waking and dreaming?*

I left that alone. The *American Heritage Dictionary* has a huge usage note associated with "wake." I took that to give me license, plus I like the flow of it.

In general, I think that paragraph is the deepest part of the whole paper. It is the Paulian idea.

Merminition 18 : *p. 13. Gleason derives the state-space structure? What do you mean? He talks about projections. What are they projecting on? What does "tr" mean? Isn't the state-space structure already there? You yourself begin by saying "Let P_d be the set of projectors associated with a ... Hilbert space ..."*

$$\text{state space structure (for me)} \quad = \quad \text{convex set of density operators}$$

The Hilbert space you are talking about—again for me—only has significance in that it defines the set of potential measurements. The quantum state, and with it the state-space structure, is a secondary notion.

But I will try to ward off confusion by adding the definition above to the text.

Merminition 19 : *p. 16. "sum total OF ways" I wrote in the margin around here that noncontextuality for POVMS seems to be an enormously stronger assumption than noncontextuality for projections . . .*

Yes. But so what? The point is—physically—the assumption is equally deep for both notions of measurement. Noncontextuality IS Bayes' rule. (Or just about so much.) It's just that applying it to POVMs actually gets you somewhere (without having the mathematical skills required to solve one of Hilbert's problems).

06-06-01 *The Haunting* (to N. D. Mermin)

You have this habit of haunting me: even when I say I'm not going to listen to you, I end up listening to you. Demon!

> Einstein was the master of clear thought. I have already expressed my reasons for thinking this in the arena of electromagnetic phenomena. Likewise, I would say he possessed the same trait when it came to analyzing the quantum. For even there, he was immaculately clear and concise in his expression. In particular, he was the first person to say in absolutely unambiguous terms why the quantum state should be viewed as information (or, to say the same thing, as a representation of one's knowledge).

07-06-01 *Frost* (to N. D. Mermin)

Just found this in my quote of the day:

> Poetry is a way of taking life by the throat.
>
> Robert Frost (1874-1963), U.S. poet. Quoted in: Elizabeth S. Sergeant, *Robert Frost: the Trial by Existence*, ch. 18 (1960).

I see you wrote me a note last night at 10:17. I'll have a look at it.

07-06-01 *The Mud of von Neumann* (to N. D. Mermin)

Merminition 20 :

> [CAF wrote:] Forget about the larger Hilbert space. It is artifice. It is only historical accident that has confused us for so long.

Now that I've added the discovery of POVMs to my discovery of the moons of Jupiter I'm less inclined to object to this. Indeed, I'd like it to be so. But if I give you a resolution of the (2 dimensional) identity for a single qubit into 17 positive operators, can you always tell me how to set up a corresponding procedure with 17 distinct outcomes without enlarging the system to one described by a larger Hilbert space?

The point is: If I give you a resolution of the (2 dimensional) identity for a single qubit into 2 projection operators, can *YOU* always tell me how to set up a corresponding procedure with 2 distinct outcomes without enlarging the system to one described by a larger Hilbert space?

Von Neumann didn't know how. He introduced the notion of a "measurement model," enlarged the Hilbert space, and got us into the muddle most of us are still in today. Now, people like Zurek are going around trying to justify the "pointer basis" on von Neumann's ancillary Hilbert space by enlarging it still further (and calling it "the environment"). And I'm sure you'll recall that von Neumann himself did even more dastardly things.

You just have to get into a different mindset. My attitude is it's time to cut the Gordian knot. That's what Section 4 and 5 are about. To the extent that one admits a mystery to the standard (von Neumann, orthogonal projection-valued) notion of measurement, one gains *nothing* by holding tight to it. One might as well transfer the mystery to the POVMs and be done with it. The advantage of the POVMs is that they are a conceptually simpler structure: the old von Neumann notion is just a horribly contorted surface of constraint within that beautifully smooth space. What is a measurement? A refinement of one's state of knowledge, full stop. Any refinement whatsoever? Yes, any refinement.

Let me give you a homework exercise. (Now I'm going to sound like Gottfried.) Go back to a classical setting where you have a probability distribution $p(h, d)$ over two hypotheses. Marginalizing over the possibilities for d, you obtain an initial state of knowledge $p(h)$ for the hypothesis h. If you gather an explicit piece of data d, then, using Bayes' rule, you should update your knowledge about h to $p(h|d)$. The question is this: Do you not find that transition $p(h) \longrightarrow p(h|d)$ a mystery you should contend with? Does it not bother you that if someone asked you for a *physical description* of that transition, you would be at a loss for words? I mean, after all, one value for h is true and always remains true. One value for d is true and always remains true. There is no transition for those variables. The transition is in your knowledge (or belief, if you will). Should we not have a detailed theory of how the brain works before we can trust in the validity of Bayes' rule? (I ask that rhetorically of course.) Should we close all the gambling houses in Nevada, on the suspicion that they know better of Bayes' rule (and its limitations) than we do and have been using that to their (nondisclosed) advantage all along?

In my view, recognizing the ridiculousness of the rhetorical question is the *first* step to freedom. Now we've got a long haul to go, but at least we're out of the parking lot.

I think I'll CC this note to Caves (across the hall) since parts of it seem to be a point of contention between us too.

07-06-01 *Modules Over a Division Ring* (to N. D. Mermin)

Actually, I picked up the word "module" in a footnote of Adler's, where he explains that he is going to flout convention and call the object a "quaternionic Hilbert space." (Or, maybe he said vector space instead.)

Anyway, "quaternionic module" does square with the definitions in MacLane and Birkhoff's book *Algebra*.

p. 134: "A division ring is defined is defined to be a non-trivial ring (not necessarily commutative) in which every non-zero element has a two-sided multiplicative inverse. A commutative division ring is thus the same thing as a field. An example of a non-commutative division ring is furnished by the quaternions . . . "

p. 190: "A module is an additive abelian group whose elements can be suitably multiplied by the elements from some ring R of 'scalars.' . . . This chapter will be concerned with general properties

of modules over field ('vector spaces') will be studied in the next chapter."

p. 253: "We have already noted that most of the properties of vector spaces (modules over a field) are shared by modules over a division ring. As the most important example of such a division ring we now construct the ring of quaternions."

I would like to know what cherished property of vector spaces goes bust with quaternionic modules ... but I just don't have the time for that now.

07-06-01 *Two Go Bust* (to N. D. Mermin)

Actually MacLane and Birkhoff say that only two of their theorems in Chapter 7 "Vector Spaces" fail for modules over a division ring. But I haven't been able to locate the theorems, and I think I'm going to have to give up for now.

07-06-01 *The Mud of Mermin* (to N. D. Mermin)

Merminition 21 :

> [CAF wrote:] The point is: If I give you a resolution of the (2 dimensional) identity for a single qubit into 2 projection operators, can *YOU* always tell me how to set up a corresponding procedure with 2 distinct outcomes without enlarging the system to one described by a larger Hilbert space?

Yes, I can. (That's why I picked that example.) The two orthogonal projections are necessarily of the form $(1 \pm n \cdot \sigma)/2$. *So all you do is find a Stern-Gerlach magnet and rotate it so its axis is along* n. *(I'm taking my qubit to be associated with the magnetic degree of freedom of a spin 1/2 particle that I can shoot between the poles of the magnet whenever I please. Tricky to do, of course, but not conceptually challenging.)*

Your rejoinder didn't faze me a bit. I just go to that same fine machinist who built your Stern-Gerlach device, and ask him to cut me two very good and very small mirrors, add the tippiest tip of glue to one of them, pass it through a dilute solution of just the right chemicals (so that just the right amount sticks to the glue). Et voila! In THEORY you CAN call such a thing an "atom in a cavity," that allows a $J = 0 \longrightarrow J = 8$ multipole transition (in principle) to be excited by a tenuous beam of light. We then apply a magnetic field and shine some auxiliary lasers in to "check" which of the 17 sublevels was "actually" excited.

I ask the machinist, "Did you feel particularly different when you worked for me than when you worked for Mermin?" He says, "Well you do pay better! But I promise you my prices were set by strictly objective criteria: I had to roam the earth to find that exotic chemical, and believe you me, that machining job was no easy task." I ask him, "Well, did you at least feel particularly quantum?" Just a guess, but I suspect he'll look at me with the same blank stare of the students on page 15 of my draft.

Merminition 22 :

> [CAF wrote:] von Neumann didn't know how. He introduced the notion of a "measurement model," enlarged the Hilbert space, and got us into the muddle most of us are still in today. Now, people like Zurek are going around trying to justify the "pointer basis" on von Neumann's ancillary Hilbert space by enlarging it still further (and calling it "the environment"). And I'm sure you'll recall that von Neumann himself did even more dastardly things.

I basically agree with all of this but I don't think it has much to do with the question I asked you, which was how to describe on the down-to-earth unphilosophical laboratory (FAPP) level how to set up an experiment whose distinct outcomes correspond to the distinct positive operators. I can think of ways to do it, but if I want to describe them in the language of QM I need a larger Hilbert space to do it. I want to know how to do it in a way that refers only to the original qubit, analogous to what I told you above.

You're just not getting the point. There is no difference in principle, between the machinist's fine work for you and his even finer work for me. But von Neumann, silly von Neumann, invoked extra Hilbert space for both jobs. At least he was consistent. You're not being consistent. What do you think the Stern-Gerlach device is if it's not extra Hilbert space (in the von Neumann view)?

Merminition 23 :

> [CAF wrote:] Let me give you a homework exercise. ...

Again, very nice, but it has nothing to do with what I was asking you.

No, it has everything to do with what you are asking. You're just causing me to drag you in kicking and screaming. The point is: You don't need to invoke physics to make sense of Bayes' rule. A part of the quantum confusion has come about precisely because people have wanted to invoke "physics" to make sense of quantum collapse. It hasn't happened in 75 years, and it's not going to happen now—or at least that's my bet. In your Stern-Gerlach example you explicitly throw away the issue of "where the outcome comes from." (Von Neumann tried to answer that issue but botched it.) But your example is precisely on the right track: you don't need to ask where the outcome comes from for Bayes' rule, and you don't need to ask it for the quantum.

Now I've got to go to lunch, and then try to recover from all the work I did NOT put into the paper today. I hope though that these notes I'm writing you are clearing a little bit of the mud away.

10-06-01 *Answers* (to N. D. Mermin)

Merminition 24 : *Before I torture myself making sure that the converse really holds, I need a little more education in the formalism. [...]*

So I'm putting myself in the place of your student on p. 16 and I'm looking for a simple example where the standard postulate you've dropped doesn't hold, so I can see for myself whether it makes a difference. What could be simpler than the POVM

$$E_b = p(b)I$$

where I is the identity and the $p(b)$ are non negative probabilities that sum to 1. Now I look at your rule for $P(b)$ and it tells me that $P(b) = p(b)$, independent of the density matrix of the system. So I raise my hand and ask why you call that a measurement when it doesn't tell you a damn thing about the system.

You will not say, "Ah, that's because the POVM you picked corresponds to the case where you turn on no interaction between the system and the ancilla, so of course the measurement of the ancilla tells you nothing about the system." You will not say that because (a) the point is to eliminate the ancilla and (b) to say nothing more about "measurement" than is in your table.

My guess is you will say that some POVMS are more informative than others and I have picked a particularly bad one (just as picking I for my observable is a particularly bad choice under the standard rules).

Yes.

Merminition 25 : *So then I say, "So there is some figure of merit for POVMs associated with how well their outcomes discriminate among various density matrices for the system? Could it be that the postulate you have dropped is there because it somehow maximizes this figure of merit?"*

There are two ways in which a POVM can be informative. 1) Suppose you have a density operator ρ for a system, and you know that someone else has a density operator ρ_i for that same system—where ρ_i falls within some fixed decomposition of ρ. You just don't know which value of i he happens to be using (outside of some prior probability). Therefore, you perform a measurement in an effort to obtain information about his i: this is the scenario of classical communication. 2) There is no extra player, there is simply ρ. However, you are dissatisfied with how little you can predict of a random measurement in the future, given that very mixed state ρ. So you perform a measurement now, in the hope that you can say more about a random measurement in the future. That is to say, by performing a measurement, you can reduce the mixedness of a state ... and in that sense a measurement can be informative.

Unfortunately, I do not say a lot about 1) in this paper outside of the paragraph you cite below. I do say a lot about 2) though. It sounds like you haven't read through that.

Of course some measurements will be better or worse for each of these tasks. In some examples of 1), you most certainly have cases where NON-vNM POVMs are required. As regards 2), I haven't thought about it enough: i.e. suppose you wanted to changed your mixedness from amount x to amount y. Could you always do that with a sufficiently well-chosen vNM?

Your example of $p(b)I$ is bad for both these tasks. Call it a "measurement" if you will. I keep thinking "intervention" or "act" is better, but I'm not going to change 75 years of ingrained language.

Merminition 26 : *Could it be that the postulate you have dropped is there because it somehow maximizes this figure of merit?*

That would be interesting if it were the case. But I don't know of any ways in which is is true.

Merminition 27 : *Is there some way to measure the ability of povms to discriminate among all the possible density matrices a system might have which shows that pvms are the most sensitive? Or, conversely, is there some natural measure of discriminatory ability for which a povm that is not a pvm does better. (I seem to remember an example in Peres — possibly even in your paper — where a three outcome povm tells you more about something or other than any possible pvm.)*

Systems don't have density operators; people ascribe density operators. But what you have in mind is 1) above.

Actually, there is at least one more interesting way to think about a POVM as being informative of a density operator. (This way is more conceptual than the two above, however.) The space of Hermitian operators is a d^2 dimensional vector space. It turns out to be possible to find POVMs with precisely d^2 linearly independent elements. For such measurements, the probabilities $P(b) = \mathrm{tr}(\rho E_b)$ uniquely specify ρ. You could of course do the same task by (conceptually) measuring $d+1$ standard observables ... like the three Pauli operators in the $d = 2$ case. But counting the total number of outcomes for this case (and lumping the whole thing into a single POVM if you will), you would have $d^2 + d$ outcomes. Thus there are "minimal informationally complete POVMs" if you move outside the von Neumann paradigm. And that too makes POVMs special over von Neumann ones. (You can read about these kinds of POVMs in our quantum de Finetti paper.)

Merminition 28 : *A second question. I'm on page 20, and a student again. My povm has only one outcome (which always occurs, again, independent of the initial density matrix rho). It is just $E_1 = I$. How am I to understand the enormous range of possible final density matrices? This would appear to be a measurement from which I learn nothing. Yet it has an enormous capacity for altering the density matrix, which encapsulates my knowledge. What's going on?*

There are good interventions, and there are bad interventions. For your example $E_1 = I$ there are no ameliorating ways to do it. You can never end up with an increase in the purity of the state describing the system.

Merminition 29 : *Your ground rules forbid you to tell me that the enormous range of outputs have to do with the fact that I've turned on an interaction that has entangled the poor system with an arbitrarily chosen ancilla, so of course, if I've paid attention to what I've done to it, the poor density matrix will change even if I subsequently perform no test on the ancilla. But what do you tell me?*

The word intervention really is better than measurement. (Did you read the "Penrose Tiles" section I sent you to?) Some ways I can act on the world now will help me predict the consequences of my actions in the future. Some ways will not. Quantum theory gives us the full range.

28-06-01 *Context Dependent Probability* (to A. Khrennikov)

Khrennikovism 1 : *It was nice to meet you in Växjö and discuss fundamental problems of quantum theory. Unfortunetely, I have the impression that my presentation on Contextual Probabilistic Interpretation of quantum theory was not so clear for participants (conversations during lunches and dinners). I try to present my views as short and clear as possible.*

Thank you for valuing my opinion on your ideas; I am flattered. So I treated the problem in a conscientious manner: I downloaded and read three of your papers (`quant-ph/0103065`, `0105059`, and `0106073`).

I am indeed quite intrigued by the possibility that quantum mechanics may be nothing more than a calculus for comparing probabilities when the experimental context cannot be deleted from the results it brings about. In vague philosophical terms, I think this is precisely the kind of idea Bohr, Heisenberg, and Pauli were bandying about in constructing their interpretation of quantum mechanics. It is certainly the kind of notion Bohr was trying to get at with his emphasis on "complementarity." So I would welcome a more precise way (a mathematical way) of expressing the essence of all this. I myself have been attracted to this sort of thing for a long time: it is a large part of the thread connecting my "Notes on a Paulian Idea"—that is, that the observer sets the context, and, in the words of Pauli, cannot be "detached" from what he finds. Also you can find discussions of it in Sections 4 and 8 of the large paper I was circulating at the conference, "Quantum Foundations in the Light of Quantum Information." I say all this to make it clear that I am more than sympathetic to your program.

However, as much as I would like to tell you otherwise (because you are my friend), I do not see that your present formulation of the problem moves very far toward quantum mechanics in a convincing way. There are problems on at least two levels.

Maybe the most devastating and immediate is your move between Eqs. (5) and (6) of `quant-ph/0106073`. (I'll focus on that paper for specificity since I did not see you make a stronger argument in either of the other two papers.) You write:

> The perturbation term $\delta(\mathcal{S}, \mathcal{S}')$ depends on absolute magnitudes of probabilities. It would be natural to introduce normalized coefficient of the context transition ...

The question anyone will ask is, "Why is this natural?" What compels the precise form of the normalization other than that it forces the equation to look of a more quantum mechanical form. Why did you choose the square root rather than the third root, say? Indeed, why not divide by the absolute value of δ, or the exponential of δ, or any other combination of functions one could pull out of a hat? To put it not so gently, it looks as if you built the desired answer in at the outset, with little justification otherwise.

The second level of my problem is that, even if you do get this far, how do you make the further step to vector space representations of quantum mechanics? Why are observables POVMs and not other exotic entities? What leads us to the starting point of Gleason's theorem? Etc., etc.? I don't see that you have enough structure to do that. But more importantly, until you have done that I would have to say that your theory remains fairly empty in making a connection to quantum mechanics. Too empty.

The way I view the problem presently is that, indeed, quantum theory is a theory of contextual probabilities. This much we agree on: within each context, quantum probabilities are nothing more than standard Kolmogorovian probabilities. But the contexts are set by the structure of the Positive Operator-Valued Measures: one experimental context, one POVM. The glue that pastes the POVMs together into a unified Hilbert space is Gleason's "noncontextuality assumption": where two POVMs overlap, the probability assignments for those outcomes must not depend upon the context. Putting those two ideas together, one derives the structure of the quantum state. The quantum state (uniquely) specifies a *compendium* of probabilities, one for each context. And thus there are transformation rules for deriving probabilities in one context from another. This has the flavor of your program. But getting to that starting point from more general considerations—as you would like to do (I think)—is the challenge I haven't yet seen fulfilled.

I very much hope that I have not offended you with these comments. I greatly respect your program. But because of that I want much from it. I want it to stretch our understanding. John Wheeler used to say, "We must make as many mistakes as we can, as fast as we can, or we'll never have a hope of gaining a true understanding!" I let that philosophy rule my research life. Thus I can only commend you for your exploration, and hold the strongest hope that something firm will come from it with a little more work and contemplation.

28-06-01 *The Oblique Observer* (to N. D. Mermin)

You haunt me so that I wake up in the middle of the night just to spar with you. It's this damned noncontextuality of quantum probabilities. If one walks into the game with the *firm* belief that a quantum state is a state of knowledge, then noncontextuality is almost a given. That, in part, is what my section "Whither Bayes' Rule?" is about. Moreover, all questions about instantaneous signaling through quantum-state change just become silly: such questions spring solely from a wrong-headed view.

But, you do not walk into this game with the firm belief that a quantum state is a state of knowledge. So I am forced to work to win your approval. In the end, with clarity achieved, your demands will have been a great gift. Right now, they are just annoying.

In any case, this leads me down the path of the oblique observer. There is a way in which a von Neumann-like view of measurement may be a virtue. That is, a view of measurement where, to make it go, I first introduce an ancilla to interact with the system. Then I introduce a second ancilla to interact with the first ancilla because I didn't know how to solve the measurement problem at

the new level. Then I introduce a third ancilla, and so forth. Von Neumann went to this extreme because he was chicken to let the mental update that is a measurement fall outside of physics (as Thomas Bayes would have). So, he piled up superobserver after superobserver, just so there would always be an outside view. But so be it: Let us glean what we can from this freedom of our descriptions.

The main point is this: You pick any measurement (any POVM) you wish for some system, and I can always think of a way to get at that measurement in an indirect way. That is to say, I can always delay my cutting of the Gordian knot until I get to a system with no residual causal link to the one I'm really interested in. In language I don't like, it means I can always induce the "physical collapse" somewhere else. I can always push the measurement to an arena where you would take the noncontextuality assumption (on the system of interest) as a given. My direct measurement on an ancilla serves only to refine my knowledge about the actual system: the actual system cannot care how I came to that refinement, or, indeed, if I ever pursued refining my knowledge at all.

You get my point—with that much repetition, you'd better. This gives rise to a vague idea that perhaps you can help me elicit into reality. (And save me some torture that you are the root of.) Forgetting about the precise structure of quantum mechanics, why should we not view all observations as oblique observations? Whose philosophy ever dreamt that we had *direct* access to the minutest details of the world in the first place? (I'll paste in the great Heisenberg/Einstein quote below so you'll have quick access in case you wish to remind yourself of it—especially the fourth, fifth, and sixth sentences.)

Moreover, coming back to quantum mechanics in particular, what is to keep me (in my derivation of the tensor product rule) from thinking of the two separate observations as each concerning the opposite system? The question is, can one get some quantitative mileage from this. At least that's the question on my mind as the sun is rising. Any thoughts?

It's a good thing you left the Växjö before Friday's lunch. The boiled potatoes were ridden with rocks: I lost an eighth of a molar in the process! What was your overall impression of the meeting (good and bad)?

28-06-01 *The Kettle Black* (to N. D. Mermin)

I'm just reading through a friend's comments on my NATO draft. Just after my equations 65–67, where I write, "The resemblance between the process in Eq. (66) and the classical Bayes' rule of Eq. (38) is unmistakable," he writes: "Seems to me contrived — you want to find a resemblance and then you find it."

Thinking back to one of his recent productions, I think "Boy, that's the pot calling the kettle black!"

But aside from that, I do seem to be impressing less people with this result than I would have thought. Is it my presentation, or is it really the substance? This result, in particular, is the one I'm most pleased with in the paper, but it gets the coldest reception. You've never yet told me your truest thoughts on that section. I'd like to hear them.

02-07-01 *Objective Properties* (to D. G. Chakalov)

Thank you for all the interest you've shown in the papers I have been involved with. I commend you in your efforts to get to the bottom of what's going on in our world. But I cannot believe it very likely that distinct new kinds of *physics* arise in our brain processes. Instead the road I have chosen to develop is making sense of quantum mechanics (as a theory predominantly of inference)

from *within* quantum mechanics. I understand that your road is distinct: but life is short, and one has to make a cut or one will certainly never get anywhere. My own direction may turn out to be completely wrong, but I have decided to pursue it with dogged determination and not to get derailed.

I wish you luck in your own pursuits.

02-07-01 *Making Good Sense* (to J. Finkelstein)

Finkelsteinism 1: *I enjoyed reading your latest "quantum states are states of knowledge" manifesto,* `quant-ph/0106133`*. I do have sympathy for that point of view, but I would like to put my two-cents-worth in by remarking that it is not quite fair to imply that experimental results such as those of Scarini et al which you cite furnish ADDITIONAL support for it. . . .*

It is certainly important to confirm the standard quantum predictions under as wide a set of circumstances as possible, but that confirmation does not distinguish between alternative interpretations all of which agree with the prediction. For example, neither I nor (I believe) you are advocates of the many-worlds interpretation. But some folks are, and those folks would have expected Scarini, Zbinden, Gisin etc to have found exactly the results that they did find. Therefore I would say that the many-worlds interpretation has the same (small) degree of plausibility after these experiments as it did before.

Would you agree?

Thanks for the note! Yes, I guess I would (though only to the small extent that I think many-worlds is coherent in the first place). But there are two things working in the background. 1) Probably plain sloppiness on our part in our wording. And 2) the fact that Rüdiger is (presently) more conciliatory to MWI than Carl and I are. He sees Bayesian probability as holding a place even in their interpretation. (A rough cartoon is: In their interpretation, the universal wave function serves an ontologic role, while the relative states in a Schmidt decomposition with respect to an observer's mind serve the same epistemic role we ascribe to them.) We should probably either remedy 1) or make 2) more clear, or both. We'll have to huddle for that.

Finkelsteinism 2: *(And also by pointing out that the experiment with detectors in relative motion was reported in Zbinden et al (*`quant-ph/0007009`*), rather than in your ref. 2.)*

That's probably my screw-up: I just assumed (from a search through SciSearch) that Ref. 2 was the published version of Zbinden et al. I presume you're telling me it's not. Is there some history here that we should be aware of? Or a different published reference?

Finkelsteinism 3: *I enjoyed reading your latest "quantum states are states of knowledge" manifesto,*

And there's still another one coming: it should have appeared on `quant-ph` today. I hope you'll read it too. This one's a solo flight by me (titled "Quantum Foundations in the Light of Quantum Information"). (BTW: Don't let the sober sounding abstract on `quant-ph` fool you; I'm as loquacious and philosophical as usual on the inside.)

02-07-01 *quant-ph/0106133* (to R. Schack)

I would only temper what you just said by making one addition:

Schackcosm 1: *Actually, I believe that the relative state an observer in some branch of a multiverse* [has no choice but to assign] *to, say, a qubit has a very natural interpretation as a state of knowledge.*

The lack of free choice is important there, and to that extent the whole scheme is non-Bayesian. Bayesian probabilities are never fixed by edict. In a way, this is just a fancy version of David Lewis's principal principle.

Top of the mornin',

04-07-01 *Invitation* (to A. Khrennikov)

Khrennikovism 2: *Yes, this is very well! However, for me, the only bridge between "reality" and our subjective description is given by relative frequencies ...*

But there other ways to make the bridge: this is what gambling situations (like the Dutch-book argument that Schack spoke about) are about. They give a NON-frequency *operational* definition to probabilities. Subjective probabilities make their *objective* mark on the world by specifying how an agent should act when confronted with them.

04-07-01 *Context Dependent Probability, 2* (to A. Khrennikov)

Khrennikovism 3: *P.S. But! How can you unify contextuality with subjective probability?*

I just don't see this as a problem. In choosing one experiment over another, I choose one context over another. The experiment elicits the world to do something. To say that the world is indeterministic means simply that I cannot predict with certainty what it will do in response to my action. Instead, I say what I can in the form of a probability assignment. My probability assignment comes about from the information available to me (how the system reacted in other contexts, etc., etc.). Similarly for you, even though your information may not be the same as mine. The OBJECTIVE content of the probability assignment comes from the fact that NO ONE can make *tighter* predictions for the outcomes of experiments than specified by the quantum mechanical laws. Or to say it still another way, it is the very existence of transformation RULES from one context to another that expresses an objective content for the theory. Those rules apply to me as well as to you, even though our probability assignments WITHIN each context may be completely different (because they are subjective). But, if one of us follows the proper transformation rules—the quantum rules—for going to one context from another, while the other of us does not, then one of us will be able to take advantage of the other in a gambling match. The one of us that ignores the structure of the world will be bitten by it!

05-07-01 *Standing Up and Saying YES* (to J. Finkelstein)

Thanks for the comments. I welcome any that you send me!

Finkelsteinism 4: *This is not really any objection to what you have written, but the story you tell on page 10 might produce even WEAKER knees with the following modification: Suppose that Alice, instead of choosing ANY state $|\psi\rangle$ for her qubit, makes her choice from a finite and previously-agreed-upon set. She broadcasts the result of her measurement, but keeps her choice a secret, except*

that she reveals her choice in a sealed envelope which she sends to Chris (who initially leaves it sealed). Bob performs the appropriate Pauli rotation, then he makes a guess as to which state Alice chose, and performs a yes-no measurement with that guess; he communicates his guess, as well as the yes-no result, to Chris.

Chris can now open the sealed envelope; if it happens that Bob's guess was in fact correct, then the result must have been "yes". So, if one wanted to be contrary (and of course I do not) one might say that, although when the yes-no measurement was performed nobody knew that the guess was correct, and although Alice did not "take the time to ... interact with it", nevertheless the qubit had "the power to stand up and say YES all by itself".

I agree, this does sound even more dramatic. And maybe I will start using it in my presentations. But the point remains the same: it is Bob's action that elicits a consequence.

You can see, I keep dreaming (modern) alchemical thoughts. Below. From: W. Heisenberg, "Wolfgang Pauli's Philosophical Outlook," in his book *Across the Frontiers*, translated by P. Heath, (Harper & Row, New York, 1974), pp. 30–38.

> The elaboration of Plato's thought had led, in neo-Platonism and Christianity, to a position where matter was characterized as void of Ideas. Hence, since the intelligible was identical with the good, matter was identified as evil. But in the new science the world-soul was finally replaced by the abstract mathematical law of nature. Against this one-sidedly spiritualizing tendency the alchemistical philosophy, championed here by Fludd, represents a certain counterpoise. In the alchemistic view "there dwells in matter a spirit awaiting release. The alchemist in his laboratory is constantly involved in nature's course, in such wise that the real or supposed chemical reactions in the retort are mystically identified with the psychic processes in himself, and are called by the same names. The release of the substance by the man who transmutes it, which culminates in the production of the philosopher's stone, is seen by the alchemist, in light of the mystical correspondence of macrocosmos and microcosmos, as identical with the saving transformation of the man by the work, which succeeds only 'Deo concedente.'" The governing symbol for this magical view of nature is the quaternary number, the so-called "tetractys" of the Pythagoreans, which is put together out of two polarities. The division is correlated with the dark side of the world (matter, the Devil), and the magical view of nature also embraces this dark region.

and

> When, in the spring of 1927, opinions on the interpretation of quantum mechanics were taking on rational shape and Bohr was forging the concept of complementarity, Pauli was one of the first physicists to decide unreservedly for the new possibility of interpretation. The characteristic feature of this interpretation—namely, that in every experiment, every incursion into nature, we have the choice of which aspect of nature we want to make visible, but that we simultaneously must sacrifice, in that we must forego other such aspects—this coupling of "choice and sacrifice," proved spontaneously congenial to Pauli's philosophical outlook. In the center of his philosophical thinking here there was always the wish for a unitary understanding of the world, a unity incorporating the tension of opposites, and he hailed the interpretation of quantum theory as a new way of thinking, in which the unity can perhaps be more easily expressed than before. In the alchemistic philosophy, he had been captivated by the attempt to speak of material and psychical processes in the same language. Pauli came to think that in

> the abstract territory traversed by modern atomic physics and modern psychology such a language could once more be attempted ...

05-07-01 *Invitation, 2* (to A. Khrennikov)

Khrennikovism 4: *I think you (and everybody) do in the following way: you have some experience with gambling (frequency!) and use this experience to introduce "subjective" probabilities.*

No, I think it is just the opposite: people almost never use frequency data as the determiners of their information in any common situation. Instead they use symmetry. If someone presents me with a coin that I have never seen before, then after a quick examination, I will likely *ascribe* a 50/50 probability to its coming up heads *simply* because I have no reason to believe otherwise. But if Danny Greenberger is the tosser of it, I know that he has the skill to make it look superficially as if it were being tossed in a haphazard fashion but it will still come up heads every time. The 50/50 ascription is not a property of the coin! It is simply a property of ignorance.

09-07-01 *The O'bleak Observer* (to N. D. Mermin)

Merminition 30: *You should not dismiss my feeling that you've not adequately justified your assumption about noncontextuality as merely a manifestation of a regretable atavistic tendency to reify the quantum state.*

The "oblique observer" note was a concession, not a dismission. It is evidence that I am taking your point very seriously (even though I'd rather being out playing with the other kids).

Merminition 31: *The question you're evading is what it means for one and the same positive operator E to appear in many different POVMS.*

No, I don't think I'm evading it. It means that those various interventions or ways of gathering data—those POVMs—physically diverse though they may be—all lead to at least one common possibility for what my knowledge can be updated to (modulo the unitary readjustment).

09-07-01 *More O'bleakness* (to N. D. Mermin)

Merminition 32:

> CAF Said: Moreover, all questions about instantaneous signaling through quantum-state change just become silly: such questions spring solely from a wrong-headed view.

No! The signalling has nothing to do with quantum-state change. (We've been through this before.) If Bob and Alice share a large number of identically prepared pairs, then a very reasonable requirement is that the statistical distribution of outcomes Bob gets from his members of the pairs cannot depend on what Alice chooses to do to her members. (If it did Alice could send useable unmediated signals to Bob.) Again, this has nothing to do with how you like to think about probabilities or quantum states. I offered this to you as an example of a situation in which you can, in fact, justify the non-contextuality of certain probabilities by appealing to an independent physical requirement (no remote signalling).

I hold firm in my opinion. It has *everything* to do with how you like to think about (quantum) probabilities. If you think the probabilities are subjective expectations for the local consequences of one's experimental interventions, then the question never arises.

However, granting you a little distrust for that, the point about oblique observations is that one might always be able to think of a quantum measurement as being enacted on a system other than the intended one. This would give your point above a natural means for being used to justify noncontextuality for *all* quantum measurements.

Again, I'm starting to feel awfully comfortable with noncontextuality as the very simplest generalization of Bayes' noncontextuality. It is the very glue that puts measurement outcomes into Hilbert space in the first place. (Otherwise we might just draw out an exhaustive list of one-outcomed, two-outcomed, three-outcomed measurements etc., etc., and never even suppose a connection between them.) But I offer the above as an effort to go in the direction you want me to.

10-07-01 *Replies on a Preskillian Meeting* (to A. J. Landahl)

Wow, what a set of notes! Thank you all for the interest in my silly efforts. This is a little unexpected.

Landahlism 1: *The talk was a smash. It went much better than I was expecting, causing much discussion. (As you know, some of the people in our group are rather reticent, so that's really saying something.)*

I am so glad to hear that. It is really very flattering.

Landahlism 2: *At the end of the talk, Sumit decided to go up to the chalkboard and take a poll, the topic and results of which I'll leave as a surprise, as I imagine John will tell you about them himself. (If he doesn't, just e-mail me back and I'll let you know.)*

I presume this is the poll Scott mentioned. I'll say more about that later.

Landahlism 3: *I also mentioned your program to establish an information-theoretic foundation for all the laws of quantum mechanics (and physics?) in this section. This proposal met with much skepticism from the audience. I'm somewhat sympathetic to your cause (certainly more so than some of our denizens!), but I don't believe that all of physics has an information theoretic description. For example, where would the (dimensionful) physical constants enter into this scheme (like Planck's constant and the speed of light)? I don't see how they could enter unless they define what physical dimensions are, which is rather peculiar.*

I really am very flattered by all this attention, but I do get dismayed when I can't seem to get the most important point across to my readers. Even sympathetic readers! Because of this, I have spent months and months trying to clarify and refine my presentation. But for some reason it is amazingly difficult to get the point across. At the very least I need people to understand what I want *before* they declare that they disagree with it. (Disagreeing with it would *then* be fair enough.) The sentences above seem to convey that you haven't gotten to the level of understanding what I want. How can the following sentences be consistent with what you say above?

1. Abstract, penultimate sentence.

   This method holds promise precisely because a large part (but not all) of the structure of quantum theory has always concerned information.

2. Section 1, last paragraph.

   Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as information—subjective information—will we be in a position to make real progress. The raw distillate that is left behind, miniscule though it may be, will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.

3. Section 2, last paragraph.

   The world is sensitive to our touch. ... The whole structure of quantum mechanics—*it is speculated*—may be nothing more than the optimal method of reasoning and processing information in the light of such a fundamental (wonderful) sensitivity.

4. Section 3, penultimate paragraph, page 9.

   The complete disconnectedness of the quantum-state change rule from anything to do with spacetime considerations is telling us something deep: The quantum state is information. Subjective, incomplete information. Put in the right mindset, this is *not* so intolerable. It is a statement about our world. There is something about the world that keeps us from ever getting more information than can be captured through the formal structure of quantum mechanics. Einstein had wanted us to look further—to find out how the incomplete information could be completed—but perhaps the real question is, "Why can it *not* be completed?"

5. Section 5, last two paragraphs.

   Perhaps the structure of the theory denotes the optimal way to reason and make decisions in light of *some* fundamental situation, waiting to be ferreted out in a more satisfactory fashion.

   This much we know: That "fundamental situation"—whatever it is—must be an ingredient Bayesian probability theory does not have. There must be something to drive a wedge between the two theories. Probability theory alone is too general of a structure. Narrowing it will require input from the world about us.

6. Section 7, last two paragraphs.

   The quantum de Finetti theorem shows that the essence of quantum-state tomography is not in revealing an "element of reality" but in deriving that various agents (who agree some minimal amount) can come to agreement in their ultimate quantum-state assignments. This is not the same thing as the stronger statement that "reality does not exist." It is simply that one need not go to the extreme of taking the "unknown quantum state" as being objectively real to make sense of the experimental practice of tomography.

   One is left with the feeling ... that perhaps this is the whole point to quantum mechanics. That is: Perhaps the missing ingredient for narrowing the structure of Bayesian probability down to the structure of quantum mechanics has been in front of us all along. It finds no better expression than in the taking account of the limitations the physical world poses to our ability to come to agreement.

I certainly believe there are some things within quantum mechanics that are beyond our subjective description. As in your example, Planck's constant could well be one of them. The dimensionality d of a Hilbert space is another one I feel fairly confident of. That number characterizes something intrinsic to a system. To that extent, it is not something that can be information-theoretic in origin.

Landahlism 4 : *To press your point more forcefully in the future, you might consider rephrasing the special relativistic axioms themselves in a more information-theoretic light.*

But it doesn't seem to me that special relativity is overtly about Bayesian or information theoretic concerns in the way that quantum mechanics is. So I wouldn't want to express those axioms in a more information-theoretic light.

Landahlism 5 : *Conclusion: quantum information theorists need to get out more!*

Yes! (And I speak for myself too.)

Landahlism 6 : *At least I got a good groan from John Preskill when Alice flipped Bob a quarter for the "two bits" of classical communication she sent him.*

I got a similar groan from Mermin when he first read the end of Section 3. BTW, footnotes 8 and 9 are not typos; several people have asked me about that.

Landahlism 7 : *In The Future section I talked about Gleason's theorem for POVMs and expressed my concerns about Emma's future psychological counseling given that you already are pressing her for a theory of measurement. None of us understood what the quote you ascribed to Hideo actually meant, which I suppose I should ask Hideo about. Do you understand it? It sure sounds amusing.*

The point is, Emma gets by without a theory of measurement, and we should all learn something from that. It is the people who think that knowledge acquisition, or better, belief acquisition, must arise from a detailed dynamical theory that are the problem.

The point of view taken here is that "detailed dynamical theories" are theories of *inference*, and therefore lie outside of the process of knowledge acquisition. This does not bar quantum theory from making contact with the REAL world—the world that was here long before man ever arose (see points about reality below)—it just means that one is not going to find it in the dynamics.

Landahlism 8 : *In the Learning section, I went over your argument for quantum collapse being a kind of Bayesian conditioning. I understand the analogy you drew here, but I hardly believe this makes quantum collapse any more gentle of a process. That's because I don't believe Bayesian conditioning is "gentle." While it's true that one can express the classical process as "plucking" a term out of a sum over conditional probabilities, the change in probabilities can be quite dramatic: the change can be from nearly zero to one in a single step!*

Fair enough. Perhaps I overplayed the imagery. The point I really wanted to emphasize is that quantum collapse can be thought of as predominantly a refinement of one's knowledge.

Landahlism 9 : *I also don't understand the meaning of the "mental readjustment" step in the quantum process. Is this just a change-of-basis for the description of the state? Is it something more or less than this? If that's all it is, then I really don't like this phrase "mental readjustment" at all.*

The track I'm on is that quantum state change is essentially Bayesian updating of knowledge, but with the proviso that the things we have to do to update our knowledge are (generally) not without effect on the world. This must be taken into account in some way. That the updating is Bayesian-like has a trace in quantum mechanics through Eqs. (57) and (58). That we still have to take into account our knowledge of our invasiveness, this has a trace in Eq. (59). That is the

"mental readjustment"—i.e., taking into account what we know about our own invasiveness. (I agree, I should have found a better word for it.) When we know that our knowledge acquisition could not have physically affected the system it was concerned with, then we need do nothing whatsoever beyond Bayesian updating. Eq. (64) is an example of that.

Landahlism 10 : *I finished the Learning section by sketching how "typical von Neumann entropy" as an uncertainty measure increases after every measurement. A question I had in your argument was why the integration is done over only von Neumann measurements. I looked over your original paper on this subject and didn't find an answer there either. Some mention is made of projective measurements being "maximally predictive," but I'm not totally convinced by this argument. I suspect that the true reason for restricting attention to these measurements is technical. A measure over POVMs doesn't exist, so one can't integrate over them. Wouldn't life be so much nicer if there were one! Is this the true reason for the von Neumann measurement restriction?*

You're suspicion is correct. My choice was no deeper than that.

Landahlism 11 : *I spent most of my time discussing the Correlations section. I went in detail over your proof of what I called "Gleason's theorem for Classically Semilocalizable Operations (CSOs)" in deference to the terminology introduced in a recent paper by Beckman et al. Personally, I thought it was cool that the tensor product arose out of noncontextuality and the measurement model. John Preskill wasn't so impressed — he believes that the tensor product will arise out of any reasonable model of measurement which has the property of locality. (I.e where neither Alice's nor Bob's local actions can meaningfully impact the other.) He may be right, but for me that isn't the point. The point for me is that the proper way to view Gleason's theorem is as a machine. The input to the machine is the measurement model and the output of the machine is the state space structure and the probability law.*

Indeed you did get the point. Thanks. The point is, how much of the structure of quantum mechanics can we shove into the simple choice: "measurements = POVMs." How much of quantum mechanics is really independent of that choice? There has been a hell of a lot of work trying to reduce all of quantum mechanics to the assumption of unitarity. I'm trying to go the other way.

Landahlism 12 : *What especially excites me about this point-of-view its potential impact on quantum field theory. The main point of the Beckman et al. paper is that causal measurements and localizable measurements are not one in the same. Wouldn't it be interesting to see what happens when we impose only causality on our measurement model and send it through the "Gleason machine?" What do you suppose the resultant structure of the state space would be?*

If I understand you correctly, Mermin in his talk in Montreal and Sweden has been wondering something very similar. In fact, he would like to see the quantum probability rule AND the tensor product rule arise out of the idea that measurement cannot give instantaneous signaling. He doesn't yet feel comfortable with my (Gleason's) noncontextuality assumption. Yours is a good question; I'll try to have a look at that paper.

Landahlism 13 : *On the whole I portrayed your "party platform" as the statement that "Quantum states are states of knowledge about the consequences of future interventions."*

That statement, as it stands, is true.

Landahlism 14 : *In particular, those consequences aren't consequences to reality, but rather consequences to states of knowledge about even further future interventions.*

That statement, as it stands, is not. (Do you not see the difference?!?!)

Landahlism 15 : *In this worldview Bayesian agents don't work to align their predictions with an underlying reality.*

They would if they could, but they don't because they can't. Realizing this—it seems to me—is the first step to understanding what the quantum world is about.

Landahlism 16 : *Instead they work to align their predictions with each other. It is as if reality in this picture is solely the agreement of predictions!*

I'd be interested to hear if you believe that this is a fair characterization of your party's platform. After reading this paper, I came to the conclusion that you didn't believe in reality at all. (Or at best I thought you believed reality = knowledge.) John Preskill tells me you believe otherwise, namely that there is a reality, which surprised me.

Yeah, you botch it pretty badly there. John is right. See my diatribe under C) above. But, let me also add to that:

1. Section 4, first paragraph.

   I have been watching my two year old daughter learn things at a fantastic rate, and though there have been untold numbers of lessons for her, there have also been a sprinkling for me. For instance, I am just starting to see her come to grips with the idea that there is a world independent of her desires. What strikes me is the contrast between this and the concomitant gain in confidence I see grow in her everyday that there are aspects of existence she actually *can* control. The two go hand in hand. She pushes on the world, and sometimes it gives in a way that she has learned to predict, and sometimes it pushes back in a way she has not foreseen (and may never be able to). If she could manipulate the world to the complete desires of her will, I am quite sure, there would be little difference between wake and dream.

This wispy little piece is the closest I've been able to come to giving substance what I call "the Paulian idea." See my "Notes on a . . . ", page vii. The world must have some impredictability about it, otherwise we would never be able to say we have seen any trustworthy trace of a reality.

Landahlism 17 : *I'm curious to hear what you believe reality is.*

Me too. The idea is not well formed yet. Perhaps this accounts somewhat for people not getting my point that the first part of attempting to identify what is real in the quantum world is to identify what is subjective and governed by "laws of thought." We should do that because that's the easier part of the program. Contemplating what's left behind is when the real fun will begin.

People are too used to seeing gurus (like Deutsch or Mohrhoff) sit on high and declare what reality *is*. My goals are more modest, even if my method of advertisement is not. I don't have an answer yet; I just feel a direction. One should not confuse my method of attack with my answer.

If you were to push me real hard on this "what-you-believe-reality-is" business, I might be inclined to say, "Read Schopenhauer's *The World as Will and Representation*." But since I haven't read it myself, I can hardly expect you to do that! Anyway, as a very *provisional* answer, I might say it's something like the "will" (the quotes around that word are very important) that Schopenhauer attributes to every piece of the world, animate and inanimate alike. For want of a better term, I call it zing.

Landahlism 18 : *As for the mechanical details of the paper itself, I enjoyed your refreshingly casual writing style. I found one of your section titles to be either exceedingly clever or merely a typographical error. Either way, I'm the only one out of a dozen people who noticed it, even after I pointed it out. I'm hoping that you intended the convey the clever interpretation.*

Caves, Bilodeau, and Schumacher also asked me if it is a typo. It is not: you can be relieved.

Landahlism 19 : *If so, I suggest you correct the grammar on the section title to "Wither Entanglement!" to make the homonym less subtle without sacrificing any wit.*

Too late. But in any case, I wanted all the section headings to be questions, except the beginning and end ones.

Landahlism 20 : *The only other typo that jumped out at me was on page 13: "shear difficulty" should read "sheer difficulty."*

Thanks, I hadn't noticed the difference before.

Landahlism 21 : *Once again, great paper. I'm psyched that it stirred up so much discussion in our group meeting.*

Me too!

Now, I said I would make some comments on Sumit's poll. But I'm too tired for that after all this writing. So I won't tell you what I think in any great detail at the present. I'll just cut and paste what Hans Primas thinks. It's below. I will say, however, that I don't see that there should be a qualitative distinction between my description of you (Andrew Landahl) and my description of the coffee maker sitting to my right. You are both physical systems embedded in this thing we call the world.

> From: H. Primas, "Beyond Baconian Quantum Physics," in *Kohti uutta todellisuuskäsitystä. Juhlakirja professori Laurikaisen 75-vuotispäivänä* (Towards a New Conception of Reality. Anniversary Publication to Professor Laurikainen's 75th Birthday), edited by U. Ketvel (Yliopistopaino, Helsinki, 1990), pp. 100–112.

> The methodology of experimental scientific research and engineering science is to a large extent characterized by the regulative principles emphasized by Francis Bacon. It is a tacit assumption of all engineering sciences that nature can be *manipulated* and that the initial conditions required by experiments can be brought about by interventions of the world external to the object under investigation. That is, *we assume that the experimenter has a certain freedom of action which is not accounted for by first principles of physics.* Without this freedom of choice, experiments would be impossible. Man's free will implies the ability to carry out actions, it constitutes his essence as an actor. We act under the idea of freedom, but the topic under discussion is neither man's sense of personal freedom as a subjective experience, nor the question whether this idea could be an illusion or not, nor any questions of moral philosophy, but that *the framework of experimental science requires the freedom of action as a constitutive though tacit presupposition.*

The metaphysics of Baconian science is based on the confidence that only the past is factual, that we are able to change the present state of nature, and that nothing can be known about nature except what can be proved by *experiments*. Francis Bacon's motto *dissecare naturam* led to a preferred way of dividing the world into object and observing systems. An experiment is an *intervention* in nature, it requires artificially produced and deliberately controlled, reproducible conditions. In *experiments* in contradistinction to *observations* – one *prepares* systems in initial states, *controls* some of the variables, and finally *measures* a particular variable. The regulative principles of Baconian science require *power to create initial conditions*, stress *the facticity of the past* and *the probabilistic predictability of the future*, and reject *teleological considerations*.

10-07-01 *Old McBleak's Ale House* (to N. D. Mermin)

Merminition 33 : *I can't believe we're talking past each other on something this basic! [. . .]*

> [CAF wrote:] I hold firm in my opinion. It has *everything* to do with how you like to think about (quantum) probabilities. If you think the probabilities are subjective expectations for the local consequences of one's experimental interventions, then the question never arises.

To say that the question never arises is to say that probabilities can never have any bearing on frequencies of experimental outcomes.

Try it in the language you prefer: if Alice's subjective expectations for the local consequences of her experimental interventions differ, depending on what kind of an experimental intervention Bob chooses to make over in the next county — not, I stress, on what Bob learns from his intervention but just on how (or whether) he decides to intervene — then Alice will be wiped out by any competent Dutch bookie, unless unmediated action at a distance is an objective feature of the world.

We are talking past each other.

But, my wording was careful enough to cover your reply (modulo the confusing parenthesis I put around the word quantum, for which I apologize). If a physical action associated with a POVM—by definition—only affects the system associated with the POVM's Hilbert space, then by definition that is all it affects. Standard quantum mechanics has that feature.

The issue is whether we should question the reasonableness of that. Or, indeed, as you would like, turn the tables and check whether the physical requirement of no-signaling gives rise to the standard probability rule full stop. Your question is a well-posed question, I do not deny that. But, as I view it, its motivation is a throw-back to the days when entanglement was thought to have some connection to the spooky ghosts of nonlocality.

I am torn. 75% of the time, I think your question is a regressive turn to the Popescu-Rohrlich-Aharonov-Shimony-Gisin "passion-at-a-distance" mentality. It seems to me acknowledging that as an interesting paradigm (even one to be ultimately shot down) is a wrong turn. But 25% of the time, I think, "Why not? It is a valid question, so answer it if you can."

Still no sympathy for me? (Probably not.) But, am I at least coherent?

18-07-01 *Horizons* (to J. Bub)

That is awful news about your visual problem and its possible causes. Please do keep me up-to-date on your health. For my own part I will cross my fingers and think of you often.

Don't worry at all about leaving me up in the air concerning a visit to Provence. Because of certain of our own medical issues, I've been lobbying my wife to postpone her European vacation until September or early October anyway. (So, you see, I would have to leave you up in the air right now too.) If it happens, it happens. The main thing is that it sounded like a good opportunity to pound out the similarities and distinctions between our points of view on quantum mechanics without being interrupted every three minutes.

I know I suggested I would write a longer letter soon, but I'm going to wimp out of it again for now. It would concern the main point of distinction I see between us (and also between myself and Pitowsky). Namely, A) that I view a large part of quantum mechanics as merely classical probability theory (which on my view may be an a priori "law of thought") PLUS an extra assumption narrowing down the characteristics of the phenomena to which we happen to be applying it to at the moment, while B) you are more tempted to view quantum mechanics as a *generalization* of classical probability theory (and with it information theory). I know that my view is not fully consistent yet, especially as I have always distrusted mathematical Platonism—which you pointed out to me I am getting oh so close to—but it still feels more right (to me, of course). Ben Schumacher, Rüdiger Schack, and I had a long discussion on this (on a long walk) the day after the round table, and I'd like to record that too. Ben took a stance quite similar to yours, and maybe even Rüdiger did too (despite his overwhelming Bayesianism). So, I may be the lonely guy out on this. And my view may be subject to change.

What I probably really need right now is more conversation than writing. So, I do hope I get to see you in an uninterrupted way soon. (By the way, would it be possible for me to get a copy of the talk you gave in Växjö? Could you copy that and mail it to me?) For now, let me post below parts of two notes I wrote Andrei Khrennikov. They touch on the discussion above, even if they are somewhat out of context here. The second note, in particular, struck me as a clean way of stating my position (in a way that I hadn't explored before). Maybe that'll help to zoom us in on the relevant issues.

There is some good news on the horizon. Gilles and I will be holding another foundations meeting in 2002. (Purely quantum info people, much like the original.) All the details aren't clear yet, but we think we may be able to have desk space for people, it may be for an extended period—maybe a month—with a revolving set of participants, etc., etc. I hope you'll be able to join us (once I get the details to you). The main difficulty is that it may have to be in the fall (after the school semester starts), which will cause participation trouble for those with a teaching load.

18-07-01 *Page 270!!* (to L. Smolin)

I'm excited to hear that you're reading some of my things. Especially since I've come across pages 270-272 of your book (paperback edition)! Indeed, there appears to be a significant overlap between some of our toy ideas. The one I'm speaking about is (in a technical way) the undercurrent of my paper "Quantum Foundations in the Light of Quantum Information." But, you can find broader-view statements of it on pages 156 and 190 of my samizdat. Also, you can see a trace of it in Mermin's foreword, page iii, last paragraph. Do you see the overlap that I do?

Anyway, I find this quite intriguing: Somehow, I had gotten the impression that you were a staunch many-worlder, and that our views of quantum mechanics might be diametrically opposite. I really apologize for my previous misreading (based on reading your book for an hour in a bookstore one day). I will read your book more carefully, and also look at the papers you recommended. [...]

You asked me, "What is the Paulian idea?" I wish I knew! More seriously, the best summaries I can give you are 1) the *conjunction* of two Pauli quotes on page vii of the samizdat, and 2) the

wispy little piece I wrote in the first paragraph of Section 4 of my paper "Quantum Foundations in the Light ...". This is a very deep idea I think, and I don't know that I've ever seen it expressed anywhere except (very sketchily) in Pauli's writings. It is that, in a world where the experimental context cannot be deleted from the consequences it brings about, there must be a kind of randomness or impredictability. Else there would be no way to distinguish between wake and dream for any observer who makes use of such contexts. It is the ultimate impredictability of the consequences of our interactions with the world that gives us firm evidence that there is something beyond us. By this view, the world is not real because it can be mathematized completely, but because it cannot.

21-07-01 *The Reality of Wives* (to A. J. Landahl & J. Preskill)

This morning one of the local hospitals had a fund-raising flea market, and I picked up a copy of Martin Gardner's *The Whys of a Philosophical Scrivener* for $0.50. I haven't been able to put the thing down all day; it's quite good, and the beginning parts are especially relevant to my recent discussion with you.

This evening while sitting outside enjoying the end of the day, I couldn't help but read Kiki a cute little story from it. Gardner writes:

> When I was an undergraduate philosophy student at the University of Chicago I attended a seminar given by Bertrand Russell. Carnap, then a professor at Chicago, went to these sessions and often engaged Russell in spirited debates which I only partly comprehended. On one occasion they got into a tangled argument over whether science should assert, as an ontological thesis, the reality of a world behind the phaneron. [Phaneron was Peirce's term for the world of our experience, the phenomenal world.] Carnap struggled to keep the argument technical, but Russell slyly turned it into a discussion of whether their respective wives (Russell's new wife was knitting and smiling in a back-row seat) existed in some ontologically real sense or should be regarded as mere logical fictions based on regularities in their husbands' phaneron.
>
> The next day I happened to be in the campus post office, where faculty members came to pick up mail. Professor Charles Hartshorne, a whimsical philosopher from whom I was then taking a stimulating course, walked in, recognized me, and stopped to chat.
>
> "Did you attend the Russell seminar yesterday?" he asked. "I was unable to go."
>
> "Yes," I said. "It was exciting. Russell tried to persuade Carnap that his wife existed, but Carnap wouldn't admit it."
>
> Hartshorne laughed. Then, by a quirk of fate, in walked Carnap to get his mail. Hartshorne introduced us (it was the first time I had met Carnap; years later we would collaborate on a book); then, to my profound embarrassment, Hartshorne said: "Mr. Gardner tells me that yesterday Russell tried to convince you your wife existed, but you wouldn't admit it."
>
> Carnap did not smile. He glowered down at me and said, "But that was not the point at all."

I followed that by saying, "You know some of my friends are afraid that I don't believe in reality. So there, you're just a figment of my imagination!" She reacted in shock. "Well, I know that can't be true," she said. "Clearly you'd make some changes!"

22-07-01 *Noncontextual Sundays* (to N. D. Mermin)

I know you're busy, but I'm going to try again. (Don't feel the need to write back until you get some time.) The issue is still noncontextuality in the Gleason-like theorems: Is it a natural assumption or not?

Here was the best answer I gave you before, but now I'm going to try to improve on it.

> **Merminition 34:** *The question you're evading is what it means for one and the same positive operator E to appear in many different POVMS.*
>
> No, I don't think I'm evading it. It means that those various interventions or ways of gathering data—those POVMs—physically diverse though they may be—all lead to at least one common possibility for what my knowledge can be updated to (modulo the unitary readjustment).

The point I'm going to try to make is that not only am I finding noncontextuality a natural assumption, but actually it may be the most *basic* assumption of the whole game. (I.e., it may even be prior to the notion that measurements correspond to POVMs.) The idea is captured above, but—I can see now—it is in too idiosyncratic of a language to convince you easily.

Here's the new shot at it (emphasizing a slightly different aspect than previously).

1) Here's the scenario. Forget about quantum mechanics for the moment. Let me take a system S and imagine acting on it with one of two machines, M and N. For the case of machine M, let us label the possible consequences of that action $\{m_1, m_2, \ldots\}$. For the case of machine N, let us label them $\{n_1, n_2, \ldots\}$.

2) If we are good Bayesians, nothing will stop us from using all the information available to us to ascribe probabilities to the consequences of these two potential actions. Thus we naturally have lying around two probability distributions, $p_M(m_k)$ and $p_N(n_k)$.

That's well and good, but it's hardly a physical theory yet. We need more. So, let us suppose the labels m_k and n_k are at least drawn from the same master set (possibly even a set with further structure, like a vector space or something). But then we must ask, under what conditions should we identify two particular labels m_i and n_j with the same element in the master set?

There's really only one thing lying around to do it with, and that's the probability assignments. If $p_M(m_i) \neq p_N(n_j)$, then surely we would not imagine identifying m_i with n_j. If, on the other hand, $p_M(m_i) = p_N(n_j)$ *regardless* of the initial state of knowledge about S, then we might think there's some warrant for it.

And that's the whole story of noncontextuality. It is nothing more than: The consequences (m_i and n_j) of our disparate actions (M and N) should be labelled the same when we would bet the same on them in all possible circumstances (i.e., regardless of our initial knowledge of S).

By this point of view, noncontextuality is a tautology—it is built in from the start. Asking why we have it is a waste of time. Where we do have a freedom is in asking why we make one particular choice of a master set over another. Why should the m_i's be drawn from the set of "effects" (i.e., the positive operators smaller than the identity on some Hilbert space)? Recall the problem on page 86 of the samizdat. Not all choices of the master set are equally interesting once we've settled on noncontextuality for the probability assignments.

You see, I really never do dismiss anything you say! Now I'm off to buy a new BBQ grill. (My family is tugging on me, and refused to let me try to hone this letter. But I really hope by this point it does make some sense.)

23-07-01 *Law without Law* (to J. Summhammer)

I very much enjoyed reading your letter to Carl Caves titled "promoting the Bayesian view." It was quite thoughtful, and makes me regret not having talked to you more while we were in Sweden.

Please allow me to ask a couple of questions based on what you wrote.

Summhammerism 1: *Here I think that, even if there is absolutely no order in the physical world, it will exhibit statistical order to a rational observer. The existence of rationality is to be taken as outside the physical world. It is a transcendental fact. (The term "law of thought" in your paper circles around the same thing.) Analysis of brain functions and molecules explains nothing, because that analysis is done by means of rationality.*

There have been times in my life when I have been very attracted to ideas like this. In particular, right now might be one of them (though I have a history of going up and going down). My first exposure to the idea came from John Wheeler's writings on what he termed "law without law." In fact presently, I'm putting together a large compendium of quotes and citations titled "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature." At the moment, it consists of 423 annotated citations, taking up 96 pages of print. The manuscript is far from complete, but will eventually be submitted to *Studies in History and Philosophy of Modern Physics*.

I wonder if you have any suggestions for things I should include in it (based on your passage above). If so, please give me as complete of references as possible. If I'm not mistaken, I detect a Kantian tinge in your thought: that's an area I haven't explored too deeply in my compendium.

Summhammerism 2: *p.21: ... the Bayesian interpretation places actualization outside its provenance ...*

I agree. Defenders of interpretations which claim to handle actualization should read selected articles on the mind-body problem, written over the last three thousand years.

Would you mind expanding on this, and also what are some of those selected articles? Can you provide references?

Anyway, it was very nice meeting you for the first time.

23-07-01 *A Nonbayesian Bayesian?* (to C. M. Caves)

I enjoyed reading your dialogue with Summhammer.

I have one question of my own.

Cavesism 1: *I do believe that natural selection only works in a world with at least a statistical order, which leads to the quasi-determinism of the macroscopic world. That we and other creatures are exquisitely attuned to this order, to the point of often finding it where it's not really there, is not surprising. The costs of finding order where there is only chaos must be less than the cost of failing to notice and take advantage of order when it is there.*

What on earth do you mean by this? Similarly when you write, "statistical order is the first element of the Bayesian reality," in Section 7 of your *Resource Material*?

Your phrase "world with statistical order" seems to teeter awfully close to an objectivist notion of probability. And it frightens me, of course, having fully made a conversion now.

23-07-01 *The Principal PrincipleS* (to C. M. Caves)

It dawned on me that I should chide you on your discussion on page 21, starting with "Two further comments on Hamiltonians . . . ." You left out the all-important part about how to connect the two notions of Hamiltonian, the subjective—or effective, as you call it—with the objective. It must be a Cavesian version of the principal principle: When the objective determinant of the time evolution of one's subjective states of knowledge is known, then the subjective determinant of one's subjective states of knowledge should coincide with it.

Another typo btw (I think). Page 19: "If you have maximal information about a quantum system and you want to retain it, you must know the system is Hamiltonian." Don't you mean "system's Hamiltonian"?

24-07-01 *Feynman Quotes* (to C. A. Fuchs)

> If, in some cataclysm, all of scientific knowledge were to be destroyed, and only one sentence passed on to the next generation of creatures, what statement would contain the most information in the fewest words? I believe it is the atomic hypothesis (or the atomic fact) that all things are made of atoms—little particles that move around in perpetual motion, attracting each other when they are a little distance apart, but repelling upon being squeezed into on another.
>
> Everything is made of atoms. That is the key hypothesis.

26-07-01 *BZZ* (to N. D. Mermin)

Renes and I are meeting Plotnitsky for lunch in NYC tomorrow. The agenda is Schopenhauer's "will" and Plotnitsky's "efficacity" (not to be confused with "effervescence").

30-07-01 *Britannica* (to J. M. Renes)

This was the only passage in the *Encyclopedia Britannica* that I could find about Nietzsche that even remotely resembled quantum mechanics. In general, he looks like a lot to wade through for little return.

> Perspectivism is a concept which holds that knowledge is always perspectival, that there are no immaculate perceptions, and that knowledge from no point of view is as incoherent a notion as seeing from no particular vantage point. Perspectivism also denies the possibility of an all-inclusive perspective, which could contain all others and, hence, make reality available as it is in itself. The concept of such an all-inclusive perspective is as incoherent as the concept of seeing an object from every possible vantage point simultaneously.
>
> Nietzsche's perspectivism has sometimes been mistakenly identified with relativism and skepticism. Nonetheless, it raises the question of how one is to understand Nietzsche's own theses, for example, that the dominant values of the common heritage have been underwritten by an ascetic ideal. Is this thesis true absolutely or only from a certain perspective? It may also be asked whether perspectivism can be asserted consistently without self-contradiction, since perspectivism must presumably be true in an

absolute, that is a nonperspectival sense. Concerns such as these have generated much fruitful Nietzsche commentary as well as useful work in the theory of knowledge.

01-08-01 *The Montréal Commune* (to W. K. Wootters)

Gilles and I are once again putting together plans for a quantum information/foundations party in Montréal, though this one may be a little more like a commune than a party. The Montreal Commune. (Probably more officially, "Workshop on the Impact of Quantum Information Theory on Quantum Foundations," or some such thing.) We hope you will join us as a communard. [...]

On another subject, the meeting in Sweden went quite well, but we did miss you. Maybe some of the most interesting discussions centered around Andy Steane's paper "Quantum Computation Only Needs One World" (which Richard Jozsa presented). Doug Bilodeau had the wonderful idea that perhaps some combination of it and your old Ph.D. thesis could give us a deeper insight into where quantum computing derives its power from: Quantum computers are not powerful because they perform so many calculations in parallel (as the many-worlds pundits imagine), but rather because they do so FEW calculations! I.e., Their power derives from not doing anything they don't have to do for the final result. (Much like in your thesis, the photon—which can only express its preparation through a probabilistic law—does better by explicitly NOT carrying around the baggage of a local hidden variable theory.) So, your input at these meetings really would be very valuable.

Let us hear from you as soon as you can.

02-08-01 *The Montréal Commune – Ditto* (to B. Schumacher)

I've marked you down in my spreadsheet, and we'll let you know what's up in a couple of weeks.

Schumacherism 1: *An interesting result that I happened on, based on two propositions: (1) "Information" resides in the relation between systems, and (2) "classical" information is exactly that information which may be copied. So we have two systems, Q and R, in a joint state ρ_{RQ}. Think of R as a "record" of Q. Suppose we require that there exists an operation on R only such that, at the end of the day, there are two systems $R1$ and $R2$ so that $\rho_{R1Q} = \rho_{R2Q} = rho_{RQ}$. (So both $R1$ and $R2$ can have just the same relation with Q that R had.) This is possible if and only if the state is of the form*

$$\rho_{RQ} = \sum_k p_k |k\rangle\langle k| \otimes \rho_k$$

where $|k\rangle$ is an orthonormal set of R-states. The result is a pleasant application of the no-broadcasting theorem.

I like your result (which can't be questioned)!

No subject without an object. No object without a subject. No information without both. I like that—it seems like a good track for ontologizing information, to the extent that it can be. The only thing that scares me is your secret desire to reify the quantum state—namely by translating (1) into a statement about bipartite quantum states (which seem to me to have no other good interpretation than information to begin with)! It takes information to get information off the ground?

07-08-01 *Knowledge, Only Knowledge* (to T.A. Brun, J. Finkelstein & N.D. Mermin)

Below is a note I started composing last Friday—but then had to leave for a long weekend for my wife's birthday—and only finished up today. In the mean time, Todd and Jerry have skirted very close to the point I wanted to make. So, the note is not quite as relevant as it might have been, but maybe some of it is still worth contemplating.

Allow me to start off in a fanciful way (like usual) with a couple of quotes:

> The subjectivist, operationalist viewpoint has led us to the conclusion that, if we aspire to quantitative coherence, individual degrees of belief, expressed as probabilities, are inescapably the starting point for descriptions of uncertainty. There can be no theories without theoreticians; no learning without learners; in general, no science without scientists. It follows that learning processes, whatever their particular concerns and fashions at any given point in time, are necessarily reasoning processes which take place in the minds of individuals. To be sure, the object of attention and interest may well be an assumed external, objective reality: but the actuality of the learning process consists in the evolution of individual, subjective beliefs about that reality. However, it is important to emphasize, as in our earlier discussion in Section 2.8, that the primitive and fundamental notions of *individual* preference and belief will typically provide the starting point for *interpersonal* communication and reporting processes. In what follows, both here, and more particularly in Chapter 5, we shall therefore often be concerned to identify and examine features of the individual learning process which relate to interpersonal issues, such as the conditions under which an approximate consensus of beliefs might occur in a population of individuals. — pp. 165–166, Bernardo and Smith, *Bayesian Theory*

> What is the nature and scope of Bayesian Statistics within this spectrum of activity? Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way." — p. 4, Bernardo and Smith, *Bayesian Theory*

Thanks again to all of you for letting me look in on your interesting emails! I've learned a lot from this exchange. Last night I was up between 1:30 and 5:00 reading them all one more time (and David's original paper too), and thinking much harder than I had before on these issues. So, now I hope to say some things in that regard, but not inane things (as I had done a couple of days ago).

The main thing that started striking me more deeply last night is that now it is very obvious that you *have* an answer, but—much more than ever before—I don't really understand what the

question ought to be. In a sense, I'm only coming across the same troubles (Samizdat, p. 236) I've had ever since David first wrote me on this issue, and it relates to the comment I made after his talk in Växjö (in case he remembers). Let me try to explain.

David, in his paper, quotes Peierls as saying,

Merminition 35 : *In my view the most fundamental statement of quantum mechanics is that the wavefunction, or, more generally the density matrix, represents our knowledge of the system we are trying to describe. . . . [Yet, density matrices] may differ, as the nature and amount of knowledge may differ. People may have observed the system by different methods, with more or less accuracy; they may have seen part of the results of another physicist. However, there are limitations to the extent to which their knowledge may differ.*

And David himself says,

Merminition 36 : *I have the feeling that if quantum mechanics is really about knowledge and only knowledge, then there ought to be further elementary constraints on the possible density matrices describing one and the same physical system that are stronger than the very weak second condition of Peierls, but not as strong as his overly restrictive first condition.*

What is being called for here—perhaps unintentionally—is a way to think about quantum mechanics from the bottom up (as Caves, Schack, and I might like it, in a Bayesian way), rather than from the top down (as Everett, Deutsch, and Bennett might like it). That is, one should view quantum mechanics as a conduit for stitching our individual pictures/thoughts/beliefs into a pastiche we ultimately call "the world." This contrasts with imagining that we have miraculously grasped the ultimate reality (the universal wavefunction, say), and can somehow see our individual points of view as being derived back out of that.

But if this is the case, then I cannot understand Peierls' command, David's quest, or the *answer* all three of you ended up coming up with:

> Two density operators ρ_a and ρ_b can describe the same system if and only if the support of ρ_a has a nontrivial intersection with the support of ρ_b.

This theorem is certainly consistent with a top-down view of the Everett sort—that, to be explained below, is really is what it seems to me to demonstrate—but it is not consistent with the bottom-up view. For, from the bottom-up view, there should not—and more importantly, there cannot—be any constraints whatsoever on what an agent can believe. I have every right to be as wrong-headed as I want to be with respect to you: The density operator belongs to me, not to you, and not to the system. I have every right to say inane things and make inane predictions—I do it all the time. What I should not do, however, if I want to remain rational, is refuse to listen to you when you point out my inanities or refuse to listen to the detector clicks that contradict my previous predictions. From the Bayesian view, it is the process of updating and the general structure of beliefs that is constrained by rationality—i.e., by the physical world, or the Platonic ideal, depending upon your orientation. It is not the actual beliefs themselves.

This point, perhaps more than any other, is why I (and Caves and Schack) should adopt the word "belief"—rather than "knowledge" or "information"—for describing the operational significance of quantum states. (I will try to be more consistent in the future, but that is really an aside as far as this note is concerned.)

Of course, your theorem is a theorem—or at least I can see nothing wrong with it—the issue here is how I, with my little Bayesian mind, can put it into a context I am more happy with. At

first, I was the most pleased with Jerry's way of motivating it: From that point of view, what the theorem seems to express is simply the conditions under which a third agent Carol can consistently incorporate Alice and Bob's disparate beliefs into her own belief system.

But from the Bayesian view, why should we care about a Carol at all? What if there's no Carol to be found? What if neither Alice nor Bob ever intend to share their thoughts about this poor physical system with anyone else? To say that there is always a Carol about, or that there ought to be one, is to come dangerously close to endorsing the Everettian (or, for that matter, Bishop Berkeleyian) program. This, of course, may not bother Jerry or Todd—I'm not completely sure about their foundational dispositions—but it does bother me, and I suspect it might bother David, with his newfound deconstructionist tendencies.

Thus, it now seems to me that Todd's original way of posing the issue may be the safer way after all. BUT that is not because it gives us *the* answer, but instead *an* answer. (I.e., there should *only* be sufficient conditions, rather than necessary ones.) Let me let Bernardo and Smith speak again (and again):

> [T]here is an interesting sense, even from our standpoint, in which the parametric model and the prior can be seen as having different roles. Instead of viewing these roles as corresponding to an objective/subjective dichotomy, we view them in terms of an intersubjective/subjective dichotomy. To this end, consider a *group* of Bayesians, all concerned with their belief distributions for the same sequence of observables. In the absence of any general agreement over assumptions of symmetry, invariance or sufficiency, the individuals are each simply left with their own subjective assessments. However, given some set of common assumptions, the results of this chapter imply that the entire group will structure their beliefs using some common form of mixture representation. Within the mixture, the parametric forms adopted will be the same (the *intersubjective* component), while the priors for the parameter will differ from individual to individual (the *subjective* component). Such intersubjective agreement clearly facilitates communication within the group and reduces areas of potential disagreement to just that of different prior judgements for the parameter. As we shall see in Chapter 5, judgements about the parameter will tend more towards a consensus as more data are acquired, so that such a group of Bayesians may eventually come to share very similar beliefs, even if their initial judgements about the parameter were markedly different. We emphasize again, however, that the key element here is intersubjective agreement or consensus. We can find no real role for the idea of objectivity except, perhaps, as a possibly convenient, but potentially misleading, "shorthand" for intersubjective communality of beliefs. — pp. 236–237, Bernardo and Smith, *Bayesian Theory*

> In the approach we have adopted, the fundamental notion of a model is that of a predictive probability specification of observables. However, the forms of representation theorems we have been discussing provide, in typical cases, a basis for separating out, if required, two components; the parametric model, and the belief model for the parameters. Indeed, we have drawn attention in Section 4.8.2 to the fact that shared structural belief assumptions among a group of individuals can imply the adoption of a common form of parametric model, while allowing the belief models for the parameters to vary from individual to individual. One might go further and argue that without some element of agreement of this kind there would be great difficulty in obtaining any meaningful form of scientific discussion or possible consensus. — p. 237, Bernardo and Smith, *Bayesian Theory*

You'll find something similar to this theme infused throughout my paper `quant-ph/0106166`, "Quantum Foundations in the Light of Quantum Information." In the present context the question is, under what conditions (on the density operators themselves), can Alice and Bob move toward consensus in their future density operator assignments for a system? You guys have answered this for the *case* when there are some extra systems beside the one of interest (all noninteracting) lying around for Alice and Bob to make measurements on. If there is no overlap between the initial supports, then no measurement-at-a-distance, which can only have the effect of refining a density operator, can get Alice and Bob any closer to agreement.

What about necessity? As I've tried to say, there can be no requirement of necessity from the Bayesian view I'd like to see prevail. Instead the question is *always* to identify those situations where agents (with disparate beliefs) can move toward consensus be it by indirect measurements, direct ones, or even by communication with further members of the community.

A good and indicative example comes from the quantum de Finetti theorem along with the points made by `quant-ph/0008113` (Schack, Brun, and Caves). From it, we have a natural case where two agents can start out with distinct density operator assignments on a large collection of systems, but through updating via some commonly viewed measurement outcomes, they can move toward complete agreement in their estimates of the outcomes of all future measurements. The only thing the agents need to walk into the room with is this much INITIAL AGREEMENT: (1) that the systems in the collection are exchangeable, and (2) that the parametric form given by the de Finetti theorem has full support (in the sense defined there). Without that initial agreement, the techniques of quantum-state tomography would lead to no final agreement at all. No one can require that two observers in Mike Raymer's lab must walk into it with such an initial agreement, but if they happen to, there will be a reward at the end of the day.

That's my spiel. How would I modify David's quest in light of all that I've just said? Here's my shot:

> I have the feeling that if quantum mechanics is really about knowledge and only knowledge—or better, belief convergence and *only* belief convergence—then FOR ANY GIVEN METHOD OF GATHERING INFORMATION, there *should* be a way to ferret out of quantum mechanics the necessary and sufficient conditions on two observers' initial state assignments, so that the gathered information leaves them in a better agreement than they started out with.

Let me give you an example of where such an exercise can go. This is a question I posed to Rüdiger while we were in Växjö, but also it is a generalization of Todd's considerations (now letting Alice and Bob's measurements being disturbing ones).

Let us agree on a distance measure on density operators. A convenient one (and my favorite) is

$$d(\rho,\sigma) = \mathrm{tr}|\rho-\sigma|,$$

but that's not so important for the considerations here. (Well, it may be in the long-run, but I don't want to let such a technicality detract from posing the question.)

Suppose Alice walks into a room and says to herself that a system is described by ρ, while Bob says to himself that it is described by σ. We can gauge their amount of initial consensus by $d(\rho,\sigma)$. Now, suppose Carol (or Alice or Bob for that matter) performs a measurement on the given system whose action—i.e., whose associated completely positive map—is to take any initial state τ to

$$\tau \longrightarrow \tau_b = \frac{1}{\mathrm{tr}(\tau A_b^\dagger A_b)} A_b \tau A_b^\dagger$$

depending upon the particular outcome b. Here, of course,

$$\sum_b A_b^\dagger A_b = I.$$

(This is nothing more than the general form of an "efficient" measurement as defined in my QFILQI paper cited above.) Let us allow Alice and Bob to be privy to this map, and indeed to the actual outcome b.

Thus, the final consensus of Alice and Bob will be gauged by $d(\rho_b, \sigma_b)$. The question is: When can Alice and Bob expect to be in better agreement after the measurement than before? That is, as far as Alice is concerned, she will *expect* their final distance to be

$$D_A = \sum \mathrm{tr}(\rho A_b^\dagger A_b) d(\rho_b, \sigma_b).$$

As far as Bob is concerned, he will *expect* their final distance to be

$$D_B = \sum \mathrm{tr}(\sigma A_b^\dagger A_b) d(\rho_b, \sigma_b).$$

For a given set of A_b's, what are the necessary and sufficient conditions on ρ and σ so that

$$D_A \leq d(\rho, \sigma)$$

and

$$D_B \leq d(\rho, \sigma)\ ?$$

Suppose we can answer these questions. Then we will be able to identify the minimal fact Alice and Bob must reveal to each other (even without explicitly revealing their full beliefs captured by ρ and σ) so that they can expect to walk out of the room in better agreement ... even if they still can't say with certainty what each other now believes.

Beliefs, only beliefs. But sometimes we can say something about their convergence. And you guys have provided an example.

07-08-01 *Amendment* (to T. A. Brun, J. Finkelstein & N. D. Mermin)

I just reread the thing after sending it. Let me be more careful before one of you accuses me of being a flaming positivist again. (I suspect Todd, in particular, will be inclined to do so, and I want to fend that off before it happens.) In my closing sentence, I wrote:

> Beliefs, only beliefs. But sometimes we can say something about their convergence. And you guys have provided an example.

Let me temper that to:

> Beliefs, only beliefs. But sometimes we can say something about their convergence, as they are steered by our interactions with the world external to us. And you guys have provided an example.

Now, I can sleep safely ...
Good wishes to all!

07-08-01 *The First Amendment* (to J. Finkelstein)

Thanks for the note. But nothing changes for me (yet, at least). Sorry for my overemphasis on Carol.

Finkelsteinism 5: *If that can be agreed to, then the rest is essentially just algebra. $S[\rho_a]$ is the orthogonal complement of the zero-eigensubspace of ρ_a, which is the set of $|\phi\rangle$ such that Alice knows with certainty that a measurement of $|\phi\rangle\langle\phi|$ must yield the result zero. So the support of the updated version of ρ_a must be a subset of the support of the original ρ_a. Likewise, Alice can see that the support of the updated ρ must be a subset of the support of ρ_b. Etc.*

The issue for me is not whether Alice *might* be willing to incorporate Bob's beliefs/knowledge into her own knowledge base—that, I'll grant her, in which case I agree that everything you say is true. Instead it is whether she *must* be willing to do it. I think you are (tacitly) trying to get me to agree that she *must* be willing to accept Bob's quantum state as extra, valid information. That's something I can't do. I reserve the right for Alice to think that Bob's quantum state is complete nonsense, something that she would never want to incorporate into her own knowledge base. (It's part of being an American.)

To say that two observers *must* be willing to incorporate their separate states of knowledge into a single state is—I think—to tacitly (there's that word again) accept an Everettian kind of view. For it never allows that quantum states are states of belief in the normal sense. Instead it makes them more like "objective points of view" (relative states) that *must* be derivable from a larger, more encompassing picture. Why else would the states have to be "consistent" with each other (for instance, in your and Todd's sense)?

Is that helping any to get my wacky point of view across?

07-08-01 *Kiki, James, and Dewey* (to J. A. Waskan)

The last month or so, I've been logging quite a few hours in the philosophical world. I read about 100 pages from this little book: A. Schopenhauer, *The Philosophy of Schopenhauer*, edited, with an introduction by I. Edman, (Modern Library, New York, 1928).

It's a small collection of pieces from Schopenhauer's big masterwork. I read about 50 pages on the world as representation, and about 50 on the world as will. Even that little was not easy for me. It's fanciful stuff, but maybe I extracted an idea or two that I like. The main one is simply the idea of a dichotomy between what things look like from the inside of any phenomenon (when there is a view from that perspective)—Schopenhauer called it will—and what things look like from the outside of the phenomenon (when there is a view from that perspective)—Schopenhauer called it idea. Or, maybe I should have more safely said "is" rather than "look like." But, in any case, that distinction (and a strict separation between the concepts) strikes me as useful or at least worth contemplating. Just about all the rest, though, I probably wouldn't be able to accept: the strict Kantian categories, the principle of sufficient reason, etc., etc.

On the other hand, I have gotten *completely* carried away with William James and John Dewey. Here's how I put it to my friend Carl Caves the other day:

> Today I focused on rounding up some more William James, John Dewey, Percy Bridgman material. I think James is taking me over like a new lover. I had read a little bit of him before, but I think I was more impressed with his writing style than anything. But I was drawn back to him by accident, after reading Martin Gardner's

Whys of a Philosophical Scrivener. Gardner devoted a lot of time knocking down James' theory of truth, because it is just so much easier to accept an underlying reality that signifies whether a proposition is true or false, rather than saying that the knowing agent is involved in eliciting the very proposition itself (along with its truth value). And something clicked! I could see that what James was talking about might as well have been a debate about quantum mechanics. He was saying everything in just the right way. (Let me translate that: he was saying things in a way similar to the way I did in my NATO "appassionato.") And things have only gotten better since.

And indeed, they have only gotten better since! Since coming to Munich, I have not been able to put James and Dewey down (when I'm not writing emails trying to translate their ideas into the quantum mechanical context, in particular for a technical problem Caves, Schack and I are disagreeing violently on). I read James' *Pragmatism, and four essays from the Meaning of Truth*, and now I'm about halfway through *John Dewey: The Essential Writings*. I'm moved by this stuff like nothing else I've ever read.

You can't tell me philosophers don't have the good life!

08-08-01 *Cross Entropy Min* (to J. Finkelstein)

Finkelsteinism 6: *Which brings up a slightly different question. Suppose, again, that Alice describes a system by ρ_a, but this time let's say for simplicity that she considers what would happen if Bob were to tell her that his ρ_b corresponded to a pure state $|\phi\rangle$; then, given her knowledge of the system (ie, given ρ_a) what could Alice say about what $|\phi\rangle$ might be?*

It follows from what we have been saying that Alice knows that for $|\phi\rangle$ to be possible, it must be in $S[\rho_a]$, and that (in finite dimensions, at least) she cannot, based on her own knowledge, rule out any $|\phi\rangle$ that is in $S[\rho_a]$. But can Alice say any more than that? Would it make sense for Alice to put a probability distribution on the possible $|\phi\rangle$ that Bob might announce to her? (It would have to be a probability density; eg, if ρ_a were a multiple of the identity, then Alice would surely judge all states to be equally-likely.) If that did make sense, it would mean that Alice would be constructing a particular (continuous) ensemble representation of ρ_a. What could that be?

As I recall, this is quite similar to the classical problem ("principle of minimum cross-entropy") explored in the two references below. But I'm not going to have a chance to refresh my memory for a while. If you've got the time, you might see if it's relevant.

1. John E. Shore and Rodney W. Johnson, "Axiomatic Derivation of the Principle of Maximum Entropy and the Principle of Minimum Cross-Entropy," IEEE Transactions on Information Theory **IT-26**(1), 26–37 (1980).

2. John E. Shore and Rodney W. Johnson, "Properties of Cross-Entropy Minimization," IEEE Transactions on Information Theory **IT-27**(4), 472–482 (1981).

08-08-01 *The First Eye* (to C. M. Caves)

I am just about to get down to some serious (political) scheming to do with our Bayesian program: I'll let you know what I'm talking about if it turns out to be successful. First though, I want to take a moment to tell you about a point of similarity between our (both far-from-completely worked out) flavors of quantum ontology. This one just struck me a few days ago.

Some time ago, I tried to explain to you what I was hoping for for [sic] an ontology behind quantum mechanics. (See Samizdat, pp. 127–129.) I said it would have something to do with the information-disturbance tradeoff in quantum eavesdropping. You replied that, try as you might, you could see no ontological content in such a statement. I think what was troubling you was that the information-disturbance relations (as I am thinking of them), by their very nature, require explicit reference to the *subjective* points-of-view/opinions/beliefs (i.e., quantum states) of various *agents*.

I, on the other hand, have no problem with that. For the way to think about it is that the world (independent of our existence) has latent within it a property that simply has no way of being properly expressed without inserting information-manipulating agents into the picture. (Or, at the very least, that this anthropocentric way of stating things may be our first firm handle for getting at a better, more objective-sounding, formulation of the latent property.) Taking this tack does not mean, of course, that there is no world independent of human existence, and that is my point. It just means that we may sometimes have to take into account our (presumably contingent) existence for expressing some of the world's properties.

Here is something, however, that you should think about in connection to your "world = Hamiltonian" hopes. Let me put the ball back into your court. Can you explain to me the role of Hamiltonians in your ontology in a way that does not make use—even tacitly—of the concept of a (subjective) quantum state? What is it that Hamiltonians do if their primary role is not in evolving (subjective, agent-required) quantum states? In the classical world, one could give an answer to this question by saying, "They evolve the positions of phase-space points." Such a statement makes no use of the concept of information-bearing agents for its formulation. (I view it as just nitpicking to argue whether the points or their trajectories (from which we can derive the Hamiltonian) are the more primary of the entities. Or whether they are equally primary.) But in the quantum case, I haven't yet seen what you can say if I take away the linguistic tool of "the quantum state" from your explanatory repertoire. What can you say?

I'll put a few passages of Schopenhauer below to inspire you. [From: A. Schopenhauer, *The Philosophy of Schopenhauer*, edited, with an introduction by I. Edman, (Modern Library, New York, 1928).]

> "No object without a subject," is the principle which renders all materialism for ever impossible. Suns and planets without an eye that sees them, and an understanding that knows them, may indeed be spoken of in words, but for the idea, these words are absolutely meaningless. On the other hand, the law of causality and the treatment and investigation of nature which is based upon it, lead us necessarily to the conclusion that, in time, each more highly organised state of matter has succeeded a cruder state: so that the lower animals existed before men, fishes before land animals, plants before fishes, and the unorganised before all that is organised; that, consequently, the original mass had to pass through a long series of changes before the first eye could be opened. And yet, the existence of this whole world remains ever dependent upon the first eye that opened, even if it were that of an insect. For such an eye is necessary condition of the possibility of knowledge, and the whole world exists only in and for knowledge, and without it is not even thinkable. The world is entirely idea, and as such demands the knowing subject as the supporter of its existence. This long course of time itself, filled with innumerable changes, through which matter rose from form to form till at last the first percipient creature appeared—this whole time itself is only thinkable in the identity of a consciousness whose succession of ideas, whose form of knowing it is, and apart from which, it loses all meaning and is nothing at all. Thus we see, on the one

hand, the existence of the whole world necessarily dependent upon the first conscious being, however undeveloped it may be; on the other hand, this conscious being just as necessarily entirely dependent upon a long chain of causes and effects which have preceded it, and in which it itself appears as a small link. These two contradictory points of view, to each of which we are led with the same necessity, we might again call an *antinomy* in our faculty of knowledge, and set it up as the counterpart of that which we found in the first extreme of natural science. The objective world, the world as idea, is not the only side of the world, but merely its outward side; and it has an entirely different side—the side of its inmost nature—its kernel—the thing-in-itself. This we shall consider in the second book, calling it after the most immediate of its objective manifestations—will. But the world as idea, with which alone we are here concerned, only appears with the opening of the first eye. Without this medium of knowledge it cannot be, and therefore it was not before it. But without that eye, that is to say, outside of knowledge, there was also no before, no time. Thus time has no beginning, but all beginning is in time. Since, however, it is the most universal form of the knowable, in which all phenomena are united together through causality, time, with its infinity of past and future, is present in the beginning of knowledge. The phenomenon which fills the first present must at once be known as causally bound up with and dependent upon a sequence of phenomena which stretches infinitely into the past, and this past itself is just as truly conditioned by this first present, as conversely the present is by the past. Accordingly the past out of which the first present arises, is, like it, dependent upon the knowing subject, without which it is nothing. It necessarily happens, however, that this first present does not manifest itself as the first, that is, as having no past for its parent, but as being the beginning of time. It manifests itself rather as the consequence of the past, according to the principle of existence in time. In the same way, the phenomena which fill this first present appear as the effects of earlier phenomena which filled the past, in accordance with the law of causality. Those who like mythological interpretations may take the birth of Kronos ($\chi\varrho o\nu o\varsigma$), the youngest of the Titans, as a symbol of the moment here referred to at which time appears, though indeed it has no beginning; for with him, since he ate his father, the crude productions of heaven and earth cease, and the races of gods and men appear upon the scene.

10-08-01 *The Fifth Amendment* (to T. A. Brun)

Thanks for the note, which I thoroughly enjoyed. You hit a lot of nails on the head with it. Let me try to expand on some of the points that—I believe—show that at least you and I are coming to a little consensus. (As for Jerry and David, I will put them in a superposition for the time being, and see how this this interaction shakes things up.)

Brunism 1: *That doesn't mean that I completely disagree with you, Chris, but I think you are making a point which is pretty far from the spirit of this problem.*

This is the point. What I am trying to get straight is: *What* is the spirit of this problem? You write,

Brunism 2: *We have been describing a consistency criterion.* If *one wishes to combine two state descriptions of a single system into a* single *state description, the criterion tells one* when *it is consistent to do so (i.e., when the two descriptions are not actually contradictory).*

I agree that nobody is holding a gun to Alice's head and forcing her to incorporate Bob's information.

Putting it like that, I can certainly accept the proposition. I want to emphasize that. The attractive feature for me is that it is built on a conditional at the outset.

But you speak of *the* spirit of *this* problem. How is your statement to be reconciled with the tone of David's `quant-ph`? In particular, say, David's quote of Peierls (which he takes as his guiding light):

Merminition 37: *In my view the most fundamental statement of quantum mechanics is that the wavefunction, or, more generally the density matrix, represents our knowledge of the system we are trying to describe. ... [Yet, density matrices] may differ, as the nature and amount of knowledge may differ. People may have observed the system by different methods, with more or less accuracy; they may have seen part of the results of another physicist. However, there are limitations to the extent to which their knowledge may differ.*

Peierls does not use the qualification that you did—nor do any of you three in many of the emails I have seen—and that is what bothers me.

What I see in Peierls version of the spirit is that two quantum states cannot co-exist (even in a Platonic sense) unless they are consistent (in one manner or another, yet to be fleshed out). It is as if the universe has these little properties floating about, called quantum states, that MUST be consistent in the BFM sense (or some other sense). I will agree that that might be fine from an Everett-kind of point of view. But if one insists on consistency (as one should with ontological, physical properties), then—it seems to me—one breaks away from the desire to give the quantum state a *purely* epistemological role ... which, as I understood it, was the goal of Peierls and Mermin (though I am willing to accept that it may not be the goal for you and Jerry).

Brunism 3: *The point of consistency is to determine* if *two points of view can be combined into a single description, not to require that they must be.*

This I also agree with: It was meant to be the whole point of my note "Knowledge, Only Knowledge." My only point of disagreement, was that it was seeming to me that the tendency in David (and Jerry?) was the assumption that the "if" must be satisfied—in some sense—in the "real" physical world.

I just don't know how to say this more clearly. I think it is a valid worry about the intent, the very definition of what the problem is about. I don't think I'm being subtle: that is certainly the last thing I want to be.

Brunism 4: *I will make one additional comment. I think that in science there is usually a* tacit *(to use your word) assumption that the separate states of knowledge of different observers can be combined, provided that they make no errors and reason logically. This then implies that there* is *a kind of "global state," in the limited sense of a state including all available knowledge. If two observers' beliefs are so inconsistent that they cannot be combined together with any amount of communication and experimental data, they might as well be living in different worlds. This is why we say that insane people are "out of touch with reality."*

I can see why one would say that, especially if one believes that the process of science has an end, and that there is a sense in which the universe is pre-formed. (So maybe I will admit to *some* postmodernist tendencies.) But then ...

Brunism 5 : *Also for this reason, a rational person may very well never assign a perfectly pure state to a system, but always a mixed state of the form*

$$\rho = (1-\epsilon)|\psi\rangle\langle\psi| + \epsilon\rho'$$

where ϵ contains the unspoken acknowledgement "But I might be wrong."

giving an agent the right to set his own density operator seems to me to be a concession to the quantum state's (purely?) epistemological content.

A note on the "The Second Amendment" to be sent to David soon after lunch.

12-08-01 *A Silver Lining in the Deutsch Cloud* (to R. Pike)

I like to think that there are silver linings to the clouds of my many diatribes! (E.g. the note I just sent Patrick.) Anyway, thinking more about Deutsch this morning (and his silliness that quantum computing only makes sense from a many-worlds view), see the two papers below.

Brooding on the Raussendorf-Briegel one especially (since David DiVincenzo brought it to Steven's and my attention Thursday), I think these are really important ones. And they may give us a much easier, nicer, more insightful way to describe what quantum computing is about.

`quant-ph/0010033`
Title: Quantum computing via measurements only
Authors: Robert Raussendorf, Hans J. Briegel

`quant-ph/0108020`
Title: Universal quantum computation using only projective measurement, quantum memory, and preparation of the 0 state
Authors: Michael A. Nielsen

13-08-01 *A Silver Lining in the Deutsch Cloud, 2* (to N. D. Mermin)

By the way, you might enjoy these too if you haven't already run across them. I think there is some parallel here to thinking of quantum teleportation in the two ways: i.e., the original 1993 way, and the quantum circuit way (like you wrote about recently). The dichotomy goes much deeper and may pervade all of quantum computation, or least that's what is starting to strike me.

"Pinging the sensitive substrate, that's what quantum computing is about." Or, at least, that's what I think would build a pretty picture. The two papers below may give us the right language to view it that way.

15-08-01 *Knowledge, Only Knowledge – Reprise* (to T. A. Brun)

Brunism 6 : *Frankly, I find your negativity about this problem puzzling.*

On the contrary, I'm quite taken with it: otherwise, I wouldn't have given it the time of day. Reading the collective emails has been a great learning experience for me.

15-08-01 *Compatible States* (to R. Schack)

Schackcosm 2 : *I got a number of rather incoherent messages on compatible states from you. Is there anything more recent on this?*

Yes. About three thousand more notes. The issue refers to David Mermin's recent `quant-ph` paper "Whose Knowledge?"

Where it stands right now is with Brun, Finkelstein, and Mermin all ganged up against me (as being the unreasonable one). The conclusion they've come to is something like this:

> Two density operators ρ_a and ρ_b can describe the same system iff the support of ρ_a has a non-trivial intersection with the support of ρ_b.

I, on the other hand, think such a statement is far too dictatorial for a Bayesian's taste. Try as I might, they just don't think my points are relevant.

I'll paste (what I deem to be) my most lucid notes below. You can judge for yourself. I'm soon going to drop out of the debate, I believe: it's now at a point of diminishing returns. But, I would like to hear your thoughts!

16-08-01 *Subject-Object* (to P. Grangier)

Thanks for your note! I'm always amazed when anyone reads or skims my papers: You have a friend for life!

Last night I read your paper `quant-ph/0012122`, which I had never seen before. Thanks for bringing it to my attention.

Grangierisme 1 : *You are probably aware that statements such as : "The quantum state is information. Subjective, incomplete information." ... "Quantum states are states of knowledge, not states of nature. That statement is the cornerstone of this paper." are unwarranted, since just opposite statements can be made without changing any physical predictions or even any technical development. My personal view is that these statements are even wrong, as soon as "quantum state" is understood as "pure quantum state" (see eg* `quant-ph/0012122`*, missing in your list on p.1).*

In your words,

> [C]ontrary to the copenhagian dogma, a central point in our approach will be to give an "objective reality" to the quantum state of a physical system, in a sense which is developed below. ... The quantum state of a physical system is defined by the values of a set of physical quantities, which can be predicted with certainty and measured repeatedly without perturbing in any way the system.

Here is a problem I have with this conception. (It is a problem I am quite sure you are aware of, but for some reason you did not address it directly in your paper.) Consider two electrons originally prepared in a spin-singlet state—one electron in the possession of Alice, one in the possession of Bob. Let us imagine now two alternative scenarios. In one, Alice measures σ_x on her particle; in the other scenario, she measures σ_z. By your criterion, Bob's particle does not start out with a quantum state (since the two electrons are in an entangled state)—which is fair enough (I have no qualm with that)—but *immediately upon* the measurement it will go into a definite quantum state, either an eigenstate of σ_x or an eigenstate of σ_z, depending upon the scenario. We know this because if Bob were to thereafter repeatedly send his particle through a Stern-Gerlach device

with the proper orientation (for the given of the two scenarios), Alice would be able predict with complete certainty which way Bob's particle would go. Moreover, if Bob is careful enough, these further measurements on his part will not perturb his system (in the sense of changing the spin quantum state of the electron). So, we have just what you had wanted: complete certainty and no necessary perturbation.

But if the quantum state is an objective feature of the electron, then we see that it can be toggled one way or the other instantaneously from a distance. (Alice's measurements causes Bob's electron to go into one or another quantum state instantaneously.) Thus, if you accept the objectivity of the quantum state, then you must also accept the objectivity of instantaneous action at a distance (that in no way diminishes with distance or the particulars of the medium filling the space between Alice and Bob).

This is something I'm not willing to do. Not out of dogma, but because it strikes me that the world would have to be horribly contrived to have this property: a little private (instantaneous) telephone line between each and every physical system that will not accept outside calls. I.e., I can never make use of this instantaneous action for real, live communication even though it really, really, really is there. It stretches my imagination too far.

You, of course, may accept it as you wish; but the reasons above are mine for not doing so. Let me address your other point that I quoted above:

> [Your statements] are unwarranted, since just opposite statements can be made without changing any physical predictions or even any technical development.

I certainly disagree with the latter part of this sentence: that was the whole point of my paper (i.e., that it can change the technical development of the theory). Taking one or another point of view about the objectivity of the quantum state motivates different directions of theoretical exploration. In my case, it motivated trying to find the four theorems I presented in that paper. One who believed in the objectivity of the quantum state—I am quite sure—would not have sought those theorems in the first place.

Grangierisme 2: *By the way I also disagree with your point of view that "Quantum Theory Needs No 'Interpretation'," Phys. Today* **53***(3), 70 (2000). The fact that a physical theory ALWAYS needs an interpretation is in my opinion a central difference between physics and mathematics.*

You won't find a disagreement with me here. The title and closing sentence of that paper were meant to be tongue-in-cheek plays on something Rudolf Peierls once said: "The Copenhagen interpretation *is* quantum mechanics." The whole paper is very definitely about an interpretation, and why one does not need to go any further than it to make sense of quantum mechanics as it stands. My paper `quant-ph/0106166` and the large (more personal) collection `quant-ph/0105039` is about going the next step, i.e., what to do once we have established the belief that quantum states are states of knowledge.

When we do finally dig up an ontology underneath quantum mechanics, I'm quite sure it will be an interesting one!

17-08-01 *Unloading Bayes* (to C. M. Caves & R. Schack)

Let me unload a couple more Bayesian thoughts on you—i.e., some things we will probably want to address in the RMP article.

1) Attached below is a note I wrote to Mermin giving what I think is the cleanest justification for noncontextuality in any Gleason theorem. In fact, it shows that noncontextuality is more basic

in the hierarchy of theories than anything else we've dealt with yet. I.e., it comes far before the particular details of quantum mechanics. (Maybe this is what Carl has been saying all along, but I had to work through it for myself before it stuck.)

2) Let me bring your attention to a cluster of papers that I think are really important.

> `quant-ph/0010033`:
> Title: Quantum computing via measurements only
> Authors: Robert Raussendorf, Hans J. Briegel (LMU Munich)
>
> `quant-ph/0108020`:
> Title: Universal quantum computation using only projective measurement, quantum memory, and preparation of the 0 state
> Authors: Michael A. Nielsen
>
> `quant-ph/0108067`:
> Title: Computational model underlying the one-way quantum computer
> Authors: Robert Raussendorf, Hans Briegel

I am especially taken with the Raussendorf-Briegel development. On one level, I think it might be the simplest avenue for addressing quantum computation from a Bayesian point of view: One just builds up the proper initial (universally valid) entangled state, and then does a measurement site by site, doing proper (Bayesian) updating of the quantum state of the remaining sites at each step. At the end of the day, one's knowledge is updated to the sought after answer. On another level (not for RMP), I think it starts to capture what I have been hoping for as an explanation of the power of quantum computation: it is not quantum parallelism that is doing the trick, but the "zing" of quantum systems that makes them sensitive to our interventions.

3) Let me put further below my replies to Khrennikov on his "contextual probabilities" business. I've already shared this with Carl, but not Rüdiger yet. What I like about my statement there is that it starts to put Gleason's theorem in a more Dutch-book kind of light. Just as in classical theory, the setting of initial probabilities is completely free (and therefore subjective). What is set by coherence/rationality is the transformation rules. To that extent, this is the objective content of probability theory. Similarly with quantum mechanics in the light of Gleason's theorem: The objective content of quantum mechanics (or at least part of it) is that if we subjectively set our probabilities for the outcomes of any informationally complete POVM, we are no longer free to set them arbitrarily for any other observable. The probabilities are now fixed and can be calculated uniquely from our previous subjective judgment. (I hate the American way of spelling "judgement.")

That's about it for now. I've certainly written loads more (Bayesian oriented stuff) since we've last seen each other. But nothing else that might have been striking is coming to mind right now. I'll probably unload more as I think of it.

As I'm hearing about more and more of you arriving in Santa Barbara, I'm starting to get a little envious that I'm missing all the fun. At least Kiki and Emma will be off to Munich Monday evening. I won't be joining them until the following Wednesday. I have this great dream of idling away my hours in the mean time with Arthur Schopenhauer, William James, and the Reverend Bayes. But I'm sure some reality will set in to knock me off my course of purity.

20-08-01 *Unloading Bayes, 2* (to C. M. Caves)

Cavesism 2: *I'm not sure anyone is going to be convinced by the above, but I think it is a start. So here's the scenario. There is a big set of things that can be true or false. The big set is determined*

by the (physical) theory you are dealing with. There are further rules that say how the elements in this big set can be gathered into subsets that correspond to questions whose outcomes are exclusive and exhaustive. Now, if you make noncontextual probability assignments to the questions, you have just ignored the structure that led to your original set. That being the input from your basic theory, if you make noncontextual assignments, you are deciding not to pay attention to your own theory. Not too bright. So you should make noncontextual assignments.

I know this won't come as a surprise to you, but (really) I found my (operational) explanation much more convincing. So, I guess I really didn't take my cue from you after all. Probably more from Pitowsky/Renes/Hardy.

Can you pinpoint what you didn't find convincing about my argument?

My trouble with your argument is that I still don't find the statement, "if you make noncontextual probability assignments to the questions, you have just ignored the structure that led to your original set," all that compelling. I guess I still don't quite understand what you are saying.

21-08-01 *Contextual Reality = Information??* (to P. Grangier)

Thanks for the longer explanation. It has indeed clarified things for me.

What I think is funny though, is that for precisely everything you say below (which I quote), I would call the quantum state "information" rather than an "objective reality."

Let me ask you this: Would you reread Section 3, "Why Information?" in my `quant-ph/0106166` and comment why and how you would use a different language in rewriting that? It seems to me that to say "a (pure) quantum state is objective, but it is contextual, ie, defined relatively to a particular set of measurements results" (as you do), can only be to teeter very close to admitting that the state is information (and only information). If the state is only defined relative to Alice's measurement results, why not call those measurement results the actual reality and be done with it? What does having the quantum state being objectively real add to the story?

Grangierisme 3 : *Alice may obviously choose between several measurements, but she must eventually decide for one and perform it. My definition of the "objective quantum state of a system" REQUIRES that Alice's measurement is completed, which does not "toggle" the state, but simply define it (at Alice's location).*

Once Alice's measurement is completed, and given her measurement result, she will be able to make predictions about Bob's state. But obviously the corresponding information (orientation of Alice's polarizer and measurement result) will have to travel (non-instantaneously) from Alice to Bob. Thus there is nothing here like "objective instantaneous action at a distance", that I dislike as much as you do. On the other hand, there is an objective quantum state, in the sense that once Alice's results have reached Bob (and not before !), Alice and Bob will know "the values of a set of physical quantities, which can be predicted with certainty and measured repeatedly without perturbing in any way the system". In this view BI are violated as a consequence of the lack of "separate reality" of the two particles, rather than as a consequence of non-locality.

To put it another way : a (pure) quantum state is objective, but it is contextual, ie, defined relatively to a particular set of measurements results. If new measurements are done that are not in the initial set, the definition of the new physical state MUST include the results of the new measurements. In some sense, this is simply a restatement of Bohr's 1935 answer to the EPR article (and maybe this can be related to your "Bayesian" approach ?).

21-08-01 *Noncontextuality* (to C. M. Caves)

Cavesism 3: *This is a weird notion of operational, since you rightly note immediately afterward that it is really tautological. How does it justify assigning the same probability to an element in the big set, no matter what the context, to note that you have a rule that assigns the same probability to an element in the big set, no matter what the context? It seems to me the real point is why you would ever identify two elements from different contexts, and the reason is that that they are the SAME element in the underlying set, which is handed to you by your underlying theory. Moreover, you only have one thing to go on, and that is the fact that the theory tells you that two elements are actually the same element in the underlying set. This means that the theory wants them to have the same probability in all contexts.*

I think what is creeping in here (anew) is a fundamental philosophical difference that is starting to come between us. Maybe I can characterize it in the following way. You want the theory to come first, and then to (somehow) recover our activity as scientific agents back out of that grander picture. I, on the other hand, am becoming more and more content to start with the scientific agents and thereafter pluck out those terms in their discourse with various common features to call a theory.

Cavesism 4: *How does it justify assigning the same probability to an element in the big set, no matter what the context, to note that you have a rule that assigns the same probability to an element in the big set, no matter what the context?*

It doesn't justify assigning the same probability. Assigning the same probability is the very reason for assigning a common element in our theoretical descriptions of two very distinct devices.

22-08-01 *Contextual and Absolute Realities* (to P. Grangier)

Grangierisme 4: *This may sound rhetoric, but if a theory explains nothing less than the stability of matter, is able to calculate g-2 (and many other things) with an incredible accuracy, and nevertheless does not speak about "physical reality", what is physics? I thus consider much more useful to put forward a reasonable definition of reality, that allows me to say to a journalist : "QM speaks about the reality of micro-objects. It is a weird reality, ... but it is REAL."*

You ought to know that I could not agree more with this (the part that I quoted, not the part that I did not quote). The issue on my mind is whether it is productive to view the quantum state in particular as THE term in the theory that corresponds to the objectively real. The way I view the issue is this: Quantum theory is a mixture of objective and subjective elements and we will only make progress in quantum foundations when we have had the intellectual strength to cleanly separate those two ingredients. With this point of view, I can answer the journalist just as soberly as you (i.e., without carrying him off to a postmodern fuzz fest).

There are certainly elements in quantum theory that I am immediately willing to identify as objectively real. A good example comes from our Alice and Bob example, generalized ever so slightly. Suppose Alice's system has a Hilbert space of dimension E and Bob's has a Hilbert space of dimension D, and that again Alice and Bob start off with an entangled state for the bipartite system. I say that the quantum state must be (freely) subjective information because, depending upon what measurement Alice chooses to perform on her side, she will ascribe one or another quantum state to the system in Bob's possession. However, there is one obvious thing that Alice

cannot change by the free choices she makes on her side of the world: It is the dimensionality of Bob's system's Hilbert space. Thus, I would say the number D is the objective reality in this situation. The number D remains constant regardless of what Alice does.

I could put this in language more to your liking, saying something like:

> The quantum state that arises for Bob's system after Alice's measurement is a "contextual reality," whereas the Hilbert-space dimension of Bob's system is an "absolute reality."

but I personally don't see that as a road to further progress. I.e., it distracts from what I view as the main point of our task in clearing up the foundations: namely, working hard to separate the subjective from the objective.

What I want to know in the most physical of terms is what does that number D signify? I want to find a way of describing its meaning that never once refers to a quantum state. When I can do that, then I will say that some progress has been made in identifying the objective part of quantum theory. But that is just an example.

Grangierisme 5 : *Remember my quotation about the 2 electrons in He : in their singlet state is "subjective information", how do the electrons know it?*

It seems to me, this is just a varied form of the Penrose argument I wrote at the end of my Section 3. I dismiss it in the same way that I did there. From my point of view, to say that the electrons are in a singlet state is to give nothing more than a compendium of things we can say about how they will react to our experimental interventions into their nonhuman bliss. It is not they who can predict the consequences of my invasions into their territory, it is me.

22-08-01 *Identity Crisis* (to C. M. Caves & R. Schack)

I enter this note with trepidation, because I know that what I am about to say will not be taken lightly by either of you, and chances are you will just view me as a troublemaker (again). I don't want to be a troublemaker, but I do have some concerns that are starting to eat more and more at me.

The problem is, I am starting to have serious misgivings about our Sections II and IV of "Making Good Sense" (PRA Version). Most of this new thinking has come about through my taking David Mermin's quest in his paper "Whose Knowledge?" to task, and my ensuing debate with Todd Brun, Jerry Finkelstein, and David himself. But some of it, I think, flows directly from the spirit of Bruno de Finetti, which I now believe I had shut my eyes to for too long.

The issue is no less than whether we really believe probabilities are subjective or not. I think a failure on our part to take their subjective character completely seriously is causing us to go down a path I'd rather not take.

Let me try to explain as best I can. The trouble is localized in our claim:

> ... if two scientists have maximal information about a quantum system, Dutch-book consistency forces them to assign the same pure state.

In the mildest version of my troubles, I am starting to think this statement is contentless. In the stronger versions, however, I find it misleading, and I almost want to say "wrong" (though maybe I won't go that far).

To make sense of what I mean by this, let me start by taking a cue from Bernardo and Smith:

> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. ... The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way. — p. 4, Bernardo and Smith, *Bayesian Theory*

If we accept this, then I think there is a much better way to word our "$p = 1$ when certainty" addition to the Dutch book argument in Section II. It seems to me it should more properly be viewed as a normalization condition, subordinate *only* to internal consistency/rationality as all the rest of the Dutch-book argument is. I say the latter to contrast it with how we presently have the argument worded in our paper—namely, by making the $p = 1$ condition subordinate to some objective feature of the world. We write:

> The only case in which consistency alone leads to a particular numerical probability is the case of certainty, or *maximal information.* If the outcome E is certain to occur, the probability assignment $p < 1$ means the bettor is willing to take the side of the bookie in a bet on E, receiving an amount px up front and paying out x if E occurs, leading to a certain loss of $x(1-p) > 0$. Consistency thus requires that the bettor assign probability $p = 1$. More generally, consistency requires a particular probability assignment only in the case of maximal information, which classically always means $p = 1$ or 0.

What does it mean for an outcome E to be "certain to occur"? I think that phrase is much more loaded than we have previously treated it. In the Dutch book argument there are three players, two of them animate and conscious (the bettor and the bookie) and one of them presumably inanimate and unconscious (the world). To which player does the certainty get attached? I don't think we make this clear in the way we ought to.

If the certainty is to be attached to the world, then what business does it have to do with my subjective judgments (which by definition cannot be in a bijective correspondence with the world's states)? Instead, I would say the "certainty" can only be a subjective judgment in and of itself. The Dutch-book argument for requiring $p = 1$ in the case of certainty should then be more accurately advertised as a call to be "true to our hearts." I.e., the argument is really that, *when* we believe an event will happen with certainty (a nonnumerical judgment), then we should ascribe $p = 1$ (a numerical judgment) for booking purposes. That is, Dutch-book coherence gives us a way to translate a nonnumeric belief into a numeric one.

The thing that is really at issue here is that I think we should remind ourselves always that "certainty" itself is nothing more than a belief. It may be a belief that can ultimately be tested against the world in a single shot, but nonetheless it is a belief. I believe with all my heart that my mother loves me; Schopenhauer believes with all his heart that she hates me. The only thing Dutch-book consistency can give us is that "if we wish to avoid the possibility of undesirable consequences" (Bernardo/Smith), then I should ascribe $p = 1$ and Schopenhauer should ascribe $p = 0$ to the proposition "love." The Dutch-book argument prescribes that we each should be true to our hearts—that we should both act in accordance with our beliefs. But it does not have within it the power to make us believe the same thing ... EVEN in the case of "certainty."

Now, we whitewash all that by introducing the phrase "maximal information," which somehow makes "certainty" seem more objective, but now I'm starting to think that that phrase is pretty impotent in this context. What role does it really play in our argument? I can't find any, other

than that it is a euphemism for declaring that we *believe* we have nothing left to learn (in the sense that we *believe* there is nothing left to learn from the remainder of the world that will help us refine our predictions for the system at hand). That belief may be wrong in the sense that rationality AND the world will not allow us to perpetuate the belief AFTER the experimental trial, but until the trial, "certainty"—from the Bayesian view—can mean nothing more than a metered belief.

You should be able to tell where I'm going with this by now. In Section IV, we write:

> Maximal information in quantum theory instead corresponds to knowing the answer to questions that share one particular projector.

I suppose what I am saying is that I just cannot accept this anymore. At least not in its present form. Instead, if I were to modify it to bring it into alignment with everything I said above, I would have to write something like this:

> Maximal information in quantum theory instead corresponds to believing adamantly that one knows the answer to all the questions that share one particular projector.

You might think this is nitpicking, but it completely takes all the steam out of Section IV. For it gives Dutch-book consistency no grounds for enforcing that two agents "with certainty" should believe the same thing. And consequently it gives no grounds for enforcing that two agents with "maximal information" should make the same quantum-state assignment.

The only way I see to reinstate our original role for maximal information is to say that two observers can only have maximal information *when* they are both right (in the sense that the world MUST CONFORM to their probability one predictions). But then, using our argument for Gleason's theorem, we would have DERIVED that quantum probabilities are objective probabilities! (This will be my only exclamation point in the whole note, so you should take it in seriousness.) That is, we would be saying that we have maximal information only when we *know* an objective reality, and by our derivation, that objective reality would then be equivalent to a compendium of probabilities.

Instead I think the best we can say is: If Alice and Bob both believe adamantly that they know the answer to some potential measurement AND that measurement happens to be the same for both of them, then Dutch-book consistency and Gleason's theorem will enforce that they make the same probability assignments for all other measurements (i.e., that they assign the same quantum state). But said that way, I don't think any non-Bayesian will be particularly impressed: For they would say that all we have shown is "if two people know the same things, then they will know the same things." Woop-ti-do.

The Bayesians among us will still have some room to be impressed: For it will not be a priori obvious to them that beliefs about one observable should have anything to do with beliefs about another. In particular, it might even surprise them that a common belief in certainty (for two observers) for any fixed observable should lead to equal probability assignments for all other observables. But even then, I think our the shock-value of our paper will be diminished. For I think in no way have we shown that when two observers make two pure-state assignments for a system, those pure states MUST be identical.

For me, this is a liberating thing to understand, i.e., that there are no dictatorial constraints on quantum state assignments. But I suspect you will feel otherwise, at least on the first reading of this note. So, let me beg your forgiveness in advance.

As I alluded to in the beginning, these thoughts of mine don't live in a vacuum. They have been spurred by my debates with Brun, Finkelstein, and Mermin. Thus let me give you some more material to chew upon: I'll attach it below, in the form of a composite note that I've already sent to

Rüdiger. Perhaps it will help clarify the things that have brought me to this position. Some of it, of course, will require that you try to imagine the context, but I think the notes are self-contained enough that you will be able to fall into the line of thought and see its relevance.

Best wishes (in spite of my predictable trouble),

23-08-01 *My Own Version of a Short Note* (to C. M. Caves)

I'm just back from a very long day in NY City (bookshopping), and a very long night before that (reading). So I won't reply to your notes until I'm a little more refreshed. But one quick comment:

Cavesism 5 : *I get the feeling, strengthened by your own confession that it is true, that my e-mail doesn't make much of a dent, so why bother with it.*

That's absolutely not true. I read everything you send me many, many times over. When they are reasoned well, I accept your arguments. And you know I much prefer this method of communication, just so I can have the opportunity to fully understand what my correspondent hopes me to absorb—I've never been a quick thinker, and this helps me fill in for that inadequacy.

Today I focused on rounding up some more William James, John Dewey, Percy Bridgman material. I think James is taking me over like a new lover. I had read a little bit of him before, but I think I was more impressed with his writing style than anything. But I was drawn back to him by accident, after reading Martin Gardner's *Whys of a Philosophical Scrivener*. Gardner devoted a lot of time knocking down James' theory of truth, because it is just so much easier to accept an underlying reality that signifies whether a proposition is true or false, rather than saying that the knowing agent is involved in eliciting the very proposition itself (along with its truth value). And something clicked! I could see that what James was talking about might as well have been a debate about quantum mechanics. He was saying everything in just the right way. (Let me translate that: he was saying things in a way similar to the way I did in my NATO "appassionata.") And things have only gotten better since.

Have safe trips to everywhere you need to go. Kiki is due December 23, but I'll see what can be done about ITP in November.

27-08-01 *Bayesian Pill Taking* (to N. D. Mermin)

I am finally writing to reply to your long note on "knowledge." I apologize for keeping you waiting so long, be even now—since Rüdiger is still in the woods—I feel a little like I am writing you prematurely. Nonetheless, I have this overpowering desire to get this issue out of my mailbox and be done with it. So here, I am. I would ask you, however, to please keep these thoughts private, at least until Rüdiger finally emerges. For, what I am about to say involves him (and Caves) directly.

I know that you think your note is ultimately conciliatory, writing:

Merminition 38 : *It seems to me that these are all valid statements about the formal structure of quantum mechanics, independent of what interpretation you favor.*

But after much soul-searching, I still cannot agree with your language. The soul-searching was required because this position of mine flies in the face of some of my very own published words (`quant-ph/0106133`).

The difficulty hinges on your Proposition 1:

Merminition 39 : *1. A system that is known (by somebody) to be in a state psi cannot be found (by anybody) to be in a state orthogonal to psi.*

In my new view, to say this is to throw away all that we have been striving so hard for in establishing that quantum states are subjective entities (and purely subjective entities). You admit so much yourself at the end of your Comment (c). [You can take this to mean that I also disagree with the beginning part of Comment (c).]

In 1880, I suspect there was not a single educated physicist who doubted one iota that the speed of light was set with respect to the stationary ether. Regardless of that, in 1881 the first evidence came out against the common belief. The *spirit* of your Proposition 1 would outlaw the very happening of that wonderful historical event.

I see no way (nor even want to anymore) to get around this. If the quantum state is a subjective entity, without rigid connection to the world in itself,—as I think Einstein's original argument for its subjectivity indisputably shows us—then this is something we simply have to live with. We must allow that experimental data can speak against our predictions, no matter how set we are to believe that they will not. Yet, on top of this, we must also accept that there is no objective sense in which a (pure) quantum state is simply "wrong" before the experiment is performed.

I put all my heart and soul into presenting this point of view in the note below to my coauthors, and I will share it with you. It took me a whole day to write the thing; I would like to think it is worth reading. (Also I have slight evidence that it makes sense in that Caves ultimately concurred himself.) I believe the note is just as relevant to you and your note on "knowledge" as it is to Caves and Schack. I will also send you the PRA version of `quant-ph/0106133`, so you can track the sections.

There. Now I will clean out my mailbox.

27-08-01 *A Little Contextuality on Noncontextuality* (to C. M. Caves)

I've finally got enough time to write a small reply to your old note.

Cavesism 6 : *There are two distinct approaches here, and I don't know which is to be preferred.*

1. The first point of view, which I have been pushing (as a way to justify noncontextuality), is that there is an underlying theory that sets up the structure of questions. This theory is primary. Noncontextuality emerges as the natural assumption that probability assignments should recognize the structure provided by the underlying theory.

2. The second point of view, which you have been pushing, starts with the role of scientific agents, as you put it, and uses the fact that probabilities are the same in different contexts to say that the elements with the same probabilities in all contexts explains why they are actually the same element in different contexts.

I do want you to understand my position, which is that I appreciate both these points of view. I'm not sure which will be the most fruitful in the long run. But they are trying to do quite different things. Here are two points:

1. The second point of view doesn't provide a justification for assuming noncontextuality, as you understand. Coming at things from the back door, as it were, it uses the fact of noncontextual quantum probability assignments to conclude that apparently different things are, in fact, the same.

I would not use language quite like that. I would say it IS a justification for noncontextuality. And it relies on quantum mechanics not one iota. Noncontextuality should be a property of any instrumentalistic theory (where Bayesian probability has been grafted onto to the world as the best

way for us to steer our actions within it). By an instrumentalistic theory, I mean one where we explicitly have to talk about our various possibilities for experimental intervention into nature—a theory where we cannot detach the experiment from the phenomenon.

Cavesism 7: *2. The first point of view appeals to me presently because it manages to make a long straight run to the state-space structure and the quantum probability rule given only the Hilbert-space structure of questions and probabilities faithful to that structure (i.e., noncontextuality). I think the second point of view needs to address the following question: given a set of elements to which noncontextual probabilities are assigned, what structure is forced onto the set by the existence of these noncontextual probabilities? This question seems hopelessly underconstrained to me, but Howard described to Joe and me some math research on this sort of question.*

What you say "needs" to be done, seems hopelessly underconstrained to me too. But I think you shouldn't view the problem that way. The point is, one simply has a way of clearing the air of the noncontextuality issue BEFORE getting down to the nitty-gritty of quantum mechanics. Noncontextuality is the base assumption upon which one plays a new game: What *physical* assumption makes it so that our instruments should correspond to POVMs and not some other mathematical structure?

By the way, you know I really dislike your phrase "given only the Hilbert-space structure of questions." I've probably said this to you before, but let me try to articulate why in more detail so that maybe you'll remember it a little better. In your own words, the phrase is "hopelessly unconstrained." What does it mean? It seems to me there are all kinds of possibilities one could imagine if one didn't know quantum mechanics beforehand. Here's a simple example: An even more basic feature of Hilbert-space before orthogonality is linear independence. When God came down and said, "You will use Hilbert-space structure for the questions you can ask of nature," why did he not mean that any set of linearly-independent vectors corresponds to a valid question? Presumably there are good reasons. But those good reasons need to be spelled out, and are not at all implied by the simple phrase "Hilbert-space structure."

Let me send you to pages 86–88 and pages 361–362 of the samizdat. There it is shown that linear independence does not mesh so well noncontextuality. It is a dumb exercise, I know, but it does indicate that ones need to be careful with one's phraseology.

Oh, let me tell you another thing, of historical note. I talked to Howard Friday, and he tells me that this kind of justification for noncontextuality goes all the way back to Mackey (but then he settles on the setting of ODOPs thereafter).

Cavesism 8: *I'm also quite interested in another question: How general are the theories where maximal information does not lead [to] certainty, yet does lead to unique noncontextual probability assignments? In other words, for what classes of theories is there a Gleason theorem?*

I think this is a really good question. I was talking to Eric Rains the other day and he thinks that the appropriate generalized setting might be the Jordan algebras. This is because this is the largest structure he knows where there is a notion of positive operator. (I had shown him the trivial POVM way of proving Gleason.)

If Gleason can be proved in such a wildly general setting, I think it would be quite interesting. For it would tell us that the quantum probability rule is not very closely connected to physics at all. Dreaming of the process of deriving quantum mechanics as successively tucking up the more general structure of Bayesian probability theory, one might say that the real physical assumptions don't come until much later in the game. That would be worthy knowledge!

Waiting for a stupid doctor's appointment,

28-08-01 *Some Questions Regarding Your Comments* (to N. D. Mermin)

Merminition 40 : *The trouble with our exchanges is that I'm always trying to zoom in on the issues under dispute and you're always trying to zoom out.*

Thanks for the note. You're point is a good one. (It's just that I've had to write so much email lately, and always, that it does take a toll. For instance, the present debate has been particularly taxing in that regard. Yet the issues have been important enough to not give up. So certainly I was hoping to recycle some material.)

I will try to write you a (focused) reply tomorrow, after recovering from today. Tomorrow evening, I fly out to Munich for a week and a half. Then I go to the quantum foundations meeting in Ireland to tote the wares.

Still tonight I've got to work on packing, etc.

01-09-01 *Left Wing, Right Wing, Not a Wing to Fly With* (to N. D. Mermin)

I was going to use my day today to write you a long, thoughtful (but focused) note on all the recent issues you raised with me on "Whose Knowledge?", but now you've gone and angered me. I mean that.

Merminition 41 : *I took my left-right terminology from the Science Wars. It seems to me that in arguing against anything objective other than knowledge Chris is taking a decidedly post-modernist position and therefore allying himself with the "Academic Left" attacked by Gross and Levitt.*

Is my point of view so very subtle that even my most sympathetic patrons cannot decipher it? Or have I finally caught on that you're really not listening to me after all?

You can't stand this, but you deserve it: I will excerpt part of a note I recently wrote to Andrew Landahl. What set me off in his case was when he wrote me the following after having read my paper "Quantum Foundations in the Light of Quantum Information" and given a Caltech journal club talk on it.

> On the whole I portrayed your "party platform" as the statement that "Quantum states are states of knowledge about the consequences of future interventions." In particular, those consequences aren't consequences to reality, but rather consequences to states of knowledge about even further future interventions. In this worldview Bayesian agents don't work to align their predictions with an underlying reality. Instead they work to align their predictions with *each other*. It is as if reality in this picture is solely the agreement of predictions!
>
> I'd be interested to hear if you believe that this is a fair characterization of your party's platform. After reading this paper, I came to the conclusion that you didn't believe in reality at all. (Or at best I thought you believed reality = knowledge.) John Preskill tells me you believe otherwise, namely that there *is* a reality, which surprised me. I'm curious to hear what you believe reality is.

The scheme below is that everything marked with a "$>$" is a direct quote from Andrew Landahl's letter. Everything else is either me, or a quote from my paper. I will put only the very most relevant part of my reply.

Please do read it before you—YES, you, the most trusted of my academic friends—slander me again. It is nothing if not EXACTLY relevant to what you wrote about me above.

With surprisingly kind regards,

02-09-01 *Left and Right* (to N. D. Mermin)

Merminition 42 : *I thought you were safely away in Ireland or pre-Ireland.*

Yes, I am in pre-Ireland mode (in Munich), but keep in mind that I am never safely away.

Merminition 43 : *I'll read what you sent in a little while. But note that I would not have made such a remark (even in jest) before I got your comments on my summary (and the cc of your letter to your own coauthors) which struck me (and I thought you too) as going beyond your earlier position.*

I will take this remark into account for the longer reply I am presently constructing for your earlier query. (It is sitting at 6K in length now, and will likely be finished tomorrow. Right now, I'm having my first beer of the evening.) But preliminarily, let me say that the only thing that letter to my coauthors did was clarify my position on the subjective character of the state vector. In that I went further than before, having gotten weak in the knees briefly about my position on two agents sharing differing quantum states. But, I do believe that I have long held fast in my opinion that there is something in the universe beyond human ken: It has always been a problem of finding the right language and right ideas for expressing what that something is, AND how it is PARTIALLY reflected in the structure of quantum mechanics.

If this does not make sense yet, I hope it will make more sense after my long note to you tomorrow.

02-09-01 *Intersubjective Agreement* (to N. D. Mermin)

Let me finally throw myself into the ring of intersubjective-agreement to see if I can wrestle you down a little. I will try to be every bit as focused for you as the issue will let me be.

Merminition 44 : *Well maybe you're more radical than I thought. It was to avoid correlations floating in the void, unattached to anything whatever, that I've been interested in trying to follow you down the path of knowledge. If all it led to were knowledge, floating in the void, unattached to anything whatever — even to other knowledge — then I'd be no better off [than] when I started down the trail.*

I think you misunderstand something very deeply here. The point of separating the categories "knowledge" and "reality" (or "subject" and "object" for that matter) is not to make knowledge an objective reality in its own right or, even worse, to make it the sole reality. Rather it is to say that there is a distinction and that that distinction should be recognized. I see nothing wrong with allowing a physical theory (such as quantum mechanics) to contain formal elements that correspond to *both* categories. The issue in my mind is which elements should be thrown into which category? The answer is not completely clear to me, but I am fairly convinced of one thing: The state vector should not be thrown into the reality side of the line.

What I have ultimately NOT been able to stomach about your wording of the whose-knowledge "answer", and Jerry's wording of the whose-knowledge "answer"—some of Todd's versions would

actually survive—is that you say, under certain circumstances, two scientific agents (observers, or what have you) MUST assign "consistent" quantum states to a given system. In the case of pure states, the two agents MUST assign the *same* pure state to the system.

Let me get that through your head: What I object to is the word MUST. Todd once wrote it this way,

Brunism 7 : *We have been describing a consistency criterion.* If *one wishes to combine two state descriptions of a single system into a* single *state description, the criterion tells one* when *it is consistent to do so (i.e., when the two descriptions are not actually contradictory).*

I agree that nobody is holding a gun to Alice's head and forcing her to incorporate Bob's information.

and to this way of speaking I can agree. But if you take away Todd's "*If*", then everything collapses in my mind. Enforcing that two agents MUST make the same state assignment if they are going to be "right" at all reinstates the very objectivity, the very agent-independence of the quantum state that the Mechanica-Quantica-Lex-Cogitationis-Est program has been working so hard to exorcise. [As you know, we made a serious misstep in our `quant-ph/0106133`, but that will be rectified in the next edition.]

It is much like the old debate. Is materialism right? Or is it Berkeley's idealism that is right? Who cares, I say. Both philosophies are just simple samples of realism: They only disagree on the precise concept which ought to be taken as real, mundane matter or sublime consciousness. The way you characterize it above, one would think that the only fruit of the Mechanica Quantica program would be the RENAMING of a material reality into an ideal one—a shift more of emphasis, rather than anything of grit.

Merminition 45 : *Are you also unable to agree with the statement that a photon that is known (by somebody) to have just passed through a horizontal polaroid cannot immediately thereafter be found (by anybody) to pass through a vertical polaroid.*

I'm asking you about this concrete example of the general proposition because I can't tell whether you're objecting to the language in which I generalized it or whether you object to the statement about photon polarizations too. If it's only the former I'm happy to use less provocative phrasing. All I meant by "be in a state psi" was "has been found to be" in the sense I specified prior to making the objectionable statement. But I worry that you object to both statements. In that case you are walking a dangerous path, denying that one of the most elementary applications of quantum mechanics has a legitimate meaning.

Here is what you are losing sight of. In the Bayesian world, two agents must agree a little before they can agree a lot. Agreeing a lot is the currency they are seeking, but agreeing a little to begin with is not the limitation of their existence. I'll come back to this from a more positive angle in a minute, but let me tackle your particular question before that.

What does it mean for "a photon that is known (by somebody) to have just passed through a horizontal polaroid?" Presumably it means that a particular quantum mechanical test, a POVM, $\{E_b\}$ has been performed and one of the outcomes of that test has obtained—in this case, the label b is "photon passed through horizontal polaroid." Now, you ask, "immediately thereafter [can it] be found (by anybody) to pass through a vertical polaroid?" Implicit in that, you are thinking that the state transformation, subject to the measurement outcome, is

$$\rho \longrightarrow E_b^{1/2} \rho E_b^{1/2}$$

up to normalization.

Suppose you are the somebody spoken of above; and let me be part of the anybody. Now, let us say that I stubbornly insist that the state transformation is

$$\rho \longrightarrow U_b E_b^{1/2} \rho E_b^{1/2} U_b^\dagger,$$

where the U_b are some unitary operators, and in particular, when

$$b = \text{photon passed through horizontal polaroid}$$

we have

$$U_b = \text{horizontal} \rightarrow \text{vertical}.$$

There is nothing in quantum mechanics (as a theory) that can keep me from believing that, so long as the ONLY thing specified is the "measurement" $\{E_b\}$. The point is, let us suppose we disagree on how our beliefs should get updated upon the incorporation of a measurement outcome into our knowledge bank. [As an aside, notice the distinction here: b is given the lofty title of knowledge, whereas ρ is subjected to being a belief. I allowed myself to do that because I am assuming we agree on the $\{E_b\}$, even if not the state-change rule. You might say we need at least this much to get the problem off the ground.]

So, it boils down to this in more common language,

Merminition 46 : *Are you also unable to agree with the statement that a photon that is known (by somebody) to have just passed through a horizontal polaroid cannot immediately thereafter be found (by anybody) to pass through a vertical polaroid?*

And I say no, I cannot agree: I saw Hideo Mabuchi in his lab yesterday, and I saw that he inserted a really fancy polarizer into his lab bench, one with an intriguing optical coating that allows horizontal photons in, but has them come out as vertical ones. I insist that I saw him do that: There's not a doubt in my mind. You insist that he is an honest upright boy, and he would never do such a thing to confuse us. With equal tenacity, there is not a doubt in your mind. We disagree, and in the strongest of ways.

How do we put our disagreement to test in the context of photo-detector clicks? WE insert a vertical polaroid—this one, I assume, we both do agree on—behind the "horizontal" one and see what happens. Aha! You were right after all! Mabuchi really did use an honest-to-god von Neumann polaroid; the input photon never made it to the final detector. It wasn't the fancy-coated polaroid after all, but I could swear I saw him put it in.

By now, I know that you are thinking I have gone through a ridiculously long-winded and pedantic way of describing a triviality: that one of us made a FALSE assumption. Implicit in your question was the reasonable starting point—indeed, the one we use in all common discourse—that all the agents involved start from a TRUE state of affairs.

But what can TRUE and FALSE mean in a world where our only handle for getting at things are SUBJECTIVE quantum states? We get at the world through our beliefs and belief updates—that's the fundamental tenet for me. And in that light, the only thing a FALSE belief can mean—as I put it to Caves and Schack in the infamous email—is that rationality (i.e., the Bayesian laws of thought) PLUS the world (i.e., the detector clicks, whose meanings in the end are also set by subjective considerations) will not allow the believer to perpetuate any remnant of his initial belief after the experimental trial. Let me say that sentence again for extra emphasis:

> The only thing a FALSE belief can mean is that rationality (i.e., the Bayesian laws of thought) PLUS the world (i.e., the detector clicks, whose meanings in the end are also set by subjective considerations) will not allow the believer to perpetuate any remnant of his initial belief after the experimental trial.

But if that is the case, what is so overpoweringly evil about having a "false" belief? Why must two valid quantum scientists necessarily be aligned in their beliefs, even in the case of "true" and "false"? I remain hardened: I see no compelling reason for asserting that necessity. Indeed, such an assertion is antithetical to the idea that a quantum state is a compendium of subjective degrees of belief.

If you think TRUE and FALSE mean something more substantial than I just described, then you tell me what role they play in my life other than as a kind of shorthand for some characteristics of my belief updates.

In pointing out these deficiencies, I am not "denying that one of the most elementary applications of quantum mechanics has a legitimate meaning." I am coming nowhere near that. I am merely asserting each scientific agent's constitutional right to believe what he will—i.e., to carry about whatever quantum state he wishes—SO LONG AS those beliefs do not contradict the constitution itself.

It is the latter that, from my view, is the most essential point you have been missing. So, let me get to that directly. With this I can finally start to define a positive program.

Merminition 47 : *I don't see that limiting "objectivity" to mean "complete and necessary intersubjective agreement" is abandoning your quest. Indeed, your Bayesian authorities say as much,*

> We can find no real role for the idea of objectivity except, perhaps, as a possibly convenient, but potentially misleading, "shorthand" for intersubjective communality of beliefs.

It must be the "necessary" that raises your hackles [. . .]

If anyone cannot see by now that it is almost solely the word "necessary" that raises my hackles, then they are not listening. You wrote at the beginning of your note that:

Merminition 48 : *If all it led to were knowledge, floating in the void, unattached to anything whatever — even to other knowledge — then I'd be no better off [than] when I started down the trail.*

But that is just not true. We have gained a serious amount of positive knowledge from this exercise. It has allowed us to see much more clearly what is firm and what is squishy in quantum mechanics. The state assignments I would say are always squishy; the rules for updating them are not. To the extent that these rules fulfill an edict in the spirit of Bernardo and Smith,

> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. . . . The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way.

I would say we have identified an objective piece of quantum mechanics.

It is not that "physics as intersubjective agreement" requires that agents always agree, or, at least, that they must agree in certain limiting circumstances. Instead it is that there is a procedure in any given case for deciding whether two agents will move closer to agreement than not after looking at the world. Quantum mechanics gives us such a framework. It might have been

otherwise: One can imagine a world so chaotic that any percipient beings which happen to arise in it would forever be in their own individual dreamlike states, never realizing that it is even possible to come to agreement with their fellow quixoticoids.

Maybe a good (but limited) analogy is this. Think of an electric potential function from which, by taking a gradient, we can derive the electric field. The potential itself cannot be the real stuff because of its ridiculous freedom that can be set freely from observer to observer. Instead it is the way the potential changes spatially that is what is of interest. That spatial change which is the common denominator of all the disparate potential assignments is the real, real stuff. Now think of the quantum state in the role of the potential, and the quantum structures of POVMs, the Gleason theorem, and the state-change rule in the role of the potential's gradient.

As long as you and I play according to the quantum rules for updating our beliefs in your example—you with your

$$\rho \longrightarrow E_b^{1/2} \rho E_b^{1/2},$$

me with my

$$\rho \longrightarrow U_b E_b^{1/2} \rho E_b^{1/2} U_b^\dagger$$

—who is to fault one of us for being irrational? We just have different beliefs about how a state ought to get updated in the particular situation. Neither one of us is taking the constitution to task; neither one of us are using a state-updating method that does not fall into the quantum mold.

The analogy of this with classical probability theory is that we both might agree on the probability for some hypothesis $p(h)$, but disagree on the joint distribution $p(h, d)$ for hypothesis and data. Learning the data and using Bayes rule, we will generally then come to two distinct posterior assignments $p(h|d)$. That nevertheless gives us no warrant for backtracking our opinion that $p(h, d)$ is just a subjective belief (as are all probability assignments). Instead it helps us see that the objectivity working in the background is Bayes' rule; it is our common denominator.

Quantum states—or through Gleason's theorem, nothing more than compendia of quantum probabilities—do not float in a void. They are tied together more tightly than any other probabilities hitherto ever found. I cannot assign probabilities for σ_x outcomes, σ_y outcomes, and σ_z outcomes at the same time as *independently* assigning them to the outcomes of any more exotic POVMs. In changing my probabilities for the outcomes of some potential new measurements (just after a previous measurement), I had better tie all those probabilities together along the lines of the general form of the quantum state-change rule.

In this, we see something like a much greater deepening of the Dutch book argument. In the classical case, we find that we will bring havoc upon ourselves if we allow ourselves to freely assign $P(A)$, $P(B)$, $P(A \wedge B)$, and $P(A \vee B)$ all independently. All compendia of quantum probability assignments must be tied to the particular structure of quantum states and the quantum state change rules. You should be thinking of the firmament rather than the void.

I think that's all the really general remarks I had wanted to say. Let me now *briefly* address the remaining specific points in your notes.

Merminition 49 : *If you do indeed object to both, then the only reason I can see for it (and I agree that this does raises non-trivial issues) is that probability 1 and probability 0 statements are idealizations — that nothing in the actual world we inhabit can be said to be certain or impossible. In that case, of course, the support of any acceptable density matrix is the whole Hilbert space and there is no content left to the criterion. But since the theory does allow you to talk about certain or impossible measurement outcomes, I'm reluctant to declare that its illegitimate to consider them in trying to develop a better understanding of the theory.*

I hope that you can see by now that "probability 1 and 0 statements being idealizations" (i.e., states of belief that we none of us, even Job, are really ever in possession of) has nothing to do with my considerations. A belief is a belief. Rationality itself cannot infringe on what numerical value a belief ought to be. It is therefore perfectly legitimate to think about these idealized situations.

Merminition 50 :

> CAF Said: on top of this, we must also accept that there is no objective sense in which a (pure) quantum state is simply "wrong" before the experiment is performed.

[Do] you also require me to accept there is no objective sense in which a pure quantum state is simply correct, after the experiment is performed?

Yes. The ghost of Bruno de Finetti haunts us:

QUANTUM STATES DO NOT EXIST

And I understand that oh so much better now than I did two months ago.

Merminition 51 : *P.S. I also asked for clarification of your views on objectivity as nothing more than intersubjective agreement, which on the one hand you seemed to reject in accusing me of going objective in comment (c) but on the other hand you seemed to endorse in quoting approvingly your Bayesian gurus.*

Bernardo and Smith would have never held fast to a "necessity clause" like you seem to be doing. That puts a gulf of distance between your two separate uses of the phrase "intersubjective agreement."

Merminition 52 : *P.P.S. Just got a blast from the Eastern Front (Mohrhoff — cc'd to you, I believe). I have the funny feeling that you two, who are so far apart in opposite directions (knowledge-without-facts vs facts-without-knowledge), may yet turn out to be strangely similar in some respects.*

To the extent that I understand him, I myself don't believe this is likely. The direction I see for physics, and quantum mechanics in particular, was perhaps no better put than by William James:

> Metaphysics has usually followed a very primitive kind of quest. You know how men have always hankered after unlawful magic, and you know what a great part in magic *words* have always played. If you have his name, or the formula of incantation that binds him, you can control the spirit, genie, afrite, or whatever the power may be. Solomon knew the names of all the spirits, and having their names, he held them subject to his will. So the universe has always appeared to the natural mind as a kind of enigma, of which the key must be sought in the shape of some illuminating or power-bringing word or name. That word names the universe's *principle*, and to possess it is after a fashion to possess the universe itself. 'God,' 'Matter,' 'Reason,' 'the Absolute,' 'Energy,' are so many solving names. You can rest when you have them. You are at the end of your metaphysical quest.
>
> But if you follow the pragmatic method, you cannot look on any such word as closing your quest. You must bring out of each word its practical cash-value, set it at work within the stream of your experience. It appears less as a solution, then, than as a

> program for more work, and more particularly as an indication of the ways in which existing realities may be *changed*.
>
> *Theories thus become instruments, not answers to enigmas, in which we can rest.* We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid.

Mohrhoff, from what I can tell, sees a "block universe" (to use another piece of Jamesian terminology). It is a completed thought in the cosmic consciousness.

Good wishes, and I hope this document answers more questions for you than it raises. Now I've got to run to the biergarten again for a little oompah-pah.

02-09-01 *Truth and Beauty* (to N. D. Mermin)

Here's another passage from William James's *Pragmatism* that may help reveal a little more of my mindset.

> The truth of an idea is not a stagnant property inherent in it. Truth *happens* to an idea. It *becomes* true, is *made* true by events. Its verity *is* in fact an event, a process: the process namely of its verifying itself, its veri-*fication*. Its validity is the process of its valid-*ation*.

03-09-01 *Subject/Object* (to M. A. Nielsen)

Nielsenism 1: *You may also be interested to hear that I'm engaged to be married :-)*

Excellent! This is only a joke partially, but lately I've been so taken with the idea that unions can give rise to things greater than those contained in the parts—thinking of quantum measurement, in particular, from this angle—I've thought about calling my view on QM "the sexual interpretation of quantum mechanics."

Many congratulations!

04-09-01 *Note on Terminology* (to C. M. Caves & R. Schack)

Thinking about it more, I would like to emphasize a point that was buried away as an "aside" in my recent note to Mermin titled "Intersubjective Agreement."

I am becoming more and more dissatisfied with the slogan "A quantum state is a state of knowledge, not a state of nature." The reason for this is that people tend to view the word "knowledge" as something that can be right or wrong, depending upon whether it is in direct correspondence or not with something in the external world. For this reason—as brought out clearly in my debate with Mermin, Brun, and Finkelstein—I think we should get more into the habit of calling a quantum state a state of BELIEF. This is more in line with the language both de Finetti and Bernardo and Smith use for probabilities anyway, and therefore gets us into a quicker connection with the personalistic Bayesians.

I now think it is much better to reserve the word KNOWLEDGE solely for the outcomes of quantum measurements once they become part of the mental makeup of an agent interested in them. I walk into Mabuchi's lab, and to the extent that he and I agree that he is performing some POVM (denoted by a set of positive operators $\{E_b\}$), it seems to me valid to call the outcome b

we both witness to be an addition to our knowledge. Now, what either of us may do with that knowledge is a different story. One thing is for sure, it ought to cause both of us to update our beliefs.

Thus knowledge (and information) bear on how we change our beliefs, and in that way—in a sense—become incorporated into our beliefs, but there is no rigid connection between the two concepts. Knowledge/information, as it is encoded in measurement outcomes, is a bridge to the external world that the quantum state has no right to be.

You may also recall another strange phrase I used in my note to Mermin: "the world (i.e., the detector clicks, whose meanings in the end are also set by subjective considerations)." This oddity is reflected in my definition of knowledge above: that is, I make a distinction between the raw stuff of the world that the measurement intervention brings about and the registration b in our noggins (as a flag for further actions in our role as agents). What I am thinking here is something roughly like the following. Take the famous white-on-black or black-on-white visual illusion that can be viewed either as a vase or as two faces facing each other. The raw stuff of the world may be compared to the ink and the paper giving the image. In order to say, however, that Mabuchi and I gain the same knowledge in viewing this we need the deeper cultural agreement that we will *both* call it a face or instead a vase when we see it.

Below I will put a glossary that tries to summarize where I have come so far in my attempts to make sense of quantum mechanics. Essentially, I'm expecting only the two terms above to be relevant to the fights we'll be having in writing our RMP article. But maybe it is nevertheless useful for me to lay my full set of language oddities on the table.

Lately, I've been jokingly calling my view (as it stands) the "sexual interpretation of quantum mechanics." (Most people turn red and become uncomfortable when I do that and explain why. I suspect the same will be true even in your reading of this note. So, brace yourselves.) The essential idea is that something new really does come into the world when two of its pieces are united. We capture the idea that something new really arises by saying that physical law cannot go there—that the individual outcome of a quantum measurement is random and lawless. The very fact that the consequence of the union is random signifies that there is more to the sum than is contained in the parts. But I promise you I won't reflect the licentious details of this view in the glossary below. I'll leave the missing terms to your imagination.

- ACT – The actual carrying out of a quantum measurement/INTERVENTION, after a DECISION has been made by an AGENT to do so.
- AGENT – Any participant in the construction of a scientific theory. In older language, the observer.
- BELIEF – In the context of quantum discussions, a quantum state. Or one might say the quantum state is a compendium of BELIEFS.
- CONDITIONALIZING BELIEF – The rule one uses to update one's BELIEF consequent to the completion of a measurement INTERVENTION. In the language of Kraus and Preskill this would be called the "quantum operation" or "superoperator", respectively.
- CONSEQUENCE – Whatever it is that a measurement INTERVENTION elicits out of the world.
- DECISION – An AGENT, within his power can decide to perform one ACT or another upon the world. Just as physical law cannot impinge on what determines the random outcome

of a quantum measurement, neither can it impinge on the mechanism behind an AGENT's decision.

- FACT – This is a word I do not like. One might have said that the outcomes of quantum measurement could be called facts just as well as CONSEQUENCES: But the word fact, to me, contains the connotation of a kind of permanence that I do not see in the quantum world. Facts are irreversible additions to the furniture of the world. But measurement INTERVENTIONS (and their CONSEQUENCES) can be reversed through the agency of a further outside intervener.

- INTERVENTION – The physical act that we call in older language the measurement of a POVM.

- KNOWLEDGE – One's mental representation of the obtained CONSEQUENCE of a given INTERVENTION into the world. Implicit in the use of this word, is that all communicating parties agree to the meaning of the given INTERVENTION, i.e., that it is this POVM rather than another.

- PROPERTY – A property is something possessed by a FACT. I don't like the word FACT.

04-09-01 *Brilliance* (to N. D. Mermin)

Merminition 53 :

> CAF Said: I am becoming more and more dissatisfied with the slogan "A quantum state is a state of knowledge, not a state of nature." ... I think we should get more into the habit of calling a quantum state a state of BELIEF.

Brilliant! All kinds of trouble would have been avoided.

You know, I'm not one to turn down a "Brilliant!" But your second sentence does clash a little with what you wrote on August 8:

Merminition 54 : *It seems to me Chris is getting much too subtle about this. I would talk about knowledge, not belief.*

All kinds of trouble WOULD HAVE BEEN avoided?

Speaking of brilliant—real brilliance this time—today I'm going into Munich to talk to Hans Briegel about his papers with Raussendorf. (I told you I would be in Munich, but I'm actually in the little village of Zorneding outside of Munich.) I think there's something very deep in them, if they hold up. You may recall I recommended them to you once.

I've got more things of a philosophic nature to write you, but I've just got to find some time to do it. I'll try to be back to the waves tomorrow.

04-09-01 *Objective Probability* (to C. M. Caves & R. Schack)

Cavesism 9 : *I expect you to have a really hard time with this—please skip the lectures on my not being sufficiently Bayesian—but it is, in my present view, a necessary feature that expresses the tension that exists in the notion that maximal information is not complete. The state assignment can't be verified by examining the system, but it can be verified by examining the trail of evidence*

from which I acquired maximal information. If someone else finding that trail of evidence could say that he didn't have maximal information or that he had different maximal information, the notion that the information is maximal would be untenable, since apparently something further would be required to make it so. This seems like a natural for someone who takes seriously those quotes about the process of intersubjective coming to agreement. It grants to maximal information in quantum mechanics some, but not all of the properties of maximal information in a realistic world.

It is hard for me to understand what that "trail of evidence" is a stand-in for if it is not a compendium of OBJECTIVE probabilities. You follow that trail, and you have NO CHOICE but to assign all the probabilities that the Gleason theorem gives (presumably if you are rational). So, pure quantum states give rise to "propensities" ... when those pure states are "right"? Is that what you are saying? (I said all of this, of course, in my original longer note, but it seems good to isolate it here.) Can you give me an operational definition of this notion of "propensity"?

And why can we toggle these propensities from a distance? Are you giving up on spacetime after all? Or is this a new way of applying the principal principle?

Now I really do have to join the family.

04-09-01 *Fourth and Fifth Reading* (to C. M. Caves & R. Schack)

Cavesism 10: *The point of our conclusion is that the Dutch-book argument leads to a unique probability or density-operator assignment in the case and only in the case of maximal information. This is just an entirely different thing from using frequency data—or something else—to specify every component of a density operator.*

I still don't get it (though I've had a lot of wine by now). By hook or crook, I use the information available to me to assign a probability distribution over the outcomes of some informationally complete measurement. That assignment gives rise to a unique density operator.

I'm still having trouble seeing what is special about a "maximal information" assignment. I'm not lying; I'm not trying to cause trouble; I'm just not seeing it. (Think of me as the second referee of the paper. Would that be ethical?)

Good night!

05-09-01 *Noncontextuality Again (and Again)* (to C. M. Caves)

Cavesism 11: *The underlying structure is a specification of alternatives that can be grouped in various ways—these are the contexts—to make exhaustive sets. We are required to make noncontextual assignments; otherwise we are ignoring the fact that this specification doesn't distinguish an alternative in two different contexts. If it did, we would be dealing with a different specification. This is the perspective of my first point of view, which justifies noncontextual probability assignments in quantum mechanics from the Hilbert-space specification of alternatives.*

I still don't entirely get this. You say we are required to make noncontextual assignments, otherwise we would be ignoring the fact that the original groupings do not distinguish an alternative in two different contexts. But why could we not ignore it? Perhaps the underlying structure is there for an entirely different reason than something to do with probabilities? For some reason, this point of view is just not clicking for me.

Cavesism 12: *Your perspective is different. As I understood it, you think of an alternative in different contexts as a single alternative because it has the same probability in all contexts. But where did you get this equal probabilities? Surely they're not measured or determined or anything like that, since they are states of knowledge. In quantum mechanics you get that they're the same because the standard quantum rule says so, but this is using noncontextuality, not justifying it.*

I don't know what more to say on this. It means that identifying this consequence of this intervention with that consequence of that intervention is a SUBJECTIVE judgment. (I.e., that identifying this outcome of this measurement with that outcome of that measurement is a subjective judgment.)

Cavesism 13:

> CAF Said: What you say "needs" to be done, seems hopelessly underconstrained to me too. But I think you shouldn't view the problem that way. The point is, one simply has a way of clearing the air of the noncontextuality issue BEFORE getting down to the nitty-gritty of quantum mechanics. Noncontextuality is the base assumption upon which one plays a new game: What *physical* assumption makes it so that our instruments should correspond to POVMs and not some other mathematical structure?

I'm going to adopt your strategy, and just flatly say I don't get this.

Let me try again. Here is the game we should be playing. In the most general terms, a measurement is defined to be a group of elements (satisfying some given property) drawn from a set with a given structure. The individual elements correspond to the outcomes of the measurement. The question is, what should that structure be? What should that property be? What are the reasons for those choices? This much we will safely assume (for the reasons given above): The probabilities of the outcomes should depend only upon the individual elements, not the group.

That's all I'm saying. Here is an example of dumb theory.

> UNDERLYING STRUCTURE = one-d projectors onto a complex vector space.
> GROUPING PROPERTY = choose any set of projectors that project onto a complete set of linearly independent vectors.

Then the only probability assignment that can be given to the outcomes of such a notion of "measurement" is the uniform distribution.

So, we start over and say, "Maybe the grouping property ought to be that the projectors add up to the identity." Aha, that gives us quantum mechanics. But you see, there are any number of other combinations of structures and properties one might have played with. The question is, what is essential about the structure and grouping properties that we do use? By saying that we have cleared the air of noncontextuality, I simply mean that the existence of noncontextuality in the probability assignments should not be a question. It was settled before we ever started the game.

Cavesism 14: *Well, when I say "Hilbert-space structure of questions," I clearly don't mean only that there is a Hilbert space, but that the questions correspond to one-d projectors. That's why I add "of questions" to the phrase.*

No, what you mean precisely is: A "question" corresponds to a set of one-d projectors that sum up to the identity. So why don't you just say it in a precise way rather than a vague way? If I were uninitiated to quantum mechanics, I might have thought that you meant the dumb theory above. I'm serious about this.

05-09-01 *Unique Assignment* (to C. M. Caves & R. Schack)

Cavesism 15: *The point of our conclusion is that the Dutch-book argument leads to a unique probability or density-operator assignment in the case and only in the case of maximal information. This is just an entirely different thing from using frequency data—or something else—to specify every component of a density operator.*

I guess my trouble stems from one of the things I said in the long note announcing my worries about "Making Good Sense." There I said:

> I think the best we can say is: If Alice and Bob both believe adamantly that they know the answer to some potential measurement AND that measurement happens to be the same for both of them, then Dutch-book consistency and Gleason's theorem will enforce that they make the same probability assignments for all other measurements (i.e., that they assign the same quantum state). But said that way, I don't think any non-Bayesian will be particularly impressed: For they would say that all we have shown is "if two people know the same things, then they will know the same things." Woop-ti-do.
>
> The Bayesians among us will still have some room to be impressed: For it will not be a priori obvious to them that beliefs about one observable should have anything to do with beliefs about another. In particular, it might even surprise them that a common belief in certainty (for two observers) for any fixed observable should lead to equal probability assignments for all other observables.

What I am wondering is: What would impress a devout Bayesian (who is just learning quantum mechanics) about our argument? Thus, given what I said above, I wonder whether he would not be equally impressed by the following. By hook or crook, Alice and Bob individually come to their own subjective probability assignments for the various outcomes of a single informationally complete POVM. Then, because of Gleason, they will have to match in their subjective beliefs about the outcomes of all other measurements they might perform. That matching made no use of the concept of maximal information. What does the maximal information case give us in shock value?

I think what you're going to say is that in the case of nonmaximal information, Alice and Bob may have come to different probability assignments for that informationally complete observable. And that they couldn't have done that if they had had "maximal information" in the first place. But as all this debate has already shown, I think I reject that position.

As my glossary from yesterday attests, I think what is going on with me is that I am becoming ever more uncomfortable with identifying quantum states with information, maximal or otherwise. Thus, instead of calling a pure state maximal information, I am becoming more inclined to something like:

$$\text{pure state} \;=\; \text{maximally tight belief (or judgment)}$$

or

$$\text{pure state} \;=\; \text{a nonrefinable belief (or judgment)}$$

or anything else more along those lines.

Leave the word information for what we gain when we see the outcomes of measurements. This entity we know two disparate observers should agree upon—by definition—if they are in free communication with each other. But the quantum state, on the inside of one's head, is a more personal state of affairs.

I'm going into Munich for much of the day (to visit with Briegel), so you may not hear from me again until tomorrow.

05-09-01 *Identity Crisis, 2* (to C. M. Caves & R. Schack)

I've now given your long note the fourth and fifth readings it deserves. (I'm holed away in an office near Briegel's.) And I'm not sure how to respond yet. I think I will await your responses to my other notes first. I think it is clear in the time that has elapsed since our first communication that we have moved further away from each other's position. Or I should say I've moved further away, from our original position.

Briegel just came; I'll be back.

05-09-01 *Malleable World* (to G. L. Comer)

Just a little quote I liked.

> Once you bring life into the world, you must protect it. We must protect it by changing the world.

From: Elie Wiesel (b. 1928), Rumanian-born U.S. writer. Interview in Writers at Work (Eighth Series, ed. by George Plimpton, 1988).

06-09-01 *The Underappreciated Point* (to C. M. Caves & R. Schack)

My fit of insomnia is running out, so I'm going to have to go back to bed soon.

Schackcosm 3 : *Please give me some feedback on these thoughts. I know that you wrote A LOT more, but I find it easier to go through your emails paragraph by paragraph.*

You know, of that LOT, most of it was written very carefully and very purposefully, so I do hope you will try to read it all with that in mind before trying to get me to readdress too much. (You can ignore the stuff on noncontextuality for now; that's not important for present issues.) Next week I won't have the leisure of writing too many notes, as I'll be out on the election trail trying to stump this Bayesian point of view ... and trying to make it LOOK consistent. (I.e., I'll be at the quantum foundations meeting in Ireland.)

But, before crawling back into bed, let me address your greatest FEAR:

Schackcosm 4 : *The most important thing to remember is the limited scope of the paper. It tries to show that Bayesian probabilities do have a place in qm, not more. Remember that most physicists would reject this. We show that, contrary to conventional wisdom, subjective quantum probabilities are not arbitrary. Let me remind you that you agreed to a paper of this limited scope last year in Montreal. I do not want a paper that is significantly expanded.*

This must be addressing my single remark of September 3,

> Of course, as you know, I view this as a good opportunity (with page limitations no longer of great concern) to expand some points and give some more references. I think we can read the referee as agreeing with that.

because, in all my voluminous letters, I don't think I ever mentioned modifying the paper otherwise. Of course I think it is unhealthy that your first trip-up would be that point ... but we are all moved by different things. (I don't fathom your pressures, and you don't fathom mine.) In any case, let me say this for the record to try to clear the air:

> If given free reign—which I do not actually want—I think I could modify the paper to my own tolerance without changing its length at all, or, at most by a paragraph. I would be more than happy if you and Carl would just find a language I could agree to, and modify the paper accordingly. The phrase *more references* was a euphemism for citing more of Chris's papers.

Deep in my heart, I believe you guys fool yourselves in thinking that this paper will be more widely read simply by being short, but that is not the issue (and it has never been the issue). My passion is to get quantum mechanics straight: So, let's get it straight.

On the whole, in reading your two notes, I found your method of expression better fit to my present mentality than Carl's. Maybe I'll give more specific examples tomorrow. For the present, let me just mention two things.

Schackcosm 5 : *It does indeed not follow from our Dutch book argument alone that two agents must agree on the maximal info they have. But suppose agent A has maximal information and agent B insists on assigning a pure state that is not consistent with A's information. A can then extract money from B. I don't think the symmetry of the situation is a problem here. From A's perspective, B is wrong, in the same sense of wrong as if A had a piece of classical knowledge that B chooses to ignore in a bet.*

Perhaps the greatest life change I have had is that I no longer like the phrase "maximal information" in this context. That little phrase carries with it an entire philosophy, and it is one that, to me, does not seem consistent with its roots and, more importantly, does not seem right.

The most one can say on Bayesian principles is that:

> From A's perspective, B is wrong. And from B's perspective, A is wrong.

If A and B can have two pure-state assignments, and the most one can say is the item above, then pure states should not be called "maximal information." They are maximal "something else", but it is not information. (In another note, I have outlined what I think that "something else" is.)

Carl thinks he can fix this by invoking a "trail of evidence" that uniquely fixes which of two pure states is actually the case. But let me juxtapose two of his paragraphs and then try to reemphasize the underappreciated point.

> Paragraph 1:
> In quantum theory maximal information also constitutes a belief, but we resist the notion that it corresponds to some objective reality out there. Why this resistance? Ultimately it's because the maximal information leads to a pure-state assignment that gives probabilities whose only reasonable interpretation is subjective. It is very important to remember that this is the primary motivation for much of what we do. Probabilities are the subjective language used to deal with situations of uncertainty, so wherever we find them, they must be subjective. The subjective view of pure quantum states gains additional support from the fact that a pure-state assignment can't be verified by consulting the system—the same can be said for a probability assignment—and

> the fact that a state assignment for a distant system changes when we obtain information about it without ever getting close to it—this also holds for correlated probability assignments.

and

> Paragraph 2:
> The state assignment can't be verified by examining the system, but it can be verified by examining the trail of evidence from which I acquired maximal information. If someone else finding that trail of evidence could say that he didn't have maximal information or that he had different maximal information, the notion that the information is maximal would be untenable, since apparently something further would be required to make it so. This seems like a natural for someone who takes seriously those quotes about the process of intersubjective coming to agreement. It grants to maximal information in quantum mechanics some, but not all of the properties of maximal information in a realistic world.

If you hold fast to the view that that trail of evidence must EXIST, then you hold fast to the view that quantum probabilities (in some cases) must be objective after all ... INDEPENDENTLY of the issue of intersubjective agreement. And that negates Paragraph 1.

I am now of the opinion that if we can just clear the air [I'm fond of that phrase] of this nonBayesian trapping from bygone times, we will finally be in a position for real progress. It is in Dutch-bookian type coherence (as a general principle) that one finds an objective statement in quantum mechanics; it is never in the quantum state itself, even when that state is pure. The OBJECTIVE statement is: All of you, each and every one of you, should manipulate your compendia of beliefs according to the rules of quantum mechanics if you wish to maximally avoid undesirable consequences in your gambles. The particular quantum states at any one time are just thin films of subjectivity floating on that wider sea of objectivity.

But, please, please do read the other notes carefully. I can only write a finite amount. I'll comment more particularly on your present notes tomorrow (i.e., today, after I get back up).

06-09-01 *Another Way* (to C. M. Caves & R. Schack)

> If you hold fast to the view that that trail of evidence must EXIST, then you hold fast to the view that quantum probabilities (in some cases) must be objective after all ... INDEPENDENTLY of the issue of intersubjective agreement. And that negates Paragraph 1.

Let me put it another way. By Carl's view, if trails of evidence MUST exist, then quantum states MUST exist, and the ghost of Bruno de Finetti should have stayed in the netherworld. For the probabilities derived from the quantum state will exist after all.

06-09-01 *Some Comments* (to C. M. Caves & R. Schack)

Now I return from an unrestful morning in bed.

Schackcosm 6 : *I'd say subjective but not arbitrary. Don't forget that the idea that quantum probabilities can at all be viewed as subjective is shocking to most readers.*

It won't be particularly shocking if, in the end, we reinstate their objectivity.

Schackcosm 7 : *I guess you are right that we should be more explicit about "whose certainty". It is the bettor's certainty.*

The deeper issue is not that we *should* be more explicit about "whose certainty," but *why* we should.

Schackcosm 8 : *You should leave Schopenhauer and your mother out of this discussion. The distinction between the cases of certainty (classical logic) and reasoning in the face of uncertainty (probability theory) is useful.*

I didn't understand this comment.

Schackcosm 9 : *As I said in my previous message, two agents having conflicting certainties is a completely classical situation. If you accept classically that in this case, one of them must misread or ignore some of the available information, then the point of the paper is that the same classical argument gives you unique state assignment, even though states are Bayesian. This is a forceful conclusion.*

It is safer to have the wrong metaphysics in the classical case. This is because certainty (i.e., overpowering belief in the outcome) for one question means certainty for all questions. And that certainty can be verified or falsified in a single shot. So, one gets in the habit of thinking that the proposition (or its material counterpart, as instantiated in the world) was already there before the verification. One can accept that metaphysics or leave it, but it is usually more convenient to accept it. In the quantum case, however, if you assert that the proposition was already there (say, as uniquely specified by Carl's "trail of evidence") then you have to assert that all the rest of the quantum probabilities were already there too. That sounds an awful lot like objective, agent independent probabilities to me.

You can retreat to objective probabilities if you wish. But I say it is better to be creative with our metaphysics. JAW said it like this, "No question, no answer." And that distinction is rearing its head in this very problem.

Schackcosm 10 : *Making a pure-state assignment is an extreme statement. It entails the conviction that assigning a different state is equivalent to handing over money. It entails the conviction that the agent assigning the different state is wrong in this sense, in the sense of irrational behavior, not in the sense of not conforming to reality.*

It "entails the conviction." That is language I can accept. It is language I like. Trying to instate that way of saying things has been the whole point of my writing such detailed notes, especially the point about "not in the sense of not conforming to reality." But though you use it so nonchalantly now, it had no representation in our previous discussion, and it has no representation in our paper. At least looking at myself personally, I feel as if I have come through a phase transition.

Schackcosm 11 :

> CAF Said: But said that way, I don't think any non-Bayesian will be particularly impressed: For they would say that all we have shown is "if two people know the same things, then they will know the same things." Woop-ti-do.

Still quite impressive. A and B know the same certain thing. Hence they must assign the same subjective probabilities to all questions. Even subjective probabilities 0¡p¡1 are prescribed by this knowledge. The non-bayesian should be quite surprised and impressed by this.

I said non-Bayesian. Non-Bayesians do not accept subjective probabilities.

Schackcosm 12 :

> CAF Said: For I think in no way have we shown that when two observers make two pure-state assignments for a system, those pure states MUST be identical.

If they are not identical, each agent has perfect reason to assume that the other one is unreasonable.

I accept that. But the point has been, and remains, that that is the ONLY conclusion we can draw.

Schackcosm 13 : *There are dictatorial constraints only in the limiting case of maximal information.*

Unless all of this email has been a grave mistake on my part, I continue to not be able to accept this. The only argument we have at our beck and call is that Dutch Book + Gleason dictates what *I* must do in my head and what you must do in your head. It tells us each how to translate a *nonnumeric* belief (certainty) about the outcomes of a single question, to a *numeric* belief about the outcomes of all possible questions. Indeed, I will lighten up: For a raw BAYESIAN that must be quite an impressive conclusion. There must a good way to say that in the paper. [For the non-Bayesian however—one with no qualms about objective probabilities, one with no qualms about the objectivity of quantum states—I remain in my belief: it will strike him as little more than a tautology.]

But all of this does not lessen my debate with Brun, Finkelstein, and Mermin which started this whole affair. There is no a priori principle in the universe that will tell us that two quantum states OUGHT to have overlapping supports. The best one can say is that IF Alice and Bob have overlapping support, then (if they wish) they may be able to communicate the reasons for their beliefs and come to a more refined consensus. If they do not have overlap in their supports, then the only they can do to lessen their strife is consult the world.

The objectivity is not in the states, but in the state-space STRUCTURE and in the answers the world gives us upon our consultations. When one has gained the latter, one has gained information. But the quantum state before and after remains belief, pure state or not.

06-09-01 *Weak Point* (to C. M. Caves & R. Schack)

Cavesism 16 : *I think we all agree that if states are Bayesian , then anyone can assign any state he pleases, including any pure state. He can be misled or tricked, or he can just be crazy, but this sort of freedom to assign any state is not of much interest for our paper. An objectivist will have no trouble agreeing that someone who is misled or irrational will use the "wrong" quantum state.*

Let me try to say it again. The main point is, in the quantum mechanical world, these "trails of evidence" you are thinking of in the back of your mind are NEVER enough to uniquely specify a quantum state. It has NOTHING to do with being misled or being irrational. Even the purest of states is thoroughly infused with belief from the get-go. That is what my note titled "Fw:

Intersubjective Agreement" from September 3 is essentially about. So, this is not a case of measure zero, where the players are irrational or dumb to begin with ... so long as we take our own arguments about subjectivity seriously.

Cavesism 17: *What he wants to know is whether scientists acting like scientists, sharing all information in a spirit of genuine co-operation, mutual respect, and dedication to truth, can assign different pure states. And we show that scientists acting like scientists can't: sharing maximal information, they must make a unique quantum state assignment.*

If that is what he wants to know, then he is not going to find it from anyone's Dutch-book argument: our last one, or our slightly modified new one. Your point is a weak one. The Dutch-book game is an adversarial game. Anyone whose intention is to make his opponent go bankrupt is NOT going to share everything he knows with him. He will be silent and bet his money.

Cavesism 18: *What rescues this conclusion from trivia? First, it answers the question of why science doesn't go down the drain: subjective state assignments are constrained in the case of common maximal information.*

I don't believe the conclusion is trivial; I said this to Rüdiger yesterday. But I also don't believe it has anything to do with rescuing science. Playing by the quantum rules ought to be enough.

Cavesism 19: *Second, it answers the question without referring to real, verifiable properties of the system in question. In a realistic world one might justify the agreement in the case of maximal information by saying that any disagreement can be resolved simply by looking at the system and seeing who's wrong.*

Disagreements in the quantum world are resolved also simply by looking at the system. Suppose you and I agree to everything in the world EXCEPT the quantum state for a given system. How do we resolve our dispute? We perform a maximally refined quantum measurement (a POVM with rank-one elements). We agree on the system's state thereafter. That is all that has ever been important in science anyway—that the world provides us with a way to COME INTO agreement for all FUTURE predictions. For god's sake, Albert Michelson did not believe that the speed of light could be a constant. But his tenure was not stripped away when he found a negative result. He revised his "impossible" belief and got over it.

ALL that one need to demand from a theory is that it provide a way for two agents to come to agreement for all FUTURE predictions. Quantum mechanics (surely) satisfies that. It has nothing to do with re-objectifying quantum probabilities, and I can't see that it has anything to do with this stuff we got in the habit of calling "maximal information."

06-09-01 *The Well Appreciated Point* (to C. M. Caves & R. Schack)

Your notes are well-appreciated themselves; I am finding reading them productive. Unfortunately, I cannot reply in detail tonight, but I hope more will be waiting for me tomorrow morning.

Let me do say though that I think I addressed some of your points in the note I just sent to Carl (and CC'd to you). The main thing was this:

Schackcosm 14: *What the Bayesian can say is: If A assigns a pure state, he knows with certainty that any other pure-state assignment is foolish (handing over lots of money).*

It is not a situation that can be resolved within science, by discussion or experiment or comparison of notes. Both A and B are certain there is nothing that could change their belief. For A, B could just as well reject all of quantum mechanics.

It is not a sin for A and B to disagree about the present. What would be a sin is if they could not come to agreement in the future. And quantum mechanics provides just such a mechanism. It is not true that experiment cannot change (absolutely firm) beliefs in the quantum world: quantum measurements are invasive, and thankfully so. Each measurement gives us the opportunity to throw away the past and start afresh.

07-09-01 *Email Not Received* (to C. M. Caves & R. Schack)

Schackcosm 15 : *But, as I argued in my last email, I think that a modified betting argument, now having A and B as adversaries (Carl thinks that this modified argument should not be called a Dutch book argument), shows that starting from two different pure states to come to a later agreement is not what science is about. A must dismiss B as a crackpot. This argument would be useful in the Peierls debate. But maybe not for our paper.*

I didn't receive such an email; can either of you send it again?

Indeed I would bet that science cannot be made in a (purely) adversarial environment. Science is about cooperation, trying our best to come to a consensus. (That is why I have not lost heart in writing all these ridiculous emails!) But, nevertheless, from time to time I do talk and try to come to consensus with people I deem crackpots. The point is, though someone may be adamantly wrong about ONE thing (say, a pure-state assignment from my perspective), it does not mean that he is adamantly wrong about ALL things. And therein lies a backdoor for a discussion with such a person.

The only thing that one has to trust in the making of science is that one's colleague is internally consistent. It is OK if he got SOME of the *facts* (*The Well Appreciated Point*) wrong (from my perspective), and that I got SOME of the "facts" wrong (from his perspective). It is enough that he is willing to join in with me in letting the world pull us together. I.e., that each of us is willing to participate in trying to convince the other that he is wrong by consulting the ultimate arbiter.

But, I'll write more later (in the context of your last two notes).

07-09-01 *(Backbreaking) Analysis* (to C. M. Caves & R. Schack)

Schackcosm 16 : *We need to find some common ground.*

Yes, that is true. And I think we already have some, maybe even a lot.

But, as I see it, there still remains a significant amount of trouble in the language we chose to use in the past ... and that is what is putting stoppers on our progress in the present.

Of course, I feel like I am repeating myself over and over, but let me go to your explicit "common ground" paragraph and try to lay out what I like and what I don't like about it. The thing that keeps me going is the hope that maybe this whole debate is a lot like beer: On their very first taste of it, most kids think it is a foul stuff. But after more and more of their friends offer it to them over time, it starts to become a pleasant diversion.

Schackcosm 17 : *Assume A has information of the kind we call maximal, i.e., A knows that a measurement of a POVM containing the 1D projector P will give the outcome corresponding to P. Then assigning any state but P will be Dutch-book inconsistent. This will be A's inconsistency with her own belief. The beliefs of the bookie or of Nature do not matter. A knows that assigning any other state would make her accept a bet in which she (not Nature or the bookie) knows that she will lose for any outcome she believes is possible.*

Now assume B has access to the same piece of maximal information. B then knows that a measurement of a POVM containing the 1D projector P will give the outcome corresponding to P. Then assigning any state but P will be Dutch-book inconsistent. This will be B's inconsistency with his own belief.

Hence: Two agents having access to the same maximal information MUST assign the same state.

Here is how I would reword it to suit my present tastes.

> Assume A is absolutely sure that a measurement of a POVM containing the 1D projector P will give the outcome corresponding to P. Then assigning any state but P will be Dutch-book inconsistent. This will be A's inconsistency with her own belief. The beliefs of the bookie or of Nature do not matter. A knows that assigning any other state would make her accept a bet in which she (not Nature or the bookie) is absolutely sure she will lose for any outcome.
>
> Now assume that B is absolutely sure of the same thing, i.e., that a measurement of a POVM containing the 1D projector P will give the outcome corresponding to P. Then assigning any state but P will be Dutch-book inconsistent. This will be B's inconsistency with his own belief.
>
> Hence:
>
> > Two agents having the same absolutistic belief about the outcome of a measurement containing the projector P *MUST* assign the same quantum state.
>
> Or equivalently (but, to me, more forcefully):
>
> > Two agents having the same absolutistic belief about the outcome of a measurement contain the projector P *MUST* assign the same (subjective) probabilities to the outcomes of all measurements that can be contemplated.

What were the main substitutions? Essentially, they were simply:

"maximal information" $\longrightarrow$ "absolutely sure"
and
"maximal information" $\longrightarrow$ "absolutistic belief"

And the same substitutions count for the word "know."

To me, those simple substitutions completely change the metaphysical complexion of the statement. The statement goes no further than it has to go to make the quantum Dutch-book theorem stand its ground. Why go further?

Carl gave his reasons: To save science. But I do not see that is necessary in any way, and I do not personally believe that that method is on the right track.

What is wrong with the word "know"? To my Western-trained mind, it conveys the idea that there is something in the external world (the world outside of my head and beyond my control) and that my mind contains a mirror image of it. It conveys the idea that the outcome to the contemplated measurement already exists "out there" in some deterministic or fatalistic sense. It conveys the idea that I really need never have a look to see if the outcome is produced by my measurement: It's already there, and I know it; why waste time on a measurement? Notice that I let the word "knows" stand when it came to describing the very logic of the Dutch-book argument.

What is right about "absolutely sure" and "absolutistic belief" for me? They convey the feeling that what I have in hand is a belief, an extreme belief to be sure, but nonetheless a belief. That phrase never reaches out to the external world for its justification—or, at least it seems so to me.

What is wrong with "maximal information"? I think it screams out no more clearly than in your concluding statement above. In words that Gary Herling might use: The very phrase "SAME maximal information" is an abomination of the English language. In my mind, information is much like the word "know" (though a little looser in constitution). It too conveys the idea of a mind or a newspaper mirroring aspects of a preexisting reality. Besides that, the very fact that we have to go to the trouble to use the word "same" in conjunction with "maximal" conveys the feeling that the word "maximal information" was never appropriate in the first place. If information is some stuff we have gathered from the world AND it is maximal—the very most one can get—AND two agents really should be gathering up the same stuff, else one of them is wrong, THEN why do we have to go to the trouble of using the word "same"?

Well, we use it (i.e., "same") to keep ourselves from contradicting the belief that probabilities are subjective after all. Fine, so that is a good reason to keep the word "same". BUT, it is a bad reason to keep the phrase "maximal information" to merely convey the concept that one is "absolutely sure" of the consequence of some action that one might take (i.e., the measurement being contemplated here).

Schackcosm 18 :

> CAF Said: Suppose you and I agree to everything in the world EXCEPT the quantum state for a given system.

It follows from what I just wrote that this situation cannot arise in the case of maximal information.

To me, that statement is a *non sequitur*. I cannot find any *logic* to bring it about. And I say that especially if you can agree to the validity of my attempt at expressing a "common ground" for us in the highlighted paragraphs above ... no matter how pedantic you think my actual phrasing is.

If the quantum state is not uniquely declared by some reality, then there is nothing to stop us from agreeing on some aspects of the world and disagreeing on others.

Please read the note I wrote Mermin titled "Intersubjective Agreement" again. If you and I (in the presence of each other) perform a given POVM consisting of rank-one projectors on a system, then you might say that we will agree on the system's state thereafter. But that requires the assumption that we BELIEVE the same quantum operation (for updating our states) will be associated with that measurement. If we don't agree on that at the outset, then we will come to conclusion of two different pure states for the system after the measurement is completed.

You say, well a quantum operation is surely an element of reality: It is either right or it is wrong. (Carl would say that in any case; I wouldn't.) But suppose it is so—I will relax my debate with Carl for the moment. How would we know which quantum operation we had? We would have to have prepared a load of quantum states beforehand to map which quantum operation is "really" there. But then we would have had to agree on our cache of exploratory quantum states in the first place. How did we get to that stage of agreement, I ask you? And, on I will do the same, ad infinitum.

The point is, in a world where our only exploratory tools are quantum states and quantum measurement outcomes, we can never terminate the chain. This is one aspect of what I meant yesterday when I said that quantum states are infused with beliefs from the get-go.

Quantum measurement outcomes alone will never, ever be enough to uniquely determine a quantum state. One has to have some further a priori information or beliefs to do that. You can play the game—as Carl wants to—that that a priori information is the world's Hamiltonian. But then you will be about as stuck as Kant was with his transcendental idealism: you will still have

to start off with agents of some initial common belief before they will ever be able to come to agreement about the Hamiltonian's form. And how are we poor finite beings to ever get to such a starting stage?

I say simply: throw out any trappings that a quantum state can ever be objective.

Schackcosm 19 : *Is there ever maximal information? Yes. You give us an example where C and F both obtain the same maximal information about the system:*

> CAF Said: We perform a maximally refined quantum measurement (a POVM with rank-one elements). We agree on the system's state thereafter.

In the sense that you want maximal information, i.e., something beyond absolutistic belief, I would therefore say that there is never any. My example required that the two agents share an almost strangulating amount of common belief. See discussion in previous section.

Schackcosm 20 : *As Carl writes, if C and F have the same maximal information, they must assign the same state. This is an important situation, as scientists share the information they have.*

If we can get past the language, I will (clearly) agree that this is an important situation. Scientists share the data AND the beliefs (interpretations, machine designs, etc.) they have.

Schackcosm 21 :

> CAF Said: The Dutch-book game is an adversarial game. Anyone whose intention is to make his opponent go bankrupt is NOT going to share everything he knows with him. He will be silent and bet his money.

No. The Dutch book game is about ONE agent's consistency.

Yes, you are absolutely right. And I apologize for throwing in extra junk that is not relevant. But the only point I really meant was that there is nothing in the Dutch-book set-up that forces the bettor and the bookie to share their information. That is an extra requirement if you want it. But it is a requirement that seems to me almost to give up the whole spirit of the Dutch-book situation: it involves no communication beyond the numbers p and x.

Schackcosm 22 : *That agents having access to the same maximal information must assign the same state is all we need for our paper.*

It seems to me, the only thing we need for our paper is the "common ground" statement I made above. I would not think that I need to say it again, but I'm getting pretty fearful of the phrase "maximal information." At the very least, I would like to start using it in a more limited sense or in a more limited way. Or perhaps in a vague enough way (for the present project) that I can worm out of it when I want to write future papers of my own without you two. (But this issue is likely to haunt us all the way through to the end of the RMP deal.)

All this email is starting to exhaust me, and it has certainly kept me from making this trip to Munich even resemble a vacation. I would like to draw it to a reasonable end soon. (But I do understand that that will require flexibility on all our parts, even me.) This may help my samizdat production, but it no longer feels like it is helping the rest of my life. I feel like I have hold of some important points that we were just too much in the "classical" tradition to recognize before. If we ultimately disagree, then we'll just have to do that, but I would rather not end up in that state of affairs.

If you have to make choices on what to do with your own time, please comment on my note titled "Note on Terminology" sooner rather than later. I fly out for Ireland Sunday morning.

08-09-01 *Negotiation and Compromise* (to C. M. Caves & R. Schack)

Let me tell you a little story I dreamed up while driving through the Austrian countryside today. It is based on one of the most annoying realities of my life: there are times when Kiki and I just cannot come to agreement. If I can use Rüdiger's words, there are times when I just think:

Schackcosm 23 : *A is a physicist who would bet his career on his state assignment. If he says B is wrong he means this in an absolute, very strong sense. He has examined all the evidence, and there is no doubt left. He is certain that B is missing some evidence. The Dutch book argument shows that A is certain that B's position is equivalent to handing over B's entire fortune. Wrong implies foolish, deluded. For A, B is a crackpot, and the circumstance that B is certain that A is wrong reinforces this position.*

But there are realities: Kiki and I are married; we share a bank account. And here and there, Kiki consorts with the Dutch.

What I am leading up to is that I think there is a place for Ben Schumacher's observation about a three-person Dutch book in our ongoing debate.

You two want to believe that there are god-given constraints on how much two people can disagree. I say there aren't. It'll be a miracle if we ever come to some consensus on this. But I have never said that there ought not to be reasons that two people might want to come to agreement ... EVEN when they have differing but, nevertheless, "maximal information." (I use the phrase "maximal information" despite my dislike for it in order to be sarcastic and to underline a further difficulty with the term in a moment.)

I am internally consistent; there's no Dutch bookie who can take me to the cleaners. And despite my feelings for Kiki's complete foolishness, I feel that she is internally consistent; there's no Dutch bookie who can take her to the cleaners (as far as she is concerned). But we will be in deep trouble if that Dutch bookie approaches us separately. (Being married, we report all our beliefs to each other.)

I can see two outs to this problem. The first one—which is less interesting—is that we make an effort to come to agreement by consulting the world. We make a measurement, and thereby, through its invasiveness, force the quantum system into a state we can agree upon. (Assuming, as I keep harping on, we can agree on the quantum operation associated with our measuring device.) But what if we have no access to the system of interest? What are we to do then?

I think we would have no choice but to, each of us, back off in the firmness of our beliefs. That is, we should agree upon a density operator that contains in its support both of our earlier ascriptions. We both give up some of our certainty is this process, but the upshot is that we no longer have probability one of becoming bankrupt.

So think about this: Two agents start out saying that they are absolute in their convictions about a some quantum measurement. But then the reality of their partner's stubbornness hits them, and the only thing they can do is back off.

You continue to want to call a quantum state information. But for the present case, again, it seems the term "information" is stretched beyond common usage by the factors people must sometimes take into account in coming to their assignments. From my point of view, Kiki's foolish quantum state assignment is not information about the physical world at all. It is completely wrong, with no reflection in the world as far as I am concerned. Nevertheless, I had better take it into account in making my bets if I don't want to lose our whole joint bank account.

The point I take home is that is sometimes better to negotiate and compromise even when one has "maximal information."

10-09-01 *Short Reply* (to C. M. Caves & R. Schack)

Just a very short reply to your latest posting. But first let me say something about this:

Schackcosm 24 : *I hope this email establishes a little more common ground. I would like to start, next week, on revising the paper in the view of this discussion and the referee's comments.*

Yes, do it. With the draft in hand, I'll be better able to see which statements make me feel like a liar and which do not. (Hopefully most of them won't.) And then we can be done with this, and then you can finally stop saying to me, "But not for this paper."

Schackcosm 25 :

> CAF Said: What is wrong with the word "know"? To my Western-trained mind, it conveys the idea that there is something in the external world (the world outside of my head and beyond my control) and that my mind contains a mirror image of it. It conveys the idea that the outcome to the contemplated measurement already exists "out there" in some deterministic or fatalistic sense. It conveys the idea that I really need never have a look to see if the outcome is produced by my measurement: It's already there, and I know it; why waste time on a measurement? Notice that I let the word "knows" stand when it came to describing the very logic of the Dutch-book argument.

I do not think any of this is clear. I "know" something means I have a firm belief in it. I don't think more is implied. The question of whether something corresponding to the knowledge (or the belief) exists out there is entirely separate from these wording issues.

Mermin once wrote me this:

> It seems to me Chris is getting much too subtle about this. I would talk about knowledge, not belief. I take "knowledge" to mean simply "true belief", a definition that as I remember goes all the way back to Plato and can be made unproblematic even in the quantum context. [Only a postmodernist would sneer at my saying this.]

And then this:

> C'mon, don't drag your heels. QM is sometimes capable of assigning probability 0 to certain outcomes. For those one doesn't have to argue about whether probability has to do with ensembles or degrees of belief or anything else. "True belief" seemed a good term to describe such outcomes, and then I remembered that Plato (I think) had used the same term (in contrast, as I remember, to "opinion".)

William James writes it like this:

> The popular notion is that a true idea must copy its reality.

The *Encyclopedia Britannica* says this (mainly the last sentences are relevant):

> In general, the philosophical tradition from the Greeks to the present has focused on the kind of knowledge expressed when it is said that someone knows that such and such is the case, e.g., that A knows that snow is white. This sort of knowledge, called propositional knowledge, raises the classical epistemological questions about the truth or falsity of the asserted claim, the evidence for it, and a host of other problems. Among

> them is the much debated issue of what kind of thing is known when one knows that p, i.e., what counts as a substitution instance of p. The list of such candidates includes beliefs, propositions, statements, sentences, and utterances of sentences. Each has or has had its proponents, and the arguments pro and con are too subtle to be explored here. Two things should, however, be noted in this connection: first, that the issue is closely related to the problem of universals (i.e., whether what is known to be true is an abstract entity, such as a proposition, or whether it is a linguistic expression, such as a sentence or a sentence-token) and, *second, that it is agreed by all sides that one cannot have knowledge, in this sense of "knowledge," of that which is not true. One of the necessary conditions for saying that A knows that p is that p must be true, and this condition can therefore be regarded as one of the main elements in any accurate characterization of knowledge.*

I put stars beside the hot stuff. [It is italicized in this samizdat version.] And I'd send you more quotes if I could, but I'm a gazillion miles away from home in a piss-poor dormitory room (with no library).

Schackcosm 26 : *There must be a misunderstanding. If you and I agree about everything in the world, we also agree on the certainty of a particular measurement outcome. We can not then disagree on the state to assign, because at least one of us would be internally inconsistent.*

You must mean that we agree on everything except the state AND the measurement.

Yes.

Schackcosm 27 :

> CAF Said: I now think it is much better to reserve the word KNOWLEDGE solely for the outcomes of quantum measurements once they become part of the mental makeup of an agent interested in them. I walk into Mabuchi's lab, and to the extent that he and I agree that he is performing some POVM (denoted by a set of positive operators $\{E_b\}$), it seems to me valid to call the outcome b we both witness to be an addition to our knowledge. Now, what either of us may do with that knowledge is a different story. One thing is for sure, it ought to cause both of us to update our beliefs.

This is a very difficult debate. I am not sure I understand why you draw the line between knowledge and belief exactly where you do. Why give belief in b a special role? I need to think much longer about this.

Because I had assumed that Mabuchi and I had agreed to a fixed "random variable." With respect to that prior assumption the thing we gain is "information" in the standard Shannon sense. I'm willing to call that knowledge that we did not have before. There is nothing personalistic about it; we both have gained the same thing.

I went to dinner with Jon Barrett, Harvey Brown, Matthew Donald, and David Wallace. Getting Bayesian ideas across to them is going to be a hard sell. But the most amazing thing is that each and every one of them was familiar with the Dutch book argument! (Now, how could anyone know it, and yet it not change their life?)

15-09-01 *Question on the Manuscript* (to R. Schack)

Just send me the draft when you're pleased with it. And I'll tell you then what I can tolerate.

Schackcosm 28 : *But as your (ugly) term "absolutistic belief" shows, they should be extremely careful before making pure-state assignments.*

I am not committed to that term, like you guys are to the (inaccurate) term "maximal information." It was just the best I could come with at the time. Change it if you wish. The most important thing is that whatever you substitute for it should carry no flavor of a "reflection theory" of knowledge.

You won't buy it, and you'll think I'm just saying this out of less than pure reasons, but you would be surprised at how many people have now encouraged me to call the quantum state a "belief" rather than "knowledge." (Four.)

I'd like to come to SB, but we'll have to see how things play out with Kiki.

15-09-01 *Melancholy Molly* (to N. D. Mermin)

I'm sorry to be writing you back so late: this week has taken a big toll on me.

Merminition 55 : *Well for better or for worse Todd, Jerry, and I came up with something we could all agree on.*

I suspect for the worse (if Renes's report to me on it is accurate). I haven't been able to download the paper myself yet, and won't until I get back into the states. My personal opinion is that it is likely to muddy the already troubled waters even more.

Merminition 56 : *Am still enchanted by the knowledge-belief breakthrough. How do your coauthors like it?*

The world has become a little lonely for me lately. I'll show you the lengths I've had to go to with all of you below. You are a troublemaker, getting me started down this route! (The document is in the format of the samizdat.)

Merminition 57 : *Are you now in Ireland? What part? Be careful.*

To think, I was once scared to be in Ireland. I'll instead take "be careful" to mean "get back into the US safely." I am in London as I'm writing this to you. My original return flight was cancelled; now I'm on the backlog with a million other people.

> Blow up your TV,
> Throw away your paper.
> Move to the country,
> Build you a home.
> Plant a little garden,
> Eat a lot of peaches,
> Try to find Jesus on your own.

16-09-01 *For Worse* (to N. D. Mermin)

Merminition 58: *Well for better or for worse Todd, Jerry, and I came up with something we could all agree on.*

Well, I managed to download your paper this morning after all. I ended up in the airport for over four hours. Now, I'm somewhere over the Atlantic.

I hate it, of course. It's built around the same stubborn disregard for the issues involved in trying to give the quantum state a NONontological status that all the BFM emails were.

To use a term I picked up from a philosopher this week in Ireland, the proposed outcome of a single quantum measurement simply cannot be a "candidate for knowledge." It's source must be considered ineffable, else the wrath of Gleason's theorem would strip away the nonontological status that was being sought for the quantum state in the first place.

I can guess from the format of the paper that it is being submitted to PRL. If I were being mean, I might say, "Good. That'll give it a higher chance of being rejected." But I'm not mean. If I were the referee I might even accept it. But, ONLY if the authors use the extra column at their disposal to explain why their statement of the problem is not a deep endorsement of the Everettista manifesto. (Or, if it is, then just come out and say it.) What else is Zeno but a baroque name for the quantum state of the universe (mixed or not) or a candidate for the one that really gets it "right"?

On a personal note: If you're going to cite a personal communication that appears in the samizdat, why don't you cite the samizdat? (I'm always looking for further ways to draw people into it.)

Best regards (in continuing disagreement),

16-09-01 *Persnickety Business* (to N. D. Mermin)

Since my last note, I've looked over your paper again. I must say I found the sentence,

> It is surely a significant feature of the theory that consideration of impossible outcomes and very little else leads, without any invocation of "the uncertainty principle" or "maximal information", to the fact that pure state assignments must be unique, as well as the more general constraint on mixed-state assignments.

a bit snobbish. Especially since you gave zero devotion to the issue of whether a quantum state could be "true" or not. If they can be "true," then why don't you just take them to be a bit of material reality and forget all this crap about knowledge? If they can't be, then what do you mean by the word "impossible"?

I still hate the paper, probably more so now.

A little further over the Atlantic (and wishing I were closer to Greenland),

16-09-01 *Arlo and Arlo* (to D. B. L. Baker)

As I just wrote another David—i.e., Mermin instead of Baker—right now I'm somewhere over the Atlantic, wishing I were a little closer to Greenland. I'm on my way back home from Northern Ireland. What a week it has been. I've never been so homesick for America before.

> Good mornin' America, how are you?
> Don't you know me, I'm your native son.

I think I might just kiss the ground if I really do land at JFK.

Once upon a time, we could actually see the twin towers from Morristown on a clear day. They pierced the horizon like nothing else in Manhattan. Hearing Emma's voice on the phone the other night was one of the toughest things. She said, "Hi Daddy-o," and then told me all about her first day in playschool. It's not easy to juxtapose that with the death of 5000 people and the many more deaths that may come in the near future.

Life is an essence in this universe. Creative, productive, reproductive life: I'm not one of those scientists who think it is just an illusion, an epiphenomena rolling on top of a clockwork or a dice-rolling world. It is something in itself. It was latent there all along, long before the trilobites had found their place, and it is our task to see that it does not return back to that latency.

Very sad times.

16-09-01 *Registered Complaints* (to J. M. Renes)

Good to hear from you; thanks for the note. I'm flying back from Ireland as I write this to you. I think the conference was successful in many ways. While there, I got to know Marcus Appleby, and he is a really good guy. He's very Jamesian in his perspective, very clear in thought, and a real seeker of the truth. I think I'm going to work to get him to Montréal next year (even though he does not practice quantum information).

I agree with your assessment of the BFM paper. I've already registered my complaint with Mermin. But you know, it's not like there are no pansies even in the midst of our own little group: Carl and Rüdiger have had no great fortitude when it came to this issue, no courage of their convictions.

I'll place my small samizdat on the subject below in case you're curious.

I hope your transition back to New Mexico has gone well. We'll miss you at Bell Labs.

16-09-01 *Nerve Therapy* (to R. Schack)

I'm in the last 1.75 hours of my flight to New York, and I'm doing everything I can to keep from getting too nervous. Let me finally turn my sights to your note from a week ago.

Schackcosm 29: *I was referring to the argument that, from A's perspective, if B assigned a different pure state, that would force B to accept a bet offered by A that amounts to handing over money to A. Therefore B is a crackpot from A's perspective (but not from his own, of course). This is interesting because it is the very fact that B has maximal information that forces him to accept A's bet: B "knows" A cannot know anything that would give her an unfair advantage in the bet. I still think this is a strong argument. Scientists would try to overcome this critical situation by questioning the world, i.e., by making measurements, but NOT on the system in question, but on everything else, e.g., Hideo's optical table. After all they both are convinced that the world is disentangled from the system in question.*

Personally, I only find this restriction to not touching the system of interest (by you and BFM) ad hoc. Since when in science do we restrict the experimentalists to never touch the objects of their interest? Besides, I think you may be missing one of the deepest facts of the quantum world: it has the power to cause agents to agree *henceforth*, no matter how adamant their previous beliefs ... AND EVEN without the underlying reality of preexisting measurement outcomes that the classical world had. With each quantum measurement, a part of the world starts afresh.

Schackcosm 30 :

> CAF Said: But there are realities: Kiki and I are married; we share a bank account. And here and there, Kiki consorts with the Dutch.
>
> What I am leading up to is that I think there is a place for Ben Schumacher's observation about a three-person Dutch book in our ongoing debate.

I cannot quite see where this situation offers any specifically quantum insight. In any case, it is completely different from the "adversarial game" I am talking about.

I'm not sure that it does lead to any specific quantum insight. But then again, I had never seen you and Carl be so adamant about taking probabilities to be objective features of nature before. (Don't do it! I know you're going to say "NO, NO, NO, we have never wanted objective probabilities!" But all the pieces of evidence—to me—point to the contrary.)

What it does show—and that is maybe where quantum theory comes in a little bit—is that there are times when no matter how adamantly I believe something, I should bet according to odds different from my belief. I.e., there are internal beliefs, and external "beliefs."

To that extent, I am starting to wonder if "belief" is even an appropriate word for capturing the essence of a quantum state. Instead it is starting to seem to me that it may more appropriately be described as a negotiated signifier to external action. I.e., in some cases, it signifies the betting strategies that a community of scientists can agree upon, even in the case of more refined and divergent beliefs.

Whose knowledge, Mermin asked? Sometimes, mine. Sometimes, yours. Sometimes it's more a matter of the policy we have been able to negotiate.

I do get a little worried in saying all of the above, in that, maybe I have not taken Savage's and Bernardo and Smith's views of probability into adequate account: namely, that probabilities and utilities spring into existence at the same time, and are a little inseparable. But presently I don't see how that fits this problem. Nor does it extinguish the problem that sometimes it really is in people's best interest to lie about what they believe.

Schackcosm 31 :

> CAF Said: I am internally consistent; there's no Dutch bookie who can take me to the cleaners. And despite my feelings for Kiki's complete foolishness, I feel that she is internally consistent; there's no Dutch bookie who can take her to the cleaners (as far as she is concerned).

I strongly believe that here marriage is fundamentally different from science.

Perhaps. But the point is, sometimes we bet according to situations that have nothing to do with our beliefs. We bet so as to obtain the best common good. And with that we do return to some aspects of science (and politics).

Schackcosm 32 :

> CAF Said: But we will be in deep trouble if that Dutch bookie approaches us separately. (Being married, we report all our beliefs to each other.)

This deep trouble arises even if both you and Kiki assign mixed states with the same support, I believe (but I need to think more about this). I am trying to make the point that pure-state assignments are different.

That is true. I chose the particular example of pure states to be as dramatic as possible ... in an attempt to hit you and Carl in a point where you were being the most hardheaded.

Schackcosm 33 :

> CAF Said: I can see two outs to this problem. The first one—which is less interesting—is that we make an effort to come to agreement by consulting the world. We make a measurement, and thereby, through its invasiveness, force the quantum system into a state we can agree upon. (Assuming, as I keep harping on, we can agree on the quantum operation associated with our measuring device.) But what if we have no access to the system of interest? What are we to do then?

Well, I gave my answer above. We question everything in the world BUT the system of interest. Using quantum measurements, of course.

Ad hoc.

Schackcosm 34 :

> CAF Said: I think we would have no choice but to, each of us, back off in the firmness of our beliefs. That is, we should agree upon a density operator that contains in its support both of our earlier ascriptions.

Yes, the same is true for two reasonable scientists. But as your (ugly) term "absolutistic belief" shows, they should be extremely careful before making pure-state assignments. Your way out is similar to having second thoughts after placing the bet.

No. My way out is to point out that there is, after all, a distinction between an (internal) belief and its manifestation as an outward bet. The belief comes before the bet.

OK, I've just passed Boston, and in the mean time, got another meal in my stomach. For the obvious reason, getting past Boston carries some symbolic significance. Let us believe in symbols.

18-09-01 *Hi Back* (to A. Peres)

Good to hear from. Yes, I am home. I was delayed by a day, spent a night in London, had to return to JFK rather than Newark, and had the most tense flight of my life, but I am home. I held and held and held on to Emma when I saw her waiting for me.

I have been in a huge email debate with David Mermin about his two latest papers. Consequently, I also got into a huge debate with Caves and Schack, though they are (slightly) more sensible. (The whole thing has been quite taxing.) John Wheeler once told us a story of a condemned man who, while waiting for the firing squad, calmed his nerves by contemplating Hamilton's beautiful equations. I learned a lesson from this: When I was worried about the safety of my flight, I calmed my nerves by continuing my email debate with Mermin, Caves, and Schack!

I hope you and Aviva are able to get home on schedule. I suspect your grandchildren are waiting for your return with excited eyes.

18-09-01 *Goodbye* (to N. D. Mermin)

I just don't know what more to say. Clearly there is something that is keeping us from communicating. I write more and more, and you just don't get my points. It's probably better if I just write less and less.

I'll record the way Caves put the best part of your paper the other day and just leave it at that. I've never said anything different, but maybe if you hear it from another voice something will click.

> It seems to me that there are various kinds of coming to agreement or inability to do so. Much of this is a recapitulation of what we have already discussed. It is phrased in a way that is supposed to avoid the notion that beliefs incorporated in a state assignment require the world to do things. [...]
>
> 1. BFM consistency (a multi-party condition): If the parties share their information, it is not ruled out that they can come to agreement on a common state assignment.
>
> If they share information, they must rule out all vectors in the subspace generated by the union of their null subspaces, and they must assign nonzero probability to all vectors in the intersection of their supports. They have the possibility of making a joint state assignment if and only if the intersection of their supports is of dimension 1 or more.
>
> Notice that we don't have to say here, as BFM do, that an outcome assigned probability zero by any party definitely cannot occur.

But let me say one last thing before I give up on this conversation. In your last note you wrote:

Merminition 59: *We've sent the hateful paper to Phys Rev A as an ordinary low-grade submission. My concern is that a referee will condemn it as trivial or well-known, not outrageous or Everittistan.*

You have probably summed up the referee's response correctly, but it doesn't make it so. Nor does it make you paper non-Everettistan after all. The whole point can be summed up with the following part of your paper.

> **A necessary condition for compatibility**
>
> Suppose Alice, Bob, Carol, ... describe a system with density matrices $\rho_a, \rho_b, \rho_c, \ldots$. Each of their different density matrix assignments incorporates some subset of a valid body of currently relevant information about the system, all of which could, in principle, be known by a particularly well-informed Zeno.

This is an edict you are placing ON TOP OF quantum mechanics. It is a construction of your own doing. I find it in no axiom system I have ever looked at. It IS a good half of the Everettista starting point.

You might say the other half of Everett is that he takes the quantum state explicitly to be an ontological entity. But you've got that too. By imagining that facts of the world (measurement outcomes) uniquely determine a quantum state, what else could a quantum state be but a stand-in for those real things, those FACTS (the stuff that one absorbs into one's mind and calls information)? You simply cannot say that quantum states MUST be consistent, if you want them to retain a nonontological status. The second you say they MUST be, then you have given up the game: states are then properties of the world, and this question of consistency was a waste of time from the beginning.

With this, I end the conversation: I give up.

19-09-01 *Morning Coffee* (to C. M. Caves & R. Schack)

Schackcosm 35 : *Yes, and this is great progress!*

Well, the note really is some progress. Let me just accept the gifts the Lord has given me.

I have two questions.

1) What does Peierls' consistency have to do with coming to agreement? I don't see it yet. It seems to me to be more a technical definition for the word "wrong" than anything else. Using it, I can say when you are "wrong" with respect to my beliefs, but that seems about it.

2) This thing you call fuchsian consistency. You bring to the surface, a point I've always been a little worried about. Why refuse to consider outcomes lying in the null subspace of a density operator? Let me give an example:

> Chris: I say the state is spin-up in the z-direction.
> Carl: I say it is spin-down.
> Chris: Spin-up!
> Carl: Spin-down!!
> Chris: OK, let's test it. Here is an ideal z-spin-measuring device. It's Kraus operation is just a von Neumann collapse. Do you agree?
> Carl: Yes, I agree.
> Chris: Are you sure you agree? We need some agreement or this won't be a meaningful game.
> Carl: Yes, I agree and am growing impatient.
> Hideo: Spin measurement done. It's spin-down.
> Chris: Damn, I honestly believed with all my heart that it'd be spin-up!
> Carl: Well, I wouldn't have said it if I didn't know it.
> Chris: OK, from here out, I say the spin is spin-down. I was a fool before, but at least we agree now.

Your worry seems to be that the formal apparatus of quantum mechanics will not allow me to propagate my belief. According to the Kraus rules, my new state should be the zero operator renormalized by a probability zero: i.e., it is an undefined object. Nevertheless, life goes on, just as the story above indicates. This, at least, does have a formal counterpart in that, if I back off in my belief even the slightest amount to any mixed state (not on the boundary), then the Kraus operation will take us both to the same place, to the same one-d projector. Consequently, the limit exists even if the actual point is undefined.

So, in saying this, I don't think one has to do anything fancy with a measurement to get this thing you call fuchsian consistency off the ground. No intricate constructions of states seem to be needed. Choose ANY (mutually agreed upon) von Neumann measurement you like, and Carl and Chris will have to agree at the end of the day. One of them will feel foolish for having been so wrong, but as long as he is rational, agreement can still be had. As Rüdiger said, the idea is that this is a property of quantum mechanics (in its capacity as a law of thought), not of the initial states.

Alternatively, one can turn the problem around, as I tried to do in my August 7 note to BFM: suppose Carl and Chris agree to a *particular* Krausian state change rule (one not even closely resembling a von Neumann collapse). Now one can ask, under what conditions on the initial states will C and C be left with more agreement at the end of the day than they started with? But that is a different problem than this f-consistency condition you speak of.

19-09-01 *Tentative Hello* (to N. D. Mermin)

Merminition 60: *I may take a shot at rewriting our argument, since I'd like to produce something for the Växjö proceedings. At that point I may try it out on you, but if you think it's likely to induce nausea, you don't have to respond.*

I owe you many things. If you want me to look at it, I will look at it, and I will try to respond (i.e., give you constructive criticism) in a civil tone.

19-09-01 *Practical Art* (to C. M. Caves, N. D. Mermin & R. Schack)

I suspect all of you have seen, in one form or another, the optical illusion where, looking at the drawing in one way, it appears to be a beautiful young woman. But looking at it in another way, it looks to be an old hag. Here's a true story about that. The first time I ever saw such a thing was in Roger Penrose's book *The Emperor's New Mind* in 1989 or so. Below the picture was a caption, explaining just exactly what I ought to see: alternatively, a beautiful woman and a old hag. But the oddity was, I could only see the beautiful woman. As much as I tried to find a hag in the picture, I couldn't do it. That fascinated me—so much so that, from time to time throughout the next year, I would pull the book off the shelf and try to find the old hag. I never did see her until one day I was searching through the book for something completely different and happened to come across the page. In fact, that may have even been two years after my initial encounter with the picture.

I think it is safe to assume from this that if Penrose had never pointed out to me that there ought to be an old hag there, I simply may have never seen her.

Let me apply this piece of art to a question on my mind. Let us imagine that some perpetrator has committed a dastardly deed to my family and left a note promising that before the end of her life she would do it again. Beyond this I have no clue of the identity of the perpetrator (or of her cause) except a sketch made of her by an eyewitness, who has now also disappeared from the scene. For intentional completeness, let us suppose there is simply no further evidence that I can lay my hands on—there is no more "currently relevant information," no deeper "trail of evidence." The carbon atoms laid out on the two pieces of paper—the note and the drawing—are the only links I have to the cause of the crime. The trouble is, in the case of the drawing, the carbon atoms sketch out the shape of Penrose's beauty-hag.

Now suppose an insurance salesman appears on the scene and is willing to sell me insurance against a recurrence of the crime precisely 30 years hence at such and such a premium. Do I buy it? That, of course, will depend upon the probability I ascribe to the crime's recurrence in precisely 30 years. What probability do I ascribe?

It will depend upon what I see in the picture. If I "see" the old hag, it will be one thing. If I "see" the beautiful woman it will be another. If I "see" there is an ambiguity, it will be still a third. But one thing is sure from this example, what I "see" is not completely dependent upon the pattern laid out by the carbon atoms. Part of what I "see" is dependent upon psychological factors that I *myself* may simply never have access to—things deep within my head, things determined when I was first toilet trained, things to do with the self-referential nature of consciousness, things that physics just ought to (and maybe even has to) leave alone. My beliefs (my probability assignments) after seeing the sketch are determined in part by something objective in the world external to me, but also in part by my previous, purely subjective beliefs. This example ought to make it clear that I cannot put all the weight of my posterior beliefs on the external world.

Here's my question: Where do you draw the line? More importantly, how do you draw the line? What is it in your view that gives the clicks of a quantum mechanical measurement a status that goes deeper than the example above? What makes the phrase "currently relevant information" more decisive in the quantum mechanical case? How does that "trail of evidence" have more power to eradicate my subjective pre-beliefs than any other? So much so, in fact, that one gets the feeling that we might even view the quantum state as determined solely by such a trail? You say, "Well, there the information conforms to a physical theory." But in what way does that change things? Can you pinpoint it? Can you make that declaration meaningful? Indeed, can you pinpoint what about the example above is not quantum mechanical? It had atoms, it had systems, it had probabilities.

I said I would not talk to David anymore, and here I am talking to him. I guess that just goes to show that that Mermin can steal your heart ... as can all of you.

20-09-01 *The Stopgap* (to C. M. Caves & R. Schack)

Attached is the latest draft. You will find two (and only two) changes.

1. I added the word "the" in front of "records."
2. I added two citations to myself. One at the beginning, one at the end. If you don't like where I put them, that's fine—feel free to change their positions—but I would like to have them somewhere in the paper.

Of course I am still troubled by the use of the phrases "maximal information" and "with certainty". I continue to think that they convey an image that is better left unconveyed, but I'm the odd man out here so I'll shut up. The main thing is that you left me enough wiggle room that I can defend my view of the quantum state when I need to. E.g., "Oh, I'm sorry, that was just a bad choice of language. The thing to keep in mind is ..." Rather than my having to say, for instance, "Yes, I believe we were wrong about that, but I couldn't convince my coauthors."

Another thing I guess I really didn't like—and this is only a new one for me, it's not something that had eaten away at me before—is the slogan "Gleason's theorem can be regarded as the greatest triumph of Bayesian reasoning." To say that is to imply that Bayesian reasoning IMPLIES Gleason's theorem. I don't think you mean that. I think what you mean is that it is one of the most valuable additions to Bayesian reasoning ever. But I won't cause trouble here: I know you both like the saying.

I think I read the paper very carefully again—more carefully than ever actually. I will live with its consequences, and I apologize for dragging my feet for three or four years.

Now, I just wish we three could come to agreement in our views of quantum mechanics!!!!!! So that I could look back on that day and say, "We few, we lucky few, we band of brothers ..."

20-09-01 *Praise, Folly, Enthusiasm* (to W. K. Wootters)

A million thanks for your encouraging letter of August 30! But also, please accept my apologies for not writing you back for more than 20 days. I had wanted to write you a rather long, contemplative note in response, and I kept waiting for the right mood. Somehow it just never came—first with my travels to Munich and Belfast getting in the way, and then with all the horrible events in the world in the last 10 days.

I certainly like aspects of this speculation of yours. Indeed, I wish you would write it down so we could all have a chance to think about it a more deeply. (Would you be willing to do that? You do have tenure now, and our individual lives are finite: we should never lose sight of that.) But of what little bit I understand of your ideas presently, they don't seem to have as much reciprocality or as much dynamism as I would like to think the world has. What I mean by this is that your ideas seem to carry a significant flavor of the Cartesian cut: the graphs take the place of res extensa, and the identifications take the place of res cogitans. The two realms—as I understood your explanation—don't really interact: The graphs are timeless and independent of the identifications we might make between them.

But, as you say, we are both speculating. And we both realize that. Here's the way William James put it:

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

Since reading this a month ago, I've been wondering what my temperament really is. What is the "potentest of all my premises"? I haven't completely tied it down yet, but I think it has to do with the idea that the world can be moved (from within), that it is malleable, that it is still under construction. That the future, for better or for worse, is not yet determined. And that this malleability—like the turtles—goes all the way down: there is no ultimate level that grounds it.

This, if it is, my "potentest of premises" has so far taken manifestation in the speculation that the components of the world are "sensitive" or "irritable." But I suppose that is part of the "impersonal reasons" that James speaks of—i.e., that they form not quite the whole truth of my motivation. The better truth is perhaps that the components of the world—the things that come out of the ways we carve the world up—are movable in a deep sense. They reach out and affect us in ways that we cannot foresee, and we reach out and affect them in ways that they cannot. And through that intercourse, birth arises in the world in a sense every bit as real as biological birth.

But I don't know how to make any of that more precise. It stands at just a program and a direction, but one I take seriously.

Your letter gave me courage, and with such speculations, believe me, one needs courage! How I wish we could get together more often to hash these things out, letting our disparate speculations refine each other. I feel deep in my heart that there is progress to be made—technical progress—it's

just a question of building a community with a critical mass of ideas, constructive opinions and techniques.

What are your plans for your sabbatical this year? In which direction do you plan to use your time? Will you be visiting IBM often? If I could get a chance to talk to you more often about the sublime side of physics, I'd surely take it.

By the way, let me draw your attention to a mistake I made in the paper you read. Howard Barnum brought it to my attention last week, and next week I plan to write a short comment on it and post it on `quant-ph` (before someone else does). It is in the derivation of the tensor product rule. For the most part the derivation holds, but I got sloppy in the very last step. I.e., by requiring the existence of a noncontextual probability assignment to the outcomes of local measurements (with one-way classical communication), one does indeed get that the probabilities are controlled by a linear operator on the tensor product of the two spaces. But, these assumptions alone don't get you that the linear operator ought to be a positive semidefinite operator. That requires more assumptions. In principle, one should be able to state those assumptions as a restriction on the correlations one can obtain by local measurements, but I don't quite see how to do it yet. (An easy way out would be to require that the linear operator give rise to positive probabilities for all measurements, local and nonlocal. But, that's kind of a dull answer after relying on locality for so much. I'm sure one can find a more interesting answer.) For instance, I'm not quite sure how to tackle the question of ping-ponging measurements in this framework. Or even whether that's the sort of thing that should be looked at for a natural restriction.

There's so much work to be done! But, it's our place to do it ...

20-09-01 *Pots, Kettles, and Frying Pans* (to N. D. Mermin)

More seriously. I think the line you are talking about is not the line I was talking about.

Caves has been writing me things like this:

Cavesism 20: *I do want to conclude with my obligatory diatribe against wholly e-mail exchanges. You think all your messages were perfectly clear, I think all mine were perfectly clear, Rüdiger probably thinks the same, but the evidence is that they were not. It's just not possible to come to agreement by e-mail, the reasons among others being that, first, questions arise in reading something after which further comments get devalued and, second, peripheral and main points often have their roles exchanged when a message is read.*

And ever more I am having to come over to his view on this subject. But, it is just so hard to give up my old email habits, especially since I've seen them become so ineffective of late. It's like a captain who just can't tear himself away from a sinking ship.

So let me say a few words in response to you. You should know I've got too much invested in the phrase "knowledge about the consequences of our interventions" to back out: it's part of my whole being. If you think that is what is going on with me, you are mistaken.

I do not deny: (1) that trails of evidence exist, and (2) that trails of evidence are created in part by our interventions into the world. What I deny is that those trails of evidence can ever uniquely determine a quantum state, even a pure one. That is what is at issue (with me at least), and that is what yesterday's note was referring to. (Somehow I get the impression that you saw something completely different in the note.)

A detector goes click. You write down that it went click; I write down that it went click. To that extent, the click is part of the objective world independent of us; it is part of the trail of evidence that you and Caves speak of. (Now, maybe the measurement apparatus itself isn't completely part

of the agent-independent world—somebody built it to begin with—but that is a different story.) The formal structure of quantum mechanics says that we must identify the click with an element in some POVM. Fine. I agree with that. (Presumably you do too.)

The issue is, which POVM? And which state-change rule associated with that POVM? Show me a place in quantum mechanics where you are told how to do that. I say it is a subjective judgment, just like the quantum state is. Or, more precisely, it is exactly *that* subjectivity that keeps our view of the quantum state as being subjective from being an inconsistent notion. In any given experiment, if there is a single POVM (and state-change rule) that is correct and objective, then so must be the quantum state.

Do you just not see the mapping problem here? If a POVM (and state-change rule) is an objective property of the interaction of two systems, then so will be the post-measurement quantum state ascribed to one of those systems. Now, if the quantum state is an objective state of affairs, why quibble about calling it "knowledge" or "information"? That would be just using two words for it that never needed to be invoked in the first place. If the quantum state is objective, then call it "quantum state" and be done with it.

If you can stomach it, try to read my note "Practical Art" again. It was meant to be a call to do some soul-searching. It was meant to try to persuade you that the subjective element can never be eliminated in a theory that makes fundamental use of probabilities. It was not meant to convey the idea that one cannot draw a line between systems and apparatuses. It was not meant to be a call to join the ranks of the Everettista.

Chris (on a rainy day—it always happens on a rainy day)

20-09-01 *Comment on Practical Art* (to C. M. Caves)

You still don't recognize that the difficulty is a logical one, do you? There is nothing squishy, postmodern, deconstructionist, or new age about the issue: if POVMs (von Neumann ones in particular) are nonsubjective, then so are quantum states. Period. You can't get around that. If you want to claim that quantum states are subjective judgments, then you have to accept that POVMs (and their associated state-change rules) are subjective judgments too. Else the post-measurement states that they give rise to would be objective.

I say this slightly better in my reply to Mermin. It is not that I am leaving the realm of science: it is the strict practice of the art that put me here. Assumptions $\longrightarrow$ conclusions.

Don't worry though: I'm not offended by your note. I'm just (continuingly) surprised by your immense rigidity.

25-09-01 *Further Comment on Practical Art* (to C. M. Caves)

Cavesism 21: *I was initially mightily annoyed by the tone and content of your message. My comment said nothing about and intended nothing about being "squishy, postmodern, deconstructionist, or new age." Moreover, your reply didn't address any of the points I raised, apparently because they were all just further examples of "rigidity."*

Let me reply to your last point first. Here's what happened: I thought I'd reply with a short and to-the-point comment first (about what I saw as the overriding issue), and then follow that up with a detailed reply to your particular points. But then time ran out before having to leave for a weekend trip to the beach. I certainly did not mean to ignore your points, and I certainly won't

(as time permits this week). It was just that I was banned from email for the weekend: I'm sorry I didn't warn you of that.

Now, as far as the postmodern business, I apologize for being oversensitive on that point. It had to do with this remark:

Cavesism 22: *It suggests to me that agonizing over the sorts of questions in your hag-beauty-insurance story is not going to help us understand what we're doing as scientists and, moreover, that it will lead our program into a place where no other scientists wants to go.*

This is (what I recall to be) the third time you have made such an allusion, i.e., that I am going places where no scientist ought to want to go. The only conclusion I could draw from this was that you were implying that the subject of this whole discussion is nonscientific. I hope you will at least understand that I might find that insulting. And my reply was an attempt to put a quick end to that train of thought. Over and over, I feel that I have been trying to make a simple point, and most importantly, a logical point—it just happened to be an unpalatable point. So, in the end I guess it seemed to boil down to both of us accusing the other of being unscientific.

I will write a (calm) set of emails replying to all the new issues you and Rüdiger have brought up in the next day or two.

03-10-01 *Kid Sheleen* (to N. D. Mermin)

What a busy couple of weeks it's been for me! I am sorry it has taken me so long to reply to you. But cars needed buying, lots of newspapers needed reading, lots of soul-searching needed doing (before canceling what would have been my fifth trip abroad this year), and Bell Labs needed some tolerance (with the transition from Brinkman to Jaffe as our Research VP).

Merminition 61: *Sorry. Didn't mean to go on for so long. Caves is right; this is a rotten way to have a conversation. No need to reply.*

Yeah, Caves is right in many ways. I've never had more frustration in an email run before. But then, I don't think I've ever made such a headlong transition in open view before either. Regardless, however, I have certainly benefited from this: I've never had to strive so hard to make a simple point clear, and I think that gave me a load more perspective on the issue than I would have come to by myself. Strangely, in a way, the whole affair has hardened me and made me confident in this new direction of thought.

Merminition 62: *so I'm sympathetic to what you're saying, but I worry that you're giving up "objectivity" on too many fronts.*

You think you worry! That reminds me of a dialogue in the old movie *Cat Ballou* (with Jane Fonda and Lee Marvin):

> Jackson Two-Bears: Kid, Kid, what a time to fall off the wagon. Look at your eyes.
> Kid Sheleen: What's wrong with my eyes?
> Jackson Two-Bears: Well they're red, bloodshot.
> Kid Sheleen: You ought to see 'em from my side.

But I don't think I've given up objectivity on too many fronts: If there's one thing for sure, it's that I don't want to go too far. The tenet I hold fast to is that there is something happening in

the world, something was happening before scientists appeared on the scene, and something will continue to happen (in one form or another) should we wipe ourselves off the planet next year.

The issue is, how does that something interface with us, to what extent can we grasp it, and how do we modify it by our very attempt to grasp it?

Let me try to reply to some of the points in your letter.

Merminition 63 : *On the other hand you did seem to be undermining the impact of the trails of evidence when you gave a central role to the question of whether your Lucent colleague really had the polaroid oriented the way you had been led to believe it was oriented.*

Hideo Mabuchi is a young professor at Caltech (probably the youngest); he won a MacArthur Fellow (i.e., $500K) last year too. He started grad school at Caltech at about the same time that I started up with Caves. You can see he's the smart one. We've been friends ever since we first met at the Torino meeting of 1994. He keeps the napkin where I first explained the Holevo bound to him; I mention him whenever I can (like in the NATO paper) because of all the pearls of wisdom he gave me.

Merminition 64 : *I'll grant you that that's something to worry about, but it seems to me on a different level from the characteristic quantum ambiguities,*

Three months ago, I would have thought it was on a different level too. But now it is clear to me that the two NECESSARILY feed in to each other. (Though the whole issue has been building in me for two years: See Samizdat, page 127, in a note to Caves titled "The Dangers of Probabilismo." I'll talk about that more in a minute.)

Merminition 65 : *unless you want to deal with your colleague and his polaroid on the same footing as the photons, whence my joke about your closet Everretism. (Please read on before concluding that I still haven't got the point.)*

I did read on—many times—and I think you've got some of the point, but not the whole thing. In particular, I still contend that Everettism is the antithesis to my point of view.

There is a sense in which Hideo and his polaroid are on the same footing as the photons (and I have always thought this). It is that in reasoning about them—i.e., in reasoning about what kinds of traces they will leave from my interactions with them—I am obliged to use the formal structure of quantum mechanics if I want to do the best job I can in that reasoning. But I don't think your remark in particular has anything to do with this.

Your remark seems to be more of the flavor: Chris says POVMs (even simple von Neumann measurements) are "subjective judgments," so he must mean that their outcomes are every bit as dreamlike and subjective too. Measurements that have no concrete outcomes? What else could this be but Everettism in disguise?

But there is a non sequitur there (if indeed that is the flavor of your reasoning). It is that you (or my caricature of you) think I am identifying—in a supremely steadfast way—the "click" that makes its way to our senses when we perform a measurement and the very being of a POVM.

Instead, the idea is that the "click" is REAL, as real as you could want anything to be (for those concerned, for those who know of it). But, the index b (from some POVM $\{E_b\}$ that we associate with it) is not the thing-in-itself. It just gives the "click" a NAME and a CONTEXT through which we can draw inferences and through which we can start to contemplate further reasoning. It is that identification which is an ultimately subjective element; it is not the "click" itself.

I had hoped to draw your attention to this distinction with the Penrose beauty-hag example, but I see I completely failed on that count (not only with you, but also with Caves and Schack). There is the stuff of the world—the "click" (in part). And then there is our description of it. The two things are not the same things; one lives in my head, and one lives partially outside it. To accept quantum mechanics, is to accept a template for that description and to accept a method of manipulating one's judgments thereafter.

Does any of this make sense? I think it is the main point I have been wanting to make to you, but as far as I can tell, you've ignored it.

Merminition 66 : *I agree that figuring out what measurements are associated with what operators (even at the von Neumann level) is something you have to bring in from the outside. So is knowing what the Hamiltonian is. I'm not so sure these are the same kinds of subjective judgments as a state assignment, as you maintain next. . . .*

The new thing I've been saying since August is that the identification of a particular POVM (and one element therein) with a measurement "click" is a subjective judgment. I had not clearly appreciated that before. However your mention of the Hamiltonian is apropos, because that one, at least, I had caught before. (That is what Samizdat, p. 127 is about.) There are several ways to see that "the Hamiltonian" must be on the same subjective footing as the quantum state. Here's one; I'll just quote you:

Merminition 67 : *I worry that "objective" is taking on too many different meanings here. For example, EPR makes it clear (at least to sensible people) that the polarization state of a photon is not an objective property of that photon.*

Will you accept that the existence of quantum teleportation carries as much force as the EPR argument that the quantum state is not an objective property of a photon? If you will, then can you tell me what the import of all the recent papers on "teleporting a unitary operator" is? (See for example `quant-ph/0005061`, but there are a plethora more.) But even if you don't, the argument is simple: Hamiltonians can be toggled from afar by our measurements on entangled states. (Carl gets himself out of this by saying that the only thing being toggled is the "effective" Hamiltonian, not the underlying quantum circuit, but I say where there is a tear in the fabric there is a rip.)

Merminition 68 : *I think you're saying that if the vertical alignment of the polaroid is an objective fact, then the state — "vertically polarized" — of the emerging photon is also an objective fact. But that's not the same as saying it's an objective property of that photon.*

That is right, there is a difference. And you have gathered what I was saying (almost) correctly. (I wrote "almost" to help remind you of the points above about identifying "clicks" with POVM elements.) The point is, the quantum state had better not be an objective fact, or the point of view that Caves and Schack and I have been trying to build up will be in deep trouble. What is wrong with taking a quantum state to be an objective fact, as long as one drops the insistence that that fact be localized with the photon? (I.e., as long as one does not make it a property of the photon where it stands.) At first sight, maybe nothing: I think that is probably the point of view you are trying to build; it is also the point of view Philippe Grangier has been trying to build in his recent `quant-ph`'s. But, on second sight, one cannot forget that the quantum state uniquely specifies a set of probabilities. If the quantum state is an objective fact, then so are those probabilities. And now it is on your shoulders to tell me what objective probability can possibly be. I won't stand for anything short of an operational definition.

Merminition 69 : *I worry that "objective" is taking on too many different meanings here. For example, EPR makes it clear (at least to sensible people) that the polarization state of a photon is not an objective property of that photon. It appears from the above that you believe you can only consistently take this position if you deny objective status to the outcome of the actual polarization measurement which enables you to predict the outcome of the measurement that has not yet been made.*

No. See the point above, where I used a little TeX notation. I accede to the objective status of something happening in a measurement intervention. I just don't accede to an objective status for what we decide to call it, i.e., for which POVM we decide to associate it with.

Merminition 70 : *(Forgive me, but this smells like a many-worlds strategy again. Your answer to the EPR paradox would seem to be to deny that the first measurement had an objective outcome. Recall Henry Stapp who has been saying for decades that only for the Everretista is nonlocality not a consequence of EPR.)*

I forgive you. But I hope you'll tell me that, with your newfound enlightenment, it doesn't smell so much like Everett anymore. (Your imagery conjures up my own imagery of walking on a warm day near a trash can full of lobster parts behind some coastal New England restaurant.)

Merminition 71 : *I'd prefer a middle ground which allows me to talk about objective facts but not objective properties. (It just now strikes me that one might call this correlations without correlata.) I've never been sure I can do this coherently, so I'm sympathetic to what you're saying, but I worry that you're giving up "objectivity" on too many fronts.*

I am in partial agreement with your first sentence, and I would like to think I have hit a sweet spot for that part. I'll give you objective "clicks" (though I might not call them "facts" ... but that's a story I probably shouldn't get into right now); I just stand fast against the objectivity of the quantum state. What is more middle ground than that? However, I do not share your aversion for objective properties.

It seems to me, quantum systems do have some properties that we can get our hands on. I usually preach the bundle of information-disturbance curves associated with a system, but let me try from a different angle to convince you of at least one property. I say that the quantum state cannot be an objective property because we can toggle it from a distance. I say that the Hamiltonian cannot be an objective property because we can toggle it from a distance. But what about a quantum system's Hilbert-space dimension d? Can you think of a way to toggle that number from a distance? I can't. And so, to that extent, I'm willing to call the raw number d an objective property of a part of the world. Now, what is the physical meaning of d? Well, that's why I struggle with all this information-disturbance stuff, but that's not the issue at hand. The issue is that one need not give up on all objective properties.

There are things in the world beyond our control: One them is the outcome of a quantum measurement, and one of them may be the dimensionality of a Hilbert space. Objectivity means nothing to me if it doesn't mean that some things are beyond my control, are beyond my whim and fancy. To the extent that I'm willing to say this, I don't think I'm giving up on objectivity on too many fronts.

Does this strike any chords in you?

PS. By now you should have received our modified version of "Making Good Sense." I won some good battles there: We no longer claim that two observers must be compelled to the same unique

state via a Dutch-book argument. But I lost some too. I continue to think the paper is misleading as hell, always talking about a "unique" state assignment and using the word "certainty" in a way that still troubles me. We were able to compromise only in that I thought things were now worded in a sufficiently vague way that I could worm out of them in my future talks and publications. I don't think we say anything factually against my beliefs, but the reader will have to be on his toes to not get fooled about where I really stand.

PPS. Here's another thing I ought to tell you. PRA made the mistake of asking me to referee the BFM paper. Despite what I wrote you earlier about probably accepting the challenge if it came up, I decided to decline the opportunity. I like the math of the paper, but I just could not agree with what you make of it. It seemed more appropriate to let some less tainted souls than mine tell you what they think of it.

03-10-01 *Replies on Practical Art* (to C. M. Caves & R. Schack)

Cavesism 23: *The philosophers tend to proceed by telling a story—reasoning by analogy, they call it. The actual problem is too hard for them to formulate, so they immediately introduce a simple analogy, reach a conclusion they like within the analogy, and then transfer the conclusion back to the actual problem, without ever justifying why the analogy has anything to do with the actual problem.*

Except for omitting the final justification—which is more than important—is this so different from what you teach in your physics classes? I.e., That one ought to try one's ideas out on a simple example first? One that may already contain the essence of the problem, before embellishing it with too many details?

Cavesism 24: *I enjoy reading your stories, but perhaps you're falling into the same sort of trap in a less obvious way. The difficult and very personalistic questions about assigning probabilities in your hag-beauty-insurance story are important in thinking about Bayesian probabilities. These personalistic factors are well known to be present in a subjective interpretation of probabilities, but do we really have to worry so much about them in the context of interpreting quantum probabilities?*

My point was to remind you guys and Mermin that these personalistic factors always must exist, else we would have no need to take such pains to talk about a "subjective interpretation of probabilities." If they are well known (as you say), then they should not be forgotten and replaced with purely objective "trails of evidence."

The point is, yes we must worry about them in the context of interpreting quantum probabilities. We must recognize that that is part of the very problem. Once we have recognized it, then we can move on and almost forget that the issue was ever there—just as one can do in whole textbooks on orthodox probability theory—but that first step is a supremely important step.

Cavesism 25: *You and others write papers every day where this party assigns this state and that party assigns that state, and I don't see any of these papers agonizing over difficult, personalistic questions of what state to assign. You're right to keep badgering us to pinpoint why this is true, but the fact that it is true—we don't worry about this kind of stuff when making quantum state assignments—leads me to believe that there is an answer.*

Nor do you see any sophomore-level textbooks on probability theory agonize over these personalistic questions when posing its exercises at the end of each chapter. On the one hand, you

completely missed the point I was trying to make, but on the other you also completely answered it.

The point is one does not have to worry about these personalistic questions to get quantum mechanics as a *calculational* tool off the ground. In that regard, the present issue is no different than with classical probability theory. Indeed, this is probably why in both theories a large sect of the practitioners have turned to "objective" interpretations of their main terms (alternatively, probabilities and quantum states) in such a misguided way. It is in the very recognition that personalistic questions exist, that one is compelled to finally get the foundations of the two theories straight.

In practice, what almost always happens? In the case of classical probabilities, when given a specific problem, one reduces and reduces the problem until one has transformed it into an equivalent problem for which one feels confident in making the uniform probability assignment. Thereafter one *derives* the probabilities for the problem of real interest by transforming and grouping, etc., until one rearrives back at the starting point. (This is a point I probably first learned from Rüdiger.) Think for instance, if I were to ask you what is the probability of obtaining a 7 or an 11 in a roll of two dice. Your mind would probably first jump to the judgment that all six sides of each die are equally likely, and then let the calculations flow.

Now of course, being a Bayesian, you would leave open the possibility for something else than a uniform assignment in that step above. But in practice, there are some things that most of us can usually agree upon ... and those are usually the starting points for textbook problems.

The issue is little different in quantum mechanics. When presented with a problem of calculating a quantum state for a given physical system, what do we usually do but reduce and reduce (or expand and expand) the problem until we come across an equivalent one for which we are confident we can predict the outcome of some measurement with certainty? Thereafter we work our way back just as before. Just think of Scully and Lamb's derivation of "the" quantum state of a laser. Alternatively, think about Moelmer's justification of the same state. [Indeed one might say that this is what the whole (worthless) decoherence program amounts to: deriving one subjective state from another and then thinking there is something deep about it. But that's an aside.]

Now just as before, being a Bayesian, one ought to leave open the possibility for something else than the particular pure state in the basic step of this derivation. But in practice, there are some things that most of us can usually agree upon ... and these are usually the starting points for textbook problems.

I think the similarity is overpowering. It is enough in both cases to recognize that ultimate personalistic issues exist, but then the homework assignment goes on—the student reduces the problem to a judgment few people would dissent on.

Cavesism 26: *It suggests to me that agonizing over the sorts of questions in your hag-beauty-insurance story is not going to help us understand what we're doing as scientists and, moreover, that it will lead our program into a place where no other scientists wants to go.*

Looking back over this note again, your language really was very scolding throughout—"trap," "badgering," "agonizing," "a place where no other scientists wants to go"—in spite of the fact that you warned me it would be "a constructive and gentle criticism." I understand that I am guilty of no less: There is no doubt that I can be arrogant and abrupt (and paranoid) at times. But in all this massive email, I feel that I have been providing a service, sharing ideas that I might not have if I didn't feel we should be brothers in arms. It became a little hard to gulp that all these notes might be viewed as little more than a nuisance.

Cavesism 27: *The answer might be as simple as this: we can only do science in situations where we scientists have agreed that such personalistic factors can be essentially eliminated, and quantum mechanics is the very pinnacle of this kind of situation. I think that's the content of our statement that "Gleason's theorem is the greatest triumph of Bayesian reasoning" and of our "principle of quantum indeterminism."*

I think the answer might just be as simple as that, but at a level higher than the one you are contemplating. The agreement we need is in accepting quantum mechanics as a method and a restriction for shuffling about our more mundane, everyday beliefs. What that entails is accepting POVMs as the structure of the questions we can ask a system and the Kraus state-change rule as our method for updating our beliefs.

Everyone keeps asking, what is the objective piece of quantum mechanics? I answered some of my beliefs on that issue in the letter I just sent off to Mermin (and then forwarded to you). But I think there is also quite a bit to be learned on the issue by first turning the question toward Bayesian probability theory. What is the objective piece of Bayesian probability theory? I think all three of us are in agreement that it is not in the particular probability assignments that one might make. But is there *any* objective piece at all? I think there is. Take Bayes' rule as an example. I would say that it is something objective in the theory: it is the ideal of behavior. If one doesn't use it, one can be taken advantage of. You agree that Bayes' rule is the ideal of behavior, and I agree that Bayes' rule is the ideal of behavior: it would remain the ideal of behavior if all of us were wiped off the planet.

Likewise, it seems to me, Gleason's theorem plays a similar role. There must be a sense in which accepting that the structure of our questions to the world (or, alternatively, our interventions upon it) conforms to the structure of the POVMs must be the ideal of behavior—something not so far removed from Bayes' rule itself. It is the ideal of behavior in the light of some crisp, physical fact. I don't know what that fact is yet (in any precise sense), but that does not stop me from seeing the outline of how the various structures in quantum theory should be classified:

measurements = POVMs	objective feature (physical fact)
Born RULE (via Gleason)	objective feature (an ideal of behavior)
Kraus state-change RULE	objective feature (an ideal of behavior)
quantum state	subjective judgment (always)
time-evolution map	subjective judgment (always)
Hilbert-space dimension	objective feature (physical fact)
particular POVM assignment	subjective judgment (always)
particular Krausian assignment	subjective judgment (always)

The list is not exhaustive; but I think these are the ones I see clearly at the moment.

The point is: Agreement required for science? Yes. Compelling interpersonal agreement as a (potential) statement about the agent-independent world? Yes. Agreement necessary at the level of quantum states? No.

Cavesism 28: *I'm not sending this to Rüdiger and Mermin, but you can send it to them if you think it's worthwhile to do so.*

Well, clearly I thought it was worthwhile to share my answers ... but there's that arrogance again. ;-)

With a smile and a conciliatory tone,

03-10-01 *Further Replies on Practical Art* (to C. M. Caves & R. Schack)

Cavesism 29: *I think we agree that there are things that are effectively facts in the effective reality of ordinary experience.*

In the words of Bennett's father (in such a context), "These are very deep waters." Since becoming enamored with James, Dewey, and Schiller—and having read copious (by my standards) amounts of them—I'm not completely sure how I should answer you. The issue is, I'm not completely sure in which sense you are using the word "fact." I have a feeling it is a more loaded sense than you would guess. But I don't want to get into that now: You suggested some simmer time, so I will leave you some until it becomes absolutely necessary.

There is, however, one thing I dislike about this sentence, and that is the phrase "the effective reality of ordinary experience." But you touched upon that very point in your note "More on Pots and Kettles"; so I'll say more to the issue in detail when I reply to that note. The main thing, though, is that I would say our ordinary experience is the rawest stuff around: It's the very stuff from which we build these super-smooth pictures by way of which we derive our further expectations. There is nothing effective about it: It is the stuff, it is the starting point. To use the word "effective" makes it feel secondary and derived (which is what you have been striving to get at, not me).

Cavesism 30: *The questions arise in what those facts tell us or, perhaps, in whether and what they compel us to believe. The argument is about pure-state assignments, not about mixed-state assignments. You believe that the subjectivity of pure states requires that it be possible for different agents to assign different pure states.*

Yes.

Cavesism 31: *To say something is subjective is to say that it exists only because of us and does not have an independent existence out there in the world. It also implies that different agents can disagree, the degree of possible disagreement being just the flip side of the degree of intersubjective agreement.*

Yes.

Cavesism 32: *Suppose we had the idea that facts in the effective reality force one to a particular pure-state assignment. The resulting pure state is then based on a trail of evidence in the effective reality and is embedded in each agent's mind. Is the pure state then out there in the world, independent of us?*

I would say, yes it is. The agent's state of belief is then an unneeded complication in everything under discussion. The fact is that there is a one-to-one correspondence between (sets of) facts and quantum states. You can say the agent's mind is nevertheless needed to "house" that state, but then, to me that looks to be nothing deeper than invoking the luminiferous ether to support the electromagnetic field.

Cavesism 33: *Do I have it right that this is the issue, or at least an issue?*

Yes. I have always perceived this to be the main issue. To the extent that I have said words all around this, it has been—I believe—to attempt to give different angles for viewing the same thing. Every time I saw you, Rüdiger, Mermin, Brun, etc., be reluctant to accept the point, I tried to present it from a different angle so as to be more convincing. I take it now that everyone only found that to be confusing. But what else could I do? And I can't complain too much, because I think the whole process has sharpened my presentation of the point (which I maybe only dimly perceived at the beginning).

Cavesism 34: *I don't necessarily see where the pure state is if it's thought to be out there in the world. The trail of evidence is not a pure state; we construct the pure state from the trail of evidence, but dogs don't and dinosaurs didn't.*

A one-to-one mapping is a one-to-one mapping. I do not see how YOU cannot see that making these statements is not a tacit acceptance that the quantum state is an objective entity after all. Maybe you have thought this all along. Namely, that when you said a quantum state is not a state of nature, what you really meant was that it was simply not localized on the physical system it is meant to describe. It is a state of nature, i.e., it is a collection of facts within nature, it is just not living on the system it's intended for. From this point of view, it's clear why dog's don't use them: Dog's aren't clever enough or advanced enough technologically to discover the true states of nature.

But I surely never thought this when I used the slogan, "a quantum state is a state of knowledge, not a state of nature." If facts can uniquely determine a quantum state, and facts live in nature, then a quantum state is a state of nature after all.

Cavesism 35: *The pure state is not out there in the system for the reasons we have long discussed: the system can't report its pure state, and a system's state can be changed to any pure state drawn from incompatible sets without ever getting close to the system. It looks to me like the pure state is purely in our minds.*

I'm not sure how this remark fits in. You might be making a call for me to consider putting the "trails of evidence" into the mind, but I'm not sure.

Cavesism 36: *You are insisting, I believe, that in order for a pure state to be subjective, it must be possible for different agents to disagree on a pure-state assignment. You say, I believe, that if we are forced to a particular pure-state assignment by the facts in the effective reality, then the pure state becomes objective.*

Yes.

Cavesism 37: *I don't know where to come down on this. It is one aspect of the question I always ask of which aspects of maximal belief get translated from realism to quantum mechanics. It also has to do with the nature of the "facts" in the effective reality and thus how the effective reality arises out of quantum mechanics (this is the content of Mermin's Pots and Kettles). My own take at present is that the effective reality is a form of intersubjective agreement.*

Fair enough that you don't know where to come down on this: I will try not to lose my patience any more.

Cavesism 38: *You are right in principle that nothing compels us to a particular pure-state assignment, but clearly wrong in practice. All our experience with quantum mechanics suggests that we have no trouble agreeing about pure-state assignments, so no matter how the facts in the effective reality arise, there is nearly total intersubjective agreement on what they imply for pure-state assignments (this is the content of my Comment on Practical Art and further comments on Pots and Kettles).*

I hope my previous note addressed this adequately. In contrast to what you say, I believe that I am right in principle and right in practice. You might have said the "all our experience" sentence about classical probability theory if your name were Richard von Mises. He would have said that all our experience with dice shows that we have no trouble agreeing that its outcomes are all equally probable. But you're not von Mises, and you've had the advantage of having had 75 years of good Bayesians clear the air for you. The issue you bring up has little to do with the structure of the physical world, and little to do with the structure of the Hamiltonians we feel compelled to describe it with.

Cavesism 39: *Let me know if I have got your position straight.*

I think you did.

Cavesism 40: *If I have, then it seems to me that we are not far apart, the only gap being how much we are willing to ascribe to the apparent agreement that exists in assigning pure states. You prefer to emphasize that nobody can be coerced into this agreement, and I prefer to emphasize that in practice nobody has to be coerced into it.*

A point of emphasis can make a huge difference in a philosophy. And a difference in a philosophy can make a huge difference in the practical and applied questions one might ask of quantum mechanics.

And I'm off to Lupé's for the best Mexican food in New Jersey. (You know that's not saying much.)

More tomorrow!

04-10-01 *Finicky Sins* (to C. M. Caves)

By the way, in saying this yesterday,

> There is, however, one thing I dislike about this sentence, and that is the phrase "the effective reality of ordinary experience." But you touched upon that very point in your note "More on Pots and Kettles"; so I'll say more to the issue in detail when I reply to that note. The main thing, though, is that I would say our ordinary experience is the rawest stuff around: It's the very stuff from which we build these super-smooth pictures by way of which we derive our further expectations. There is nothing effective about it: It is the stuff, it is the starting point. To use the word "effective" makes it feel secondary and derived (which is what you have been striving to get at, not me).

it dawned on me afterward (on my drive home) that I was committing a sin: There was a time when I liked the phrase. As you know, I commandeered it when writing my *Physics Today* articles with Asher.

So, maybe I was being overharsh, or maybe just finicky. In any case, even when I used it unreservedly, I know that I had a distinct flavor of the phrase in mind from the way you had been using is. [See our discussion starting on page 133 of the Samizdat.]

Maybe I'll say more about this later today.

04-10-01 *Replies on Pots and Kettles* (to C. M. Caves & R. Schack)

This letter is going to be a hard one to reply to, because I don't quite see how much of what I have said in the past led to the points you make here. So, let me just plunge into the thing and see what comes out.

Cavesism 41 : *I thought you were in the camp that holds that the our experiences—our actions or interventions and our perceptions of the world's response—are primary*

I thought I was too.

Cavesism 42 : *and that the function of science is to account for them.*

It's this part of the sentence that I'm not so sure of (though it's not clear to me exactly what you have in mind). If our experiences are primary, then it does not seem to me to be within science's purview to account for them. I believe, instead, the view I have had for quantum mechanics for some time is best mimicked by these words I picked up from William James last month:

> Metaphysics has usually followed a very primitive kind of quest. You know how men have always hankered after unlawful magic, and you know what a great part in magic *words* have always played. If you have his name, or the formula of incantation that binds him, you can control the spirit, genie, afrite, or whatever the power may be. Solomon knew the names of all the spirits, and having their names, he held them subject to his will. So the universe has always appeared to the natural mind as a kind of enigma, of which the key must be sought in the shape of some illuminating or power-bringing word or name. That word names the universe's *principle*, and to possess it is after a fashion to possess the universe itself. 'God,' 'Matter,' 'Reason,' 'the Absolute,' 'Energy,' are so many solving names. You can rest when you have them. You are at the end of your metaphysical quest.
>
> But if you follow the pragmatic method, you cannot look on any such word as closing your quest. You must bring out of each word its practical cash-value, set it at work within the stream of your experience. It appears less as a solution, then, than as a program for more work, and more particularly as an indication of the ways in which existing realities may be *changed*.
>
> *Theories thus become instruments, not answers to enigmas, in which we can rest.* We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid.

Science does not account for our experiences. Science builds on them and gives us a structure by which to imagine pushing them to a new extreme. This is why I have laid such emphasis on calling the world "malleable" (for instance in my "Activating Observer" document that I shared with Rüdiger ... and maybe you too, I can't remember). It seems to me, science does not say so much about what is, but what can be (subject to the limitations to our actions captured in the very structure of the given theory).

Cavesism 43: *Trouble is that when our interventions proceed to too fine a level, the world's response is not deterministic and, furthermore, cannot be described within the realistic language of ordinary experience. Surprisingly we find that we can use the strange, unrealistic formalism of quantum mechanics to describe the intrinsic indeterminism that intervenes between our actions and our perception of the world's response.*

What do you mean by "too fine a level?"

Cavesism 44: *I thought you were ascribing some sort of objective or intersubjective reality to our primary experiences.*

Pretty much. Or, at least that's what I thought too.

Cavesism 45: *I thought the difference between you and me was that I think that we must somehow derive from quantum mechanics—or, at least, make consistent with quantum mechanics—the apparently realistic features of the emergent "effective reality" of ordinary experience, whereas you think this is unimportant, thus accounting for our different reactions to the decoherence program. But you now seem to be demanding much more than I do,*

I've always thought that I've demanded much less than you, and I don't think I've changed my tune on this account for several years. For the view I dream of constructing, what is the classical world? It is a world for which the agents describing it are full of ignorance and the best to which they can muster is a lot of imprecise control.

I look out at one of the trees outside my window, and I ask how I might capture everything I'm willing to say about it into a single density operator. I can't say much about that, but I'm willing to bet that if I would carry the project through, what I would end up with will be so mixed, so thermal, that it'll be just about commuting with anything else I might have come up with, even if I had stared at the tree a little longer. This little fact—it seems to me—has nothing to do with the Hamiltonians of the world (as if they were objective things). It is a function of my pure ignorance and my unwillingness to tear the tree apart and refine my beliefs.

The idea toyed with here is that it is just ignorance, no matter how we each walk into the room with it, that leads to the classical world. If I am so ignorant as to use an almost commuting set of density operators for a given object, then any (gentle) attempt you—as another scientist—may make to refine those beliefs will not cause my beliefs to be any less valid: Your information gathering, will not cause a disturbance to my description. And therein—the speculation goes—lies the essence of classical mechanics.

Cavesism 46: *that before we even start, we be able to explain exactly how the effective reality works and at what point it emerges. As David points out, this is exactly what the Everettistas demand.*

As best I can tell, this remark can only come from viewing my program (more accurately, my dream) from your philosophical predispositions. I would never demand that we "explain exactly how the effective reality works and at what point it emerges." The classical world comes first. Quantum mechanics (as a theory of inference) extends beyond it, by taking into account new phenomena that simply can only be seen when working in a regime of less ignorance.

A relevant ditty to read might be my essay "Always One Theory Behind" on page 464 of the Samizdat.

Cavesism 47 : *Of course, after making the demand and finding present responses unsatisfactory, you and the Everettistas go in quite different directions. They, out of an anal need for naive realism, simply concoct a naive realism to go with the state vector. You certainly aren't going in that direction, but being risk averse and already burned, I'm not going to risk a description of your direction here.*

I loved the phrase "anal need."

Cavesism 48 : *To your credit I think you won't claim to have gotten your ideas worked out entirely (perhaps the rest of us can be allowed some access to that defense). Still you might want to think about the road you're traveling on and how it relates to this question of taking as given the apparently objective experiences of our daily lives.*

Thanks for the credit. It's refreshing to be complemented for not acting like a guru with all the answers. People at foundations meetings will have none of that. It's been my experience that they demand you tell them exactly what reality *is* ... before they quickly tell you you're wrong. (Matthew Donald told me he couldn't take me seriously as a foundations researcher because I keep evading the question of what reality *is*.)

Anyway, in conclusion, give me some feedback: Did I answer anything that you wanted answered? (I sure hope I did: I'm trying.)

04-10-01 *Replies to Morning Coffee* (to C. M. Caves & R. Schack)

Schackcosm 36 : *Oh no, I thought we had reached some agreement. One problem is that Kraus operations (unless they are 1D projectors) will not bring you to a unique place. Which means that qm does NOT provide a universal rule of coming to agreement. You need some ad hoc assignment.*

Yes, I had always understood that. I am sorry if I was sloppy about expressing it, but I thought I had always emphasized that there are two things that one can contemplate: 1) if Alice and Bob have complete freedom to choose what measurement they might perform, and 2) if instead they have at their disposal some fixed measurement (perhaps not of their choosing). In the later case, only certain initial states for Alice and Bob will lead to further agreement after the measurement. (See, for instance, my note to Mermin and company dated August 7.)

Schackcosm 37 : *The other problem is that your conversation is far too playful. State assignments are compilations of betting odds. They are COMMITMENTS. Chris in your dialogue should have been deeply shaken. He would have betted his house in New Jersey on this outcome to be impossible.*

Yes, perhaps. But, on the other hand, there is a counter trend in you that troubles me. And that is the basic philosophy that comes across as the message, "Once a quack, always a quack." What I mean by this is, suppose I ascribe a pure state $|\phi\rangle$ to some system, whereas you ascribe $|\psi\rangle$. As we have laboriously teased out of this correspondence, from your perspective, I am simply wrong. My judgment is not to be trusted (from your perspective). This much we agree upon. But I sense that you want to conclude more: Not only am I not to be trusted in my conclusions about the given system, but that I am not to be trusted about anything. You conclude that I am truly insane just because I adamantly disagree with you about one thing (as captured by our differing pure states). I say that goes too far.

I tell too many stories, but here is a true story. In discussing the cardinality of the natural numbers versus the even numbers, Kiki will accede that there is a one-to-one and onto mapping

between the two sets. Nevertheless she contends that there are more natural numbers than even numbers. I have never had more annoying conversations than the one we revisit about once a year on this subject. I simply cannot convince her that she is not being logical on this issue. But still I do find that I trust her judgments on other issues.

A "misstep" on a quantum state (even a pure state), it seems to me, is not the end of the world precisely because of this.

Granted, I am a bit confused on what I think the ascription of a pure state actually does capture, but I think making it carry the weight of an agent's rationality or irrationality goes too far.

Schackcosm 38 : *Yet another comment: You have said nothing in all your notes (to my best knowledge. . .) that tells me why this situation is different from Chris being certain that there are two chairs in this room, and Carl with Hideo's help convinces him that he was wrong. I'd say either Chris was tricked, or he had hallucinations. Todd said: "This is why we say that insane people 'out of touch with reality' ". I said the same thing in a different way.*

Since you ask this question more pointedly in another note, I'll wait on answering it until I get there.

Schackcosm 39 :

> CAF Said: So, in saying this, I don't think one has to do anything fancy with a measurement to get this thing you call fuchsian consistency off the ground. No intricate constructions of states seem to be needed. Choose ANY (mutually agreed upon) von Neumann measurement you like, and Carl and Chris will have to agree at the end of the day. One of them will feel foolish for having been so wrong, but as long as he is rational, agreement can still be had. As Rüdiger said, the idea is that this is a property of quantum mechanics (in its capacity as a law of thought), not of the initial states.

I found the idea quite attractive that in a quantum world, differences can be resolved (using a well-chosen measurement) that would be impossible to resolve classically. I thought that this was what you had in mind.

As far as I can tell, no physical statement (no ascription of a phase space point) is impossible to resolve classically. What is different is that quantum mechanics can do that even without the preexistence of a phase-space point . . . and that surely is a property of quantum theory. But I said I'd come back to this in another note.

04-10-01 *Replies to a Conglomeration* (to C. M. Caves & R. Schack)

Now let me reply to a conglomeration of notes from you two.

First to Rüdiger:

Schackcosm 40 : *I remember you writing something to the effect that the click in a measurement is the closest thing to a fact one could come up with (sorry for not looking it up, but you write TOO MUCH).*

Did the reply I wrote to Mermin yesterday make any sense to you? I am now in the habit of drawing a distinction between a "fact" and a "proposition." The difference was not so important classically, but I now think it is paramount quantum mechanically. The fact (or consequence of our intervention) is the raw, uninterpreted, unclassified stuff of the world. It is the real stuff that makes its way to our senses and then to our brain to be pondered. The proposition, on the other hand, by its very nature attaches a meaning to the fact and, as such, is a subjective judgment. What this means in the quantum case is that to say there is a "click" is one thing: Presumably that is not a subjective judgment if I say it, and Steven says it, and everyone else we talk to says it. However, to say that that means the particular outcome E_b occurred in the POVM $\{E_b\}$ is to lay down a proposition, a subjective judgment.

The reason we could get confused in the classical case, and think that a proposition was more than a subjective judgment is because in the classical case, propositions don't entail probability assignments.

This distinction I'm drawing is not so different than the one Marcus Appleby draws in criticizing the Meyer-Kent-Clifton "nullification of the Kochen-Specker theorem." See his paper, `quant-ph/0109034`.

Schackcosm 41 : *I am afraid that I still don't know precisely where you stand, despite of your effort at explaining.*

Let's start from the classical notion of certainty. Let's consider the case where a physicist is certain that some outcome will not occur. Dutch book consistency implies that the outcome will be in his nullspace. That's the quantum part.

Do you agree?

Yes.

Schackcosm 42 : *1.) Carl is certain that up will occur and Chris is certain that down will occur.*

2.) Carl is certain that there are three chairs in the room, and Chris is certain that there are two chairs in the room.

In both cases their beliefs are contradictory in the same, classical sense.

Do you agree?

No, I do not think the statements have the same meaning. In the first case, in order to find out which of us is "right" and which of us is "wrong" we must elicit the world to produce something that it didn't contain beforehand—namely, the result of the measurement. In the second case, we can go blissfully along thinking that one of us is "right" and one of us is "wrong" simply because the world has something in it that one of the two of our brains is mirroring correctly.

There is a difference. In the classical world, reality is the ultimate arbiter of truth. In the quantum world, where we are fairly convinced that "unperformed measurements have no outcomes," we are actually lucky in a way that there is still an arbiter of agreement—we just can't identify it with a preexisting reality. It seems to me this is a feature of quantum mechanics: The theory can still bring us to agreement even without a preexisting truth value for our propositions. One might have imagined a more malicious world where we would not have even been able to rely upon that.

Schackcosm 43 : *If we make claims, we are COMMITTED to those claims (e.g., via betting behavior implied by the claims). I believe that starting from the notion that different scientists (different agents in the same linguistic community) should not have contradictory beliefs is eminently reasonable. To throw this notion over board, one needs excellent reasons. I enjoy playing with the idea, but I am far from converted.*

This hits upon what I wrote to Carl yesterday. The gulf that separates us seems only to be in where we think this agreement must be applied to get the engine running. I say both of us accepting QUANTUM MECHANICS as a structure is good enough. (I.e., accepting the theory is our common agreement.)

Now to Carl:

Cavesism 49: *Your point, as we see, is that we can think about life going on after finding a result deemed to be impossible. As you point out, there is a limit (add on the null subspace with epsilon eigenvectors, get a result in the epsilon subspace, update, and then take the epsilon goes to zero limit) in which we can think of updating a state assignment based on outcomes in the null subspace. But I think this misses the point. This isn't updating a prior belief. It's realizing that your prior belief is full of it and abandoning it in favor of life going on, as you put it.*

Moreover, the really nice distinction between classical and quantum Fuchs consistency is lost if we adopt your point of view. If we adopt your way of formulating Fuchs consistency, then it has no content either classically or quantum mechanically.

I'm just repeating myself now, I but I don't see that as contentless at all. In fact, though the effect is the same in both theories, the content is quite different across the two of them. In the classical case, we can always "pick up the pieces" as you say, by realizing that there is something really there and just checking what it is. In the quantum case, we can always bend the world into something we *will* agree upon, even if we violently disagree upon the meaning of some subset of our previous interventions.

Here's the way, Josiah Royce put it in a letter in 1888:

> Thus called upon to explain amid the trade-winds, and under the softly flapping canvas, the mysteries of [quantum mechanics], I put the thing thus: "There was once a countryman," I say, "from Cape Cod, who went to Boston to hear Mark Twain lecture, and to delight his soul with the most mirth-compelling of our humorists. But, as I have heard, when he was in Boston, he was misdirected, so that he heard not Mark Twain, but one of Joseph Cook's Monday Lectures. But he steadfastly believed that he was hearing Mark. So when he went home to Cape Cod, they asked him of Mark Twain's lecture. 'Was it *very* funny?' 'Oh, it was *funny*, yes,—it was *funny*,' replies the countryman cautiously, 'but then, you see, it wasn't so *damned* funny.' Even so, Captain," say I, "I teach at Harvard that the world and the heavens, and the stars are all *real*, but not so *damned* real, you see."

Cavesism 50: *The parties can always come to agreement, no matter what their state assignments, simply by getting amnesia regarding their prior beliefs and then picking up the pieces in the only way they know how.*

But there's really more to the story. They can always come to agreement, indeed—regardless of how disparate their initial opinions—if they are willing to make an essentially infinite expenditure toward laboratory technique. That is to say, the only thing that will give assured agreement in all cases is a set of Kraus operators all of rank-one. Jacobs and I called those infinite strength measurements: the idea being that they are hard to actually do. In more real-world measurements, where the operators are never really rank-one, coming to final agreement will generally require some initial agreement. Whence the point in my August 7 letter to Mermin.

Cavesism 51: *I believe that Fuchs consistency is about coming to agreement in the light of the outcome of an agreed-upon measurement where no party has to abandon his prior beliefs (certainly one has to agree that this is a legitimate case to consider). It could be that all parties are dumbfounded by the result, but let's put that case aside. For all other outcomes, the point is that all the parties be able to come to agreement by updating their prior beliefs. This imposes a strong constraint classically—all parties must have the same support—but appears to be no constraint quantum mechanically. That's an important distinction, it seems to me.*

I do agree that that is a legitimate case to consider. What I am not seeing presently is that its study will shed some foundational light. But I think I'm open-minded on this one: I might be convinced yet; I just don't see it now.

08-10-01 *Larger, Smaller* (to J. Summhammer)

Thank you for your wonderful, thoughtful, long letter! I have now read it several times, and each time I think I've gotten a little more from it. Thank you also for your concern over my family and associates in light of the September 11 attack: As far as I know, all my friends, and my friends' friends were left unscathed physically. But it is all a very frightening affair, and it is certainly weighing on everyone's life on this continent and the world.

Concerning the content of your letter, let me especially thank you for the large number of YES's you wrote into the margin of my paper! Let me make a couple of small comments on your one NO.

Summhammerism 3: *It appears to me that quantum theory is the correct way of reasoning, and classical probability theory is a certain limit of it. But both spring from the SAME way of reasoning. So far, physics has stood in the way of clarifying this. The perennial talk of systems and properties of systems one is forced to carry along when dealing with quantum theory is a real hindrance to clear thought. Remnants of mass points, forces, fields, etc. always sneak in, and with it the need to allude to an objective world out there. As if repeatably detectable structures in the statistics of probabilistic events and their efficient description weren't objective and "out there" enough (to me mathematical truths and the Himalayas are equally "out there"; the former are mastered by acts of mental climbing, the latter by acts of physical climbing, but both require willful action to be conquered. You call it free-standing reality.)*

This issue has now come up in my email a couple of times since the Sweden meeting. Here's the way I put it to Jeff Bub on the last round:

> The main thing is that it sounded like a good opportunity to pound out the similarities and distinctions between our points of view on quantum mechanics without being interrupted every three minutes.
>
> I know I suggested I would write a longer letter soon, but I'm going to wimp out of it again for now. It would concern the main point of distinction I see between us (and also between myself and Pitowsky). Namely, A) that I view a large part of quantum mechanics as merely classical probability theory (which on my view may be an a priori "law of thought") PLUS an extra assumption narrowing down the characteristics of the phenomena to which we happen to be applying it to at the moment, while B) you are more tempted to view quantum mechanics as a *generalization* of classical probability theory (and with it information theory). I know that my view is not fully consistent yet, especially as I have always distrusted mathematical Platonism—which you pointed out

to me I am getting oh so close to—but it still feels more right (to me, of course). Ben Schumacher, Rüdiger Schack, and I had a long discussion on this (on a long walk) the day after the round table, and I'd like to record that too. Ben took a stance quite similar to yours, and maybe even Rüdiger did too (despite his overwhelming Bayesianism). So, I may be the lonely guy out on this. And my view may be subject to change.

To some extent, I can understand both motivations, i.e., to see quantum theory as the larger of the two structures, and alternatively to see it as the smaller of the two. My thoughts are not completely set yet about which direction is the best direction, but let me try to explain a little about what I mean by probability theory possibly being a larger structure than quantum mechanics. (As evidenced in my paper, this is certainly the direction I lean most toward presently.)

Consider some physical system, say my house. And consider some set of questions you might ask about it. For instance, what color is it? (The answers being R, O, Y, G, B, I, V.) Or, what kind of flooring does it have? (The answers being wood, tile, vinyl, or carpet.) On so on: Consider every question you might ask about it. If you were a Bayesian, you would not hesitate taking all the information you know about me and applying it to the construction of a probability function for the outcomes of each such question that could be asked. For instance, if you had gained the impression in Växjö that I am a sentimentalist, you might place a higher probability on my floors being wooden than otherwise.

However it is also part of the Bayesian creed that there is no such thing as an invalid probability assignment. If there are no logical connections between a set of questions, then there are no constraints on the probability assignments I might give for their potential answers. So, for instance, though you might put a peaked distribution on the answer to the question about my floors, you might put a flat distribution on the colors. And so forth, for every elementary question that might be asked about my house.

However, when we come to quantum mechanics something changes about this. Now, the elementary questions correspond to POVMs. But, using Gleason's theorem, we are no longer free to assign probabilities to their outcomes willy-nilly. All but a very few probabilities assignments are tied together via the existence of a density operator. For instance, viewing quantum probabilities as Bayesian probabilities, one is completely free to assign any probabilities one deems relevant to the outcomes of a σ_x, σ_y, and σ_z measurement. However, once that is done, one is no longer free to specify an arbitrary assignment for spin in the n direction, for any other n.

From this point of view then, quantum mechanics allows only a subset of the vastly larger set of probability assignments one might make to the answers of the physical questions one might ask. And one might think that restriction is accounted for by some physical fact—the yet-to-be-discovered fact that is the essence of quantum mechanics.

Summhammerism 4: *Still, it is in this connection that I wrote a NO into your paper. On p. 28 you say "Probability theory alone is too general of a structure.", and at some other place you say there must be an input from nature. Based on my own games with these questions I doubt this. I think quantum theory is already contained in the basic notions that lead to probability theory. The sum rule of probability is no obstacle, if you ponder what mutually exclusive" means from an operational point of view. For this reason I see a valuable contribution in Lucien Hardy's attempt of starting from a few axioms, although an axiomatic approach is unsatisfactory as long as the axioms aren't simple truths instead of formal assumptions.*

So, indeed, what I said above is what I meant as an input from nature: It is whatever binds us to Gleason's theorem. (Gleason's theorem being the string that ties all the various distributions for a physical system together.)

You'll note actually that Hardy is almost an antithesis to this idea. He starts with structures that are larger than both classical probability theory AND quantum mechanics. By adding an extra postulate he can narrow it down to either one or the other (or any of a number of other structures). What I want is start with classical probability and then narrow it down to quantum mechanics.

It could be the wrong direction, but it is the one that feels predominantly right to me (and the one that seems to me to have the highest probability of leading to some interesting physics). It is a subjective judgment of course, but that's all that each of us has.

By the way, Caves and Schack and I have been thinking about applying for a visit to the Erwin Schroedinger Institute next spring or early next summer. The plan would be to write (the bulk of) an RMP article on all this Bayesian business while we're there. It'd be great to have your ear to test it out on, if we do follow through with the plan.

09-10-01 *Writing Physics* (to N. D. Mermin)

I'll place the new supplement to the Samizdat in the next email as plain text. Please let me know whether you're able to TEX it up fine; if that doesn't work out, I can post it on my web page (as a PostScript file) as I did before.

Collecting it up, it's hard to believe I've written this much in the little time since Växjö. I guess it's been an active time for me. I think there's no doubt that I've gone through a phase transition. For all my Bayesian rhetoric in the last few years, I simply had not realized the immense implications of holding fast to the view that "probabilities are subjective degrees of belief." Of course, one way to look at this revelation is that it is a *reductio ad absurdum* for the whole point of view—and that will certainly be the first thing the critics pick up on. But—you wouldn't have guessed less—I'm starting to view it as a godsend. For with this simple train of logic, one can immediately stamp out the potential reality/objectivity of any of a number of terms that might have clouded our vision.

You'll find the most useful stuff in here starting at my last note to you, i.e., page 78 onward. In particular you might enjoy the chart on page 84. It shows, I think, that when this exercise of epistemologizing so much is over and done with, there's still a fair amount left that one might be willing to call concrete reality.

I do hope you get something out of this. Two of your questions were the sources for the vast majority of the pages in the document. If you hadn't pushed me, I may have never seen that so much was waiting in the wings to be made sense of.

16-10-01 *Craters on the Moon* (to J. A. Waskan)

In a way you stole me away from the family tonight; the little ride home was full of thoughts about craters on the moon. You said something like, "That there are a thousand craters on the dark side of the moon, is a true statement regardless of whether it's useful or not."

Below is a passage I took from David Darling. It once made a good impression on me (long before my James days), and I can't help but feel that it is relevant to tonight's conversation too. If all the world is but atom and void—or substitute your favorite metaphysic, for that matter—then, it seems to me, there is no strict sense in which there are "craters" on the moon at all. To interpret the coarse information I have about some aspect of my experience as a map of the craters of the moon, seems to me an ultimately subjective judgment—one that I make because it is more or less useful.

Of course, I'm not wedded to these ideas: the game I play is to pick and choose anything from any philosophy that will help me make sense of the physics I'm doing and to promote it to a new level. Sometimes, after a sufficient amount of play, I change my philosophical mind. But I think the observation below is not a completely idiotic one, and it sends me some way toward the pragmatic conception of truth. No proposition I use in my daily life can be strictly true or false in the sense of reflecting the world as it is independently of me. And if not the case in my daily life, then why should it be the case at some more "ultimate" level (i.e., fundamental physics) that—after all is said and done—was intellectually derived from that daily experience in the first place?

> The interface between mathematics and everyday reality appears sharp and immediate at this point: one sheep, one finger, one token; another sheep, another finger, another token, and you can take away tokens or add them, as you can with your fingers. The tokens—the numbers—are just abstracted fingers; the operations for dealing with the tokens are just the abstracted raising or lowering of the fingers. You make a one-to-one correspondence between the tokens and whatever it is you want to reckon, and then forget about the fingers.
>
> At first, it seems clear from this that mathematics must be somehow already "out there," waiting to be discovered, like the grain of the stone. One sheep add one sheep makes two sheep. Two sheep add two sheep makes four sheep. That is certainly the practical end of the matter as far as the shepherd and the merchant are concerned. But already, even in this most simple mathematical maneuver, something strange has happened. In saying "one sheep add one sheep" we seem to be implying that any two sheep will always be identical. But that is never the case. Physically, the first sheep is never exactly equal to the second: it may be a different size, have different markings. It takes only one molecule to be out of place between the two, and they are not identical. Indeed, because they are in different places they are inevitably not the same on that basis alone. We have extracted a perceived quality to do with the sheep—namely, their "oneness," their apartness—and then merged this quality by means of another abstraction—the process of addition. What does it mean, physically, to "add" things? To put them together? But then what is "putting together" two sheep? Placing side by side, in the same field—what?
>
> All this may seem like nit-picking. But on the contrary, it brings us back to the central mystery—the relationship between the inner and the outer, the world of the rational mind and the world "out there." In the physical world, no two sheep are alike. But, more fundamentally, *there are no "sheep."* There are only some signals reaching the senses, which the left brain combines and then projects as the illusion of a solid, relatively permanent thing we call a sheep.
>
> Like all objects, sheep are fictions: chimeras of the mind. It is our left hemispheres, having through natural selection evolved this skill for extracting survival-related pieces of the pattern, that trick us into seeing sheep, trees, human beings, and all the rest of our neatly compartmentalized world. We seek out stability with our reasoning consciousness, and ignore flux. We shut our eyes to the continuous succession of events if those events seem not to substantially affect the integrity of what we see. So, through this classifying and simplifying approach we make sections through the stream of change, and we call these sections "things." And yet a sheep is not a sheep. It is a temporary aggregation of subatomic particles in constant motion—particles which were once scattered across an interstellar cloud, and each of which remains within the process that is the sheep for only a brief period of time. That is the actual, irrefutable case.

17-10-01 *Quick Single Point* (to J. A. Waskan)

Thanks for the note: I'm still digesting it. But let me quickly reply to the one thing that I can reply to.

Waskanism 1: *Also, it seems strange for the fellow you quote to concede that there is light, eyes, the left half of one's brain, a process known as natural selection, and at the same time to deny the existence of sheep.*

Indeed, it seems strange to me too: He's pretty clearly not being consistent. But the role of the quote for me was as a motivating piece. (I read Darling a few years ago, and the main reason I used the quote last night was because it was already in my computer and I could, thus, send off a quick note to you.) My sentences above the quote were meant to show that I toy with the idea of going a further (more consistent) step:

> And if not the case in my daily life, then why should it be the case at some more "ultimate" level (i.e., fundamental physics) that—after all is said and done—was intellectually derived from that daily experience in the first place?

It is going that extra step that seems to me to be heading down the track to Jamesianism.

Let me read your note again ...

17-10-01 *Quick Second Point* (to J. A. Waskan)

Sorry, I can't wait for the beer.

Waskanism 2: *Here's a daily affirmation for you. Look in the mirror (not Rorty's mirror of nature, the one in your house), and say the following:*

I am pretty darned sure that I exist. I think there is other stuff in the universe too. I'm pretty darned sure that I have beliefs about (i.e., representations of) the other stuff that might be out there. My beliefs are true insofar as the world is how I represent it to be. If the world outside of my mind is in no way how I represent it as being (e.g., IF THERE ARE NO SHEEP, etc.), then all of my beliefs are false. Even if all of my beliefs about the things outside of my mind are false, this should have no bearing on the nature of truth itself (i.e., correspondence).

If you want to use true and false that way, then—in my present state of mind—yes, I would say that, strictly speaking, most all my beliefs are false. Beliefs play the role of coordinating our actions, and, in that way, can be more or less useful. But (in the too small of thought I've given this) I can't find a role for the concept of belief outside its use.

Like you, I am pretty darned sure of the existence of a world outside myself. But I would say that that surety comes NOT from some (transcendental?) knowledge that my beliefs mirror that world as it is. It is just the opposite. I believe in a real world outside myself because, throughout my life, things continue to take me by surprise. Significant numbers of my beliefs are systematically INvalidated with each new day. There's my evidence of the real world. Below is the way I put this point in a recent paper.

The point of view is not completely worked out yet—and it may never be—but my experience in quantum mechanics makes it feel more right than the other options I've seen so far.

Knowing me, I'll probably give your note another read, and be back again tomorrow. I hope you don't lose patience with me.

17-10-01 *Quick Third Point* (to J. A. Waskan)

OK, one more for the day.

Waskanism 3: *Also, sure no two sheep are exactly alike. Neither are any two bachelors. That doesn't entail that there are no bachelors. X is a bachelor if X is unmarried, and X is male, and X is eligible (e.g., not a priest).*

Granted. But what I thought was at issue is whether there are "bachelors" without the agents who make up (and use) all these judgmental categories. If all the world is BUT atom and void—to use an allegory I like but which should not be taken literally as my view—then where do all these extra distinctions come from if not the judgmental agent?

I say I thought this was the issue because my reading of the pragmatic conception of truth is more to the following point: Without agents, there are no "propositions." Therefore "propositions" cannot be true or false in any absolute sense. Without the agent, there is the world, and it is just whatever it is. A proposition adds something to the world that it itself did not possess before the agent's attention was drawn to it (via the act of dreaming it up, writing it down, acting upon it, etc.).

There's probably nothing worse than to have an armchair philosopher in your presence ...

18-10-01 *No Doobies Here?* (to J. A. Waskan)

Waskanism 4: *Lots of folks want to say that there are joints in the natural order (though, admittedly, bachelorhood probably isn't one of those joints).*

I'm not sure what you mean by the term "joints." Can you define it precisely? (Not knowing what you mean, it leaves me unable to reply to most of your message.)

Waskanism 5: *If, however, you take a step back and look at the big picture, ... I suspect that the same can be said for the relationship between atoms and tables.*

You know what my worry is (fueled by the 75 year debate on the interpretation of quantum mechanics). It's that we just can't step far enough back. We are immersed in this thing called existence, and there's just no way to get a view from outside it. We do the best we can from the inside, and that's called science. (For me, the phrase "best we can" means to eliminate impredictability—i.e., (only half jokingly) to delete reality as much as possible. Cf. yesterday's note about Emma.)

Tell me what a joint is, so I can think a little more about what you said.

22-10-01 *The Dilemmas of Subjectivism* (to R. Schack & C. M. Caves)

I apologize for holding off so long in a reply to Rüdiger's letter concerning the RMP article. The difficulty has come in that I didn't know how to reply. (I guess I still don't.)

The point of some potential consternation is this:

Schackcosm 44: *What I think we should be doing is a paper on "Interpretations of probability in quantum mechanics (with special emphasis on the Bayesian viewpoint)". The paper would NOT be on the interpretation of quantum mechanics.*

The problem is, I don't see how to separate the two issues. Where does the interpretation of probability fall off and the interpretation of quantum mechanics kick in? How can one have an interpretation of quantum mechanics wherein the wavefunction is objective, but still think of probabilities as being strictly subjective? Similarly, vice versa?

Let me do this: Let me ship to you both the mini-samizdat of my thoughts that came out of my post-Växjö broodings. You tell me which *sentiments* will be banned and which won't if we end up skinny-dipping together. I can foresee some being excluded—like the stuff in my letter to Wootters—and I can accept that; but for the greater majority of the writings, I can't see myself drawing a line, and I'm wondering where you will draw it.

Looking back on the BFM debate, I think the most important thing to come out of it for us three in particular is that it makes it absolutely clear that we need to get our thoughts straight on the "principal principle" before we can embark on a consistent statement of our position. For I see no way to erase the dilemma: Either we accept that the ascription of one Kraus rule over another in a measurement intervention is a subjective judgment, or we accept a quantumatized version of Lewis's principal principle. Why did we all reject the principle before if we find ourselves accepting it now? This is something we ought to reflect upon more deeply.

23-10-01 *Increased Surveillance* (to N. D. Mermin)

My spies tell me you gave a talk on "Whose Knowledge?" yesterday. How did it go? Did anyone in the audience (unintentionally) lobby any shells on my behalf? If so, hand over their names, and I'll put out a recruiting effort for them.

23-10-01 *United We Stand Airlines* (to R. Schack)

Schackcosm 45 : *Do you think you could give me a lift at such an early time?*

I do whatever it takes for the greater good of quantum mechanics. Of course I can give you a lift! (If you can stand to listen to me babble that early in the morning.)

26-10-01 *The Feynman Cult* (to J. Butterfield)

I don't remember railing against the Feynman cult in your presence, but your letter gives some evidence that I must have. (Doing such things was a common pastime for me at Caltech, but I've mellowed a little in my efforts since leaving, i.e., since the cult hasn't been in my face on a daily basis.)

But, anyway, yes I told Brandt that I would come. (I hope you will come too.) I've even thought about how I will open my talk: with the Feynman quote below. Lord knows I'm no materialist, so you can rest assured that I'll do my best to "zing" it up afterward. (See my Samizdat, page 237.)

> If, in some cataclysm, all of scientific knowledge were to be destroyed, and only one sentence passed on to the next generation of creatures, what statement would contain the most information in the fewest words? I believe it is the atomic hypothesis (or the atomic fact) that all things are made of atoms—little particles that move around in perpetual motion, attracting each other when they are a little distance apart, but repelling upon being squeezed into on another.
>
> Everything is made of atoms. That is the key hypothesis.

27-10-01 *Coming to Agreement* (to C. M. Caves & R. Schack)

Schackcosm 46 : *In any case, do you know a reference for strong Dutch-book consistency?*

Nothing comes to mind: I think I first learned of it from you (was it in Cambridge?). Might it have been in either of those papers we looked up in a trip to the university library one day? This was one of them (though I don't have it anymore):

> J. G. Kemeny, "Fair Bets and Inductive Probabilities," J. Symb. Logic **20**, 263–273 (1955).

Ahh, here was the other one:

> R. S. Lehman, "On Confirmation and Rational Betting," J. Symb. Logic **20**, 251–262 (1955).

It might be a good idea to dig those up again.

I'll try to answer your other questions next week, after I get a chance to get a better grasp on the paper.

27-10-01 *Literature* (to R. Schack)

Schackcosm 47 : *A question to you: How much literature is there on interpretations of probability in q.m.? Would it be feasible to review it all?*

Attached is everything I had collected up previous to the Cerro Grande fire. A skim of that document might answer your question in the most direct way.

By the way, I've asked Maria Carla Galavotti for her criticisms (and any others that she knows) of the principal principle. But I haven't gotten a reply from her yet.

Thinking back on it, I can't remember if we ever had any NONquantum reasons that were substantially different than J. S. Mill's argument against a "substance" underlying the phenomenal world:

> If there be such a *substratum*, suppose it at this instant miraculously annihilated, and let the sensations continue to occur in the same order, and how would the *substratum* be missed? By what signs should we be able to discover that its existence had terminated? Should we not have as much reason to believe that it still existed as we now have? And if we should not then be warranted in believing it, how can we be so now?

Similarly one could say of objective probability: Bayesian coming-to-agreement would work precisely the same whether the objective probability is there or not. But did we have any other arguments than that (that did not depend on upon quantum mechanics, for instance in the nonuniqueness of the density operator decomposition)? Did we have any examples where believing in objective probability in Lewis's sense would be downright misleading in how it might suggest tackling a practical problem?

I so wish I had my old file cabinet back again; writing you this morning has made that feeling more acute.

04-11-01 *Dreams of an Ever-Evolving Theory* (to A. Peres)

Asherism 2: *I am reading a wonderful book "Dreams of a Final Theory" by Steve Weinberg. Chapter 7 is "Against Philosophy" and I highly recommend it. I got that book for $7.00+tax in a used books shop in Santa Barbara, where Aviva was looking for something else.*

Thank you for the tip; I did read Weinberg's chapter 7. He writes in a very crisp and no nonsense way, and I like that. (You know I constantly fight tendencies in the opposite direction in my own writing.) His points are well taken, especially the ones about how a preset philosophy can create immense blinders for the scientist:

> "... in rejecting it the [PHILOSOPHY-X]ists were making the worst sort of mistake a scientist can make: not recognizing success when it happens." — page 177, paperback version.

But, I think a deeper point is the one he makes near the beginning of the essay:

> "I do not want to draw the lesson here that physics is best done without preconceptions. At any one moment there are so many things that might be done, so many accepted principles that might be challenged, that without some guidance from our preconceptions one could do nothing at all." — page 167

And I agree with the numerical tally of the next sentence:

> "It is just that philosophical principles have not generally provided us with the right preconceptions." — page 167

However, I part company with him in thinking that that is a strong argument against pursuing philosophy as a sideline to science. That is, I don't know what the preconceptions can be if they're not philosophies.

When it comes to philosophies and, not unrelated to that, scientific research directions, I tend to take a lot of stock in a Darwinian kind of conception. That is, we each should do precisely what we feel compelled to do; we each should research precisely what we feel compelled to research. There's probably nothing we can do about it anyway. Indeed ninety-nine percent of the time we will be on the wrong track: The world supplies a selection pressure for our thoughts, just as it does for the lifespan of the drosophila. And just as it is not possible for the drosophila to change its genetic makeup before it meets its demise, I think the only thing we can do as scientists is cultivate to the best of our ability the philosophical preconceptions that led us down our own paths. Ninety-nine percent of us will be forgotten from the history books, but the ones of us that remain will do so because the world is such that it is less likely that we should fall.

For myself, I have DISCOVERED that I have chosen a direction of thought that is very closely aligned with the philosophical movement of pragmatism from the early part of the 20th century—a movement the details of which have been nearly forgotten in modern times. Interestingly, the thing that set me on to this realization was Martin Gardner's essay "Why I Am Not a Pragmatist" in his book *The Whys of a Philosophical Scrivener*. (You probably remember Gardner from his column in *Scientific American*.) This happened about three months ago. I really recommend you read the article if you get a chance. Maybe your library has a copy of the book. I think in reading it, you will discover that the essay might just as well have been titled, "Why I Am Not in Agreement with Fuchs and Peres's Physics Today Article." For, with each reason Gardner used to explain why he was *not* a pragmatist, I found myself thinking of quantum mechanics and saying to myself, "ahh, I

guess that means I *am* a pragmatist." Really, the analogy is *that* close even though the article has nothing to do with quantum mechanics per se.

The issue is no less than whether "unperformed measurements have no outcomes." The pragmatists, for various reasons, thought it was *safer* to assume that they didn't. The movement then spent the greater part of its time developing the (liberating) consequences of this supposition. Gardner (and Bertrand Russell and G. E. Moore and gazillions of others) thought "how silly" and "how contrived" when it is so much easier to use standard realist language to describe the outcomes of experiments—to assume the outcomes are there before one has a look. But you and I know better, of course. And, I think it is quite useful to know that there was a set of people carrying through the detailed consequences of this line of thought for their broader worldview long before you and I were on the scene. The way I view it, these old thoughts can be a resource to our explorations of quantum mechanics just as much as any other: However, their use is in setting the directions for potentially fruitful lines of thought . . . but that should always be the use of any philosophy for any scientist.

Below I'll attach a letter I wrote Bill Wootters a while ago on a similar subject. What I write just after the quote of William James better explains why I chose the title that I did for this note.

14-11-01 *Samizdat II* (to N. D. Mermin)

Merminition 72 : *Expanding slightly on my reaction to Samizdat II: (Please don't conclude that I've missed the point yet again until we meet):*

It seems to me that all interpretations of QM have to come up against what is loosely called the "measurement problem" in one form or another, which I would describe as how we can reconcile a world empty of "facts" to the "facts" of our own experience. (I would not describe it as having to do with how wave-functions "collapse" or "decohere".)

Various people (at least those who don't want to decree that there is a "cut" between two domains) deal with the problem by extending the facts of our experience to everything else (Bohmians, GRW collapsists). Others deal with it by extending the indefiniteness of QM to our apparently definite experience (many worlders). It seems to me you're following the second strategy (which is all I meant by my irritating jokes about your Everettism — your version of the second strategy is obviously different from theirs) by insisting that the subjective character of quantum probabilities requires us to take the same subjective approach to classical "facts" — i.e. to insist that they too are beliefs that can also be dealt with only through a (subjective) probabilistic treatment.

This is intriguing and well worth exploring. I do worry (with Carl, I think) that it's getting rather far from how physicists do physics – or at least from how they think they do physics. But I wouldn't say it means the end of science. Whatever that means.

Thanks for the extended comments. I won't say you've missed the point: I think you've got it. But I don't quite understand the Everett analogy yet. I would say their world—Deutsch's world—abounds with facts. Facts far, far in excess of what any of us ever see. (I.e., all their worlds.) But I'll think much harder about your note. There's no need to reply yet again.

19-11-01 *A Lot of the Same* (to C. M. Caves & R. Schack)

Schackcosm 48 : *Before you have me burned alive, please tell me why I am wrong!*

Come on, you know it's the Thanksgiving season. I would never burn you, only roast you. (Though my brother-in-law once fried his turkey in hot oil.)

Sorry for the hiatus, but I just got inundated with email last week, and I didn't have the proper mentality for replying to any of it. So I shut down for a while. Now I'm stuck with trying to clean out an even bigger pile of old mail. But let me compliment you by letting you know that I'm tackling your letter first! (It's the only interesting one in the lot.)

Schackcosm 49 : *I started writing up a summary of our discussions, and hit upon a difficulty when I tried to formulate exchangeability for models. Here is the problem.*

In the traditional formulation of exchangeability, we say that we have N identical systems (same Hilbert space). At this stage, it is thinkable to assign a different state to each system. We then make the judgement that the state of the N systems is exchangeable.

For models, we say that we have N identical apparatuses. Alternatively, we say that we use the same apparatus to perform N measurements, let's say on different, independently prepared systems. At this stage, I can't think of a good reason to even consider assigning different models to the apparatuses. It seems to me that one is forced to say that each apparatus performs the same operation, so should be described by the same model. That leads immediately to a heresy: there exists a true model. If we don't know it, we assign probabilities to models. What we wrote down on your whiteboard is consistent with this viewpoint. The difficulty we encountered formulating exchangeability could mean that it is an unnatural concept in this case. Writing down a mixture of N-fold products of models is completely natural however.

Let me try to allay your fears. I think the issues here are almost precisely the SAME as they are in our old de Finetti considerations. To say it in a way that maybe Carl would endorse, "It's really all about learning." Or in a way that I'm more tempted to these days, it's all about demonstrating a willingness to update one's beliefs—one's commitments, one's pragmatic strategies for action, one's betting behavior—in the light of factual data.

Let me start with an example that's essentially already well-worn for us by now. Suppose we have a rather complicated quantum measurement device whose manufacturer purports it to be the best σ_z measurement device ever built. Furthermore, suppose we have a fresh supply of 10^8 calcium atoms, all meticulously prepared to have spin-up in the x-direction. What do we expect to happen if we individually dump all the atoms into the measuring device? We expect about 50% of them to get registered as spin down and about 50% of them to get registered as spin-up. But what happens if one after another, all the registrations are of the spin-up variety? Well, that outcome sequence would be no less likely or no more likely than any other outcome sequence if we walked into the laboratory with such a radically adamant prior belief. In a real-life situation, however, we would be shocked; we would update our beliefs accordingly—for we would have allowed for the possibility of "learning."

But in this situation, notice that there are at least two extreme cases to which we could attach the possibility of learning. The learning could be about the device or it could be about the preparations. Who's to say that the learning is about something more objective in the one case than the other? Prosaically, it takes both ingredients (the preparations and the device) to certify the device, and you can't get away from that.

Let me try to tighten this up by sketching how we ought to start thinking about a de Finetti theorem for unknown quantum models. I run a measurement device on N independently and identically prepared quantum systems. Suppose I am absolutely confident of these preparations—i.e., with respect to them, there is nothing left to learn in the technical sense of i.i.d. statistics for any repeated and KNOWN measurement. Then, what can it mean—from a Bayesian point of view—that the measurement device works according to an unknown model? It means that after all the outcomes are gathered, there's still something left to be learned from the posteriori quantum state for the systems that were measured.

That is, more simply, the best judgment we can make about the systems that passed through the measurement device is that they are exchangeable CONDITIONED on the registered measurement outcomes. For instance, suppose the device spits out an index i at each round. The issue is, what pragmatic meaning should we give to each such i? Quantum mechanically, the predictive meaning of an index i is specified by the Kraus operator A_i we associate with that outcome. (It's retrodictive meaning is given by the positive part of A_i—the POVM.)

If we think we don't exactly know what the device is doing to each individual system, then we shouldn't yet dare to make an association $i \to A_i$. (To make an extreme point of it, for all we know, the device might be entangling all our test systems.) We should just rest confident that no matter what order we send the systems through the device, we will end up with the same subjective beliefs in the end. Thus, if we gather up all the systems for which an outcome i occurred (as opposed to some other outcome j), then the subjective density operator we assign should be exchangeable. Using the standard quantum de Finetti theorem, we then get that that density operator must be of de Finetti form. Writing each of the final density operators as a linear map acting on the initial density operator, we (should) get something like the desired theorem for unknown quantum models. If we believe that we can learn something about the model, then the probability distribution that appears in the de Finetti form is restricted to being something other than a *delta*-function.

In summary, our belief that the best we can say of the outputs is that they ought to be exchangeable (conditioned on the factual outcomes), leads directly to a notion of mixture of models—i.e., that the output density operator is controlled either by a Kraus operator A_i, or a Kraus operator B_i, etc., etc., about which we capture our ignorance through some subjective probability distribution.

Now, just as the regular de Finetti theorem cannot put an end to the principal principle, we cannot use this (proposed) theorem to put a stake through the heart of the true believer of objective quantum models. That is, David Lewis might say of the regular de Finetti theorem, "That is a very nice theorem, but it doesn't change the fact that there really is always a 'man in the box.' His name is God." And so he would probably also say of our quantum models (if he knew quantum mechanics). Instead, all the (proposed) theorem can do is show that it is *possible* to get by without a man in the box. We don't need him; all we need is something like the judgment of exchangeability for the outputs (conditioned on the outcomes) along with i.i.d. on the inputs.

So that's the sketch. Now, how to dot the i's and cross the t's? I can foresee at least one difficulty that I'm not clear-headed about right now. That is, using the description above, for each index i, we will generate a probability distribution over models. But by what regularity condition can we assure that $p_i(\mathcal{A}) = p_j(\mathcal{A})$? What I mean by this notation is that $\mathcal{A}$ stands for the model in total (i.e., all the Kraus operators in it) and $p_i(\mathcal{A})$ stands for the probability distribution in the de Finetti theorem derived for each index i. It is probably so simple as this: If we were to imagine doing tomography on the posterior states for each index, then the states derived from that should always average up to a valid density operator. But I'm not exactly sure how to put that idea into action.

Oh, and here's another intriguing point that ought to be explored. Suppose we focus our attention on a given exchangeable density operator. There are many ways that operator could arise, but suppose that it came about as the posterior state arising from many identical measurements (in the sense above). The question I have in mind is how much freedom do we have for trading off between an unknown preparation and an unknown model for getting to the final state? Can one always find a fixed initial preparation and a mixture of models that will give rise to the final exchangeable state? Can one always find a mixed model and a mixture of initial preparations? Probably yes and yes, but I'm not completely sure.

I'm so glad to hear that you may have reversed your opinion on William James (at least a little). By the way, I hope you notice how these de Finetti considerations are drawing out a lot of the

considerations I was trying to express to you in Samizdat-II and our subsequent discussions. For it helps draw the distinction between the amorphous index i that arises in a quantum measurement and the meaning A_i that I ascribe to that event. The symbol A_i plays the role of a proposition that I write about i: It carries the information about how I will react after having seen it, how I will place my bets.

I'm willing to believe this whole debate about "truth" might be a red herring—i.e., that we might easily be able to get away with never uttering the word. But I think the realization of the last paragraph had a primitive expression in James's worries about "truth" nevertheless, and to that extent maybe he and his movement of pragmatism are worth contemplating (though of course not subscribed to in toto).

19-11-01 *William James* (to R. Schack)

Schackcosm 50 : *You will be pleased to learn that I bought a copy of "Pragmatism" and enjoy reading it a lot! His rhetoric is the best I have ever seen from a philosopher. He is definitely not tender minded.*

It's a curious mix, though. Sometimes he seems to be very close to Kantian ideas on truth, then he seems to subscribe to a naive correspondence theory of truth, at least for simple facts such as "this detector has clicked."

Yes, I'm more attuned to that now, and I'm trying to get it straight through extended readings (such as A. J. Ayer's book *The Origins of Pragmatism* that I picked up in New York City with you).

Schackcosm 51 : *I am also a little disturbed by his praise for Ostwald.*

I don't remember his praise for Ostwald. Who was Ostwald? And what does James praise him about?

Schackcosm 52 : *I am half way through writing a much improved draft of our paper.*

Sounds great.

20-11-01 *A Bathtub Moment* (to C. M. Caves & R. Schack)

I'm sure I've already told you both the story of the time I happened to end up at a British pub with Caroline Thompson, the famous Bell-inequality conspiracy theorist, but let me repeat it for the purpose of having it in this box. Somebody at the table was speaking of the great importance of intuition, of being able to "see beauty in a theory." I, with my usual example, piped up that I thought that was hogwash: I always thought Mary Ann was the prettiest girl on Gilligan's Island; my best friend thought it was Ginger. Anyway, I followed that comment with, "I never use any intuition in my calculations; I don't even know what intuition can be in that context." Caroline Thompson harrumphed, "Well we could see that from your talk!"

OK, so it won't be an intuition, but here's a hunch that hit me while I was taking a shower this morning. It's connected to the long note on de Finetti I sent you yesterday.

Among the thirty other reasons I have been thinking that trace-preserving quantum operations (and now measurement models explicitly) are subjective entities is because one can make a one-to-one correspondence between them and the density operators on a larger Hilbert space. That

is, they have the same structure as the states of belief that we've already toyed with.[2] This has suggested to me that there ought to be a Gleason-like theorem for quantum operations (which I pursued a little bit but never could quite make things connect). But now this idea is rearing its head again in the context of a de Finetti theorem for models.

Yesterday, I blithely said something to the following effect. To get at a concept of an unknown model, what you do is 1) to 4) :

1. Start with many copies of a quantum system, for which you believe 1) that they are exchangeable and 2) that there is nothing left to learn. The standard quantum de Finetti theorem then gives us that the density operator we ascribe to the collective system will be a tensor product of identical quantum states.

2. Now drop each of those systems into a measurement device and note the outcomes i. Separate the post-measurement systems into bins according to those outcomes.

3. Finally suppose we believe that the quantum state we ascribe to each bin ought to be an exchangeable state for which we *can* learn something. The standard de Finetti theorem gives that this state must be of de Finetti form (with a nontrivial support).

4. The hunch was that the conjunction of 1), 2), and 3)—or them along with some minor additional regularity condition—would specify the content of the phrase "an *unknown* quantum measurement model." The unknown model is simply given by making explicit the form of the linear maps connecting 1) to 3) for all possible inputs into A.

Notice that nowhere in there did I say anything about these maps being completely positive. I just chose the word "linear" for some reason. But surely the assumption of complete positivity must come into this too. So now my question: UNDER THE ASSUMPTION that a measurement model is a state of belief, can one adequately explain the notion of an unknown measurement model by de Finetti techniques WITHOUT the technical assumption that these linear maps are completely positive? Or, instead, is complete positivity absolutely crucial to the program?

It strikes me that Carl's superoperator calculus has got to be the way to go for exploring this question.

20-11-01 *James's Loose Lips* (to R. Schack)

Schackcosm 53 : *In lecture 2, James says "I found a few years ago that Ostwald, the illustrious Leipzig chemist, had been making perfectly distinct use of the principle of pragmatism [. . .]"*

In my own words, Ostwald rejected as meaningless any statement that did not have observable consequences. A very pragmatic attitude indeed. The trouble was that Boltzmann's ideas about atoms fell into this category, at least that was the public opinion, led by Ostwald, at the time. No cash value in the "atom hypothesis". I imagine somebody like Ignacio (I pick him only because of his obvious scepticism at my talk in Benasque) to ask: where is the cash value in the Bayesian approach to quantum mechanics?

I agree with you now that that is a troubling praise coming from James. I noticed similarly somewhere else in the book that he also classified Mach in the ranks of the pragmatists.

[2] By the way, Carl, as I recall, was never happy with thinking this point had any significance for our program. I think he saw it as little more than a coincidence.

But there is a grave distinction between positivism (Mach, Ostwald, etc.) and pragmatism (James, Dewey, etc.) as I see it. The positivists eschewed all metaphysical assumptions—thus the egg on their faces for not coming up to speed on the statistical mechanics an atomic hypothesis can afford. The pragmatists, on the other hand, are willing to glorify any metaphysics with a cash value. This relates to the passage by James on Mill that I read you while you were visiting. With metaphysics, the cash value is not in its explanation of any previously discovered facts, but in the concrete actions its BELIEF will give rise to in the agent believing it. Thus James's argument, for instance, for everyone's right to believe in a God, even if that God will never have the opportunity of being confirmed or falsified in an objective fashion. A God's validity in an agent is in his cash value for the agent's ethics, morals, and mode of action for his daily life.

To put this in concrete terms for Carl versus me: I would say that the ontological hypothesis I'm shooting for will show some cash value in the amount of interesting physics it leads to, to the opening up of new quantum computing and quantum control and quantum cryptography methods. And I think (or, more accurately, BELIEVE) it will help us make the leap to the next stage of physics. Whereas I would say—but it's just a gut feeling—that the ontology Carl has been shooting for (i.e., the Hamiltonian) has no such cash value. Only the money flow in the banks will ultimately tell. (And unfortunately, that can only be done with hindsight.)

James—I think in his essay "The Sentiment of Rationality"—has a beautifully worded passage on these considerations that I'll try to get scanned in tonight and sent to you. But I think James himself is either not consistent in his writings, or he's pretty sloppy in reading the other writers he wants to praise. (What I know of him now, I think it's probably mostly the latter.)

Sorry for writing all this. I got carried away. I hope to get your new draft printed out today, and studied partially tonight.

By the way, I think Ignacio is a good benchmark with all this. If we can't find a way to impact him in five or seven years, say, then maybe indeed all this is for naught.

20-11-01 *One Horse's Mouth* (to J. M. Renes)

I finally get to the last note I owe you.

Renesism 1: *you make this point in the nato paper explicitly (that the unitary taking the quantum bayes rule post measurement state to the orthodox post measurement state is a "mental readjustment" and does depend on the input state). there's a lot here, though, especially since carl initially balked pretty hard at the idea since "we're physicists so i don't know what he's (you) talking about."*

I'm intrigued by your phrase "there's a lot here, though." I don't quite understand what you're trying to get at—that you agree that it's a difficulty, or that it's a good thing? Or that if Carl balked, that might be an unintentional mark in its favor? Can you explain a little better?

Renesism 2: *some days i don't feel like a physicist (happily coinciding much of the time with the days i don't want to) so i'm not initially troubled by this. however, as i said, there's a lot of "stones unturned" here. are you saying that there is no physical picture of what's going on, there must be some subjective element "uncaptureable" by a physical picture? this seems to fit with your rep as being an "extreme subjectivist" but i'd rather hear it from the horse's mouth.*

I'm not sure exactly how I should reply to this. You probably know my thoughts at this point better than I know them myself. Maybe I should say it like this: My pet idea at the moment is that there was a world here before humankind ever appeared on the scene; there'll be a world here

after we disappear. But I would say the world is still under construction; there is no sense and no ultimate level at which it is already complete. To that extent, I believe our beliefs, our passions, our actions, our inventions, and our dreams modify the world and form part of its construction in a nonnegligible way.

And I think our greatest hint of that comes from quantum mechanics. I would say that what we're learning in a precise way from it is that there is something about the stuff of the world that makes it uncaptureable with a purely physical picture. We find that we cannot even draw a picture of the world without including our beliefs and belief changes as a crucial background in the sketch. (How could we if the world's not completed yet?)

Does that make me an extreme subjectivist? I don't know. Whatever it is though that I should be called, I think this willingness to accept a substantial part of quantum mechanics as simply "law of thought" will keep me from going down a misguided path. I.e., the path of trying to ascribe all the easiest terms in the theory a kind of physical reality independent of our presence as active agents.

One horse's mouth.

21-11-01 *Pragmatism versus Positivism* (to R. Schack)

Both quotes are taken from William James's essay "The Sentiment of Rationality."

Quote I:

> The necessity of faith as an ingredient in our mental attitude is strongly insisted on by the scientific philosophers of the present day; but by a singularly arbitrary caprice they say that it is only legitimate when used in the interests of one particular proposition—the proposition, namely, that the course of nature is uniform. That nature will follow tomorrow the same laws that she follows today is, they all admit, a truth which no man can *know*; but in the interests of cognition as well as of action we must postulate or assume it. As Helmholtz says: "*Hier gilt nur der eine Rat: vertraue und handle!*" And Professor Bain urges: "Our only error is in proposing to give any reason or justification of the postulate, or to treat it as otherwise than begged at the very outset."
>
> With regard to all other possible truths, however, a number of our most influential contemporaries think that an attitude of faith is not only illogical but shameful. Faith in a religious dogma for which there is no outward proof, but which we are tempted to postulate for our emotional interests, just as we postulate the uniformity of nature for our intellectual interests, is branded by Professor Huxley as "the lowest depth of immorality." Citations of this kind from leaders of the modern *Aufklärung* might be multiplied almost indefinitely. Take Professor Clifford's article on the "Ethics of Belief." He calls it "guilt" and "sin" to believe even the truth without "scientific evidence." But what is the use of being a genius, unless *with the same scientific evidence* as other men, one can reach more truth than they? Why does Clifford fearlessly proclaim his belief in the conscious-automaton theory, although the "proofs" before him are the same which make Mr. Lewes reject it? Why does he believe in primordial units of "mind-stuff" on evidence which would seem quite worthless to Professor Bain? Simply because, like every human being of the slightest mental originality, he is peculiarly sensitive to evidence that bears in some one direction. It is utterly hopeless to try to exorcise such sensitiveness by calling it the disturbing subjective factor, and branding it as the root

of all evil. "Subjective" be it called! and "disturbing" to those whom it foils! But if it helps those who, as Cicero says, "*vim naturae magis sentiunt*," it is good and not evil. Pretend what we may, the whole man within us is at work when we form our philosophical opinions. Intellect, will, taste, and passion co-operate just as they do in practical affairs; and lucky it is if the passion be not something as petty as a love of personal conquest over the philosopher across the way. The absurd abstraction of an intellect verbally formulating all its evidence and carefully estimating the probability thereof by a vulgar fraction by the size of whose denominator and numerator alone it is swayed, is ideally as inept as it is actually impossible. It is almost incredible that men who are themselves working philosophers should pretend that any philosophy can be, or ever has been, constructed without the help of personal preference, belief, or divination. How have they succeeded in so stultifying their sense for the living facts of human nature as not to perceive that every philosopher, or man of science either, whose initiative counts for anything in the evolution of thought, has taken his stand on a sort of dumb conviction that the truth must lie in one direction rather than another, and a sort of preliminary assurance that his notion can be made to work; and has borne his best fruit in trying to make it work? These mental instincts in different men are the spontaneous variations upon which the intellectual struggle for existence is based. The fittest conceptions survive, and with them the names of their champions shining to all futurity.

The coil is about us, struggle as we may. The only escape from faith is mental nullity. What we enjoy most in a Huxley or a Clifford is not the professor with his learning, but the human personality ready to go in for what it feels to be right, in spite of all appearances. The concrete man has but one interest—to be right. That for him is the art of all arts, and all means are fair which help him to it. Naked he is flung into the world, and between him and nature there are no rules of civilized warfare. The rules of the scientific game, burdens of proof, presumptions, *experimenta crucis*, complete inductions, and the like, are only binding on those who enter that game. As a matter of fact we all more or less do enter it, because it helps us to our end. But if the means presume to frustrate the end and call us cheats for being right in advance of their slow aid, by guesswork or by hook or crook, what shall we say of them? Were all of Clifford's works, except the *Ethics of Belief*, forgotten, he might well figure in future treatises on psychology in place of the somewhat threadbare instance of the miser who has been led by the association of ideas to prefer his gold to all the goods he might buy therewith.

In short, if I am born with such a superior general reaction to evidence that I can guess right and act accordingly, and gain all that comes of right action, while my less gifted neighbor (paralyzed by his scruples and waiting for more evidence which he dares not anticipate, much as he longs to) still stands shivering on the brink, by what law shall I be forbidden to reap the advantages of my superior native sensitiveness? Of course I yield to my belief in such a case as this or distrust it, alike at my peril, just as I do in any of the great practical decisions of life. If my inborn faculties are good, I am a prophet; if poor, I am a failure: nature spews me out of her mouth, and there is an end to me. In the total game of life we stake our persons all the while; and if in its theoretic part our persons will help us to a conclusion, surely we should also stake them here, however inarticulate they may be.

Quote II:

Now, I wish to show what to my knowledge has never been clearly pointed out, that belief (as measured by action) not only does and must continually outstrip scientific evidence, but that there is a certain class of truths of whose reality belief is a factor as well as a confessor; and that as regards this class of truths faith is not only licit and pertinent, but essential and indispensable. The truths cannot become true till our faith has made them so.

Suppose, for example, that I am climbing in the Alps, and have had the ill-luck to work myself into a position from which the only escape is by a terrible leap. Being without similar experience, I have no evidence of my ability to perform it successfully; but hope and confidence in myself make me sure I shall not miss my aim, and nerve my feet to execute what without those subjective emotions would perhaps have been impossible. But suppose that, on the contrary, the emotions of fear and mistrust preponderate; or suppose that, having just read the *Ethics of Belief*, I feel it would be sinful to act upon an assumption unverified by previous experience—why, then I shall hesitate so long that at last, exhausted and trembling, and launching myself in a moment of despair, I miss my foothold and roll into the abyss. In this case (and it is one of an immense class) the part of wisdom clearly is to believe what one desires; for the belief is one of the indispensable preliminary conditions of the realization of its object. *There are then cases where faith creates its own verification.* Believe, and you shall be right, for you shall save yourself; doubt, and you shall again be right, for you shall perish. The only difference is that to believe is greatly to your advantage.

The future movements of the stars or the facts of past history are determined now once for all, whether I like them or not. They are given irrespective of my wishes, and in all that concerns truths like these subjective preference should have no part; it can only obscure the judgment. But in every fact into which there enters an element of personal contribution on my part, as soon as this personal contribution demands a certain degree of subjective energy which, in its turn, calls for a certain amount of faith in the result—so that, after all, the future fact is conditioned by my present faith in it—how trebly asinine would it be for me to deny myself the use of the subjective method, the method of belief based on desire!

In every proposition whose bearing is universal (and such are all the propositions of philosophy), the acts of the subject and their consequences throughout eternity should be included in the formula. If M represent the entire world minus the reaction of the thinker upon it, and if $M + x$ represent the absolutely total matter of philosophic propositions (x standing for the thinker's reaction and its results)—what would be a universal truth if the term x were of one complexion, might become egregious error if x altered its character. Let it not be said that x is too infinitesimal a component to change the character of the immense whole in which it lies imbedded. Everything depends on the point of view of the philosophic proposition in question. If we have to define the universe from the point of view of sensibility, the critical material for our judgment lies in the animal kingdom, insignificant as that is, quantitatively considered. The moral definition of the world may depend on phenomena more restricted still in range. In short, many a long phrase may have its sense reversed by the addition of three letters, *n-o-t*; many a monstrous mass have its unstable equilibrium discharged one way or the other by a feather weight that falls.

26-11-01 *PRA Proofs* (to C. M. Caves & R. Schack)

I'm in the office again finally, and I've read over the PRA proofs. Of course, as always, I can't see that any of their changes were for the betterment of the paper ... but this time at least, none were overly annoying to me.

Here are my notes (which one of you two might want to incorporate into a reply).

1. page 2, "If one accepts this conclusion ..." : Note they take away the "a" in front of the kets. That grammatical change takes away the impression that the final state is unspecified within the set. I vote that we force them to reinstate the "a"s unless you can figure out a smoother way to express the proper idea.

2. page 2, "The physical basis of Einstein's ..." : They changed the end of the sentence to "amenable to experimental testing." That seems odd to me. Should we protest?

3. page 3, "We then use a version of the so-called Dutch-book ..." : They want us to explain "Dutch-book argument." That's pretty stupid, given that they didn't ask us to explain "Bayesian probability theory" or even "Gleason's theorem." We even used the warning sign "so-called." I don't know what more can be said without inserting Section II into the middle of this paragraph. Perhaps you guys have a nicer way to approach it than I would.

4. page 5, "The probability assignment is thus inconsistent ..." : You'll note in the proof that the equation at the end of the sentence is broken at the end of the line in an awful way. Can we ask them to keep it together? Or perhaps we can simply display the equation.

5. page 5, "For example, normalization of the probabilities ..." : This is not a problem of theirs but a question of mine. We end the sentence with "so obvious that it needs no justification." Do we really need to insert that phrase? I find it a little distracting from the main point of the sentence.

6. page 6, "The keys to these results are ..." : I would change the very last word to "paragraphs."

7. page 10, "The data gathered from the measurements are said ..." : What is this, England? "The House of Commons have voted ..." Horrible. I view "data" as a collective noun.

That's it really.

But let me take this opportunity to give one last ramble from the heart. It has nothing to do with changing the present paper: I just want to say it because it's on my mind again.

After reading the paper once more, I found myself feeling awful again. I don't think there is a reader out there besides Schack and Caves that will come away with the feeling that the Dutch-book argument is *purely* an internal consistency argument (or rationality check). The English in a sentence like,

> Given the assumptions of Gleason's theorem, if a scientist has maximal information, any state assignment that is different from the unique pure state derived in the last paragraph is inconsistent in the Dutch-book sense; i.e., it leads to a sure loss for a bet on the outcome of a measurement on a single system that includes the unique pure state among the outcomes.

is just loaded with imagery. Who out there will read "sure loss" as anything other than a factual state of affairs—something dictated by the world independent of the agent? Who out there will read the word "unique" as really meaning "unique with respect to the agent's belief of certainty"? Who out there will not interpret the phrase "maximal information" in an objectivistic way—i.e., that there is one and only one way to have maximal information? Or here's a better acid test: If a reader were to be confronted with our paper in its present form at the same time as the BFM paper, would he be able to see any philosophical differences in the approaches of the two papers? I can't imagine it. All that troubles me very deeply: A small change of language really could have made all the difference in the world.

29-11-01 *Observation* (to J. M. Renes)

Renesism 3: *here's some stuff relating to what i wrote the other day from "The Taboo of Subjectivity" by B. Alan Wallace:*

> *The disdain of scientific materialism for subjectivity has also shaped the very concept of scientific observation. While nonscientific kinds of observation also detect phenomena—such as our joys and sorrows, hopes and fears, ideas and inspirations—they are thought to be tainted by human subjectivity and are therefore suspect. From the perspective of scientific materialism, human sensory perception may be deemed not only unreliable but irrelevant. For a scientific observation to take place, all that is required is a detector, or receptor. The human eye is one type of receptor, which detects a certain range of electromagnetic frequencies, but other instruments also measure this and other types of information, and they are regarded as more reliable.*
>
> *In common parlance, for an observation to take place, the received information must be transformed into humanly accessible information that is, sooner or later, perceived and understood by a human being. But according to scientific materialism,* observation *is assimilated into the general category of* interactions*, thereby freeing it from the subjectivity of its normal associations. This interpretation is said to be central to grasping what is involved in scientific objectivity in the search for knowledge and the justification of belief.*

[reference to Dudley Shapere "The Concept of Observation in Science and Philosophy," Philosophy of Science **49** *No. 4, 1982, pg 485.]*

Beautiful quotes! Do you have the whole Shapere article? Do you know whether he is for or against that conception of observation (i.e., as interaction)? If you do have the article, could you make a copy and send it to me? And what about the Wallace thing—is that a book or an article? How is it?

29-11-01 *Community* (to W. K. Wootters)

What a beautiful letter; thank you.

In a way, I've been going to my own seminar on science and religion lately—but with me, through my reading choices. I've gotten stuck on the "pragmatism" movement, predominantly William James's version. I had never realized before what a wealth of material was there (for the kinds of thoughts I'd like to think, about quantum mechanics in particular). Nor had I realized

how Wheeleresque and Woottersesque James was in his outlook—hanging so much on the idea that the universe is (in part) a product of our collective experience.

In that regard, but with respect to religion, I have never been so impressed by the possibility—necessity even—of "faith" than when I read James's articles "The Sentiment of Rationality" and "The Dilemma of Determinism." They have made a huge effect on me. If you happen to read them and have any reaction, I would love to hear your thoughts. (For fun I just had a look at the Williams College library; you guys have a collection of 63 items penned by James! In contrast, not one hit in the Bell Labs library ... but who would have guessed otherwise.)

I sympathize with your unease with telepathy. Here's how Stephen Brush put it:

> 'Wheeler's dilemma' is this: how can one maintain a strong version of the Copenhagen Interpretation, in which the observer is inextricably entangled with that which is observed, while at the same time denying that our consciousness affects that which we are conscious of—and thus accepting the possibility of telekinesis and other psychic effects? For Wheeler himself there is no dilemma at all; one simply has to recognize 'the clear distinction between (1) the strange but well verified and repeatable features of quantum mechanics and (2) the pseudo-scientific, non-repeatable and non-verified so-called extra sensory perception.' But Wheeler's own views are likely to strike a non-physicist as being just as bizarre as those of the parapsychologists he deplores. Indeed, no one has yet formulated a consistent worldview that incorporates the Copenhagen Interpretation of Quantum Mechanics while excluding what most scientists would call pseudo-sciences—astrology, parapsychology, creationism, and thousands of other cults and doctrines.

The issue whether there is something besides unitarity—whether it can ever "breakdown" as you put it—is acute, but I think a lot of the problem hinges on how one views unitarity's status in a physical theory. I am inclined to believe that unitarity—or more generally "trace-preserving complete positivity"—does not breakdown either. But that is because I am inclined to view the time-evolution mapping one ascribes to a system as an epistemological entity (rather than an ontological one), much as I view the quantum state one assigns the same system. That is to say, I am struck that there is a deep reason one can make a bijective correspondence between the completely positive maps on one Hilbert space and the density operators in a larger space, a reason that has nothing to do with imagining ancillary systems: A CP map *is* a density operator, it is a state of knowledge in just the same way any other quantum state is.

There are other (peculiarly quantum) reasons for saying what I just did above, but it is also part of a larger program I have in mind (and one I think James had in mind too)—namely, to find a little slippage between the notion of a physical theory and the world itself. Below I'll quote a little piece I wrote to Carl Caves on the idea. It gets at the sentiment, even if not at the technical details! [See 04 October 2001 note to Caves-Schack, "Replies on Pots and Kettles".]

30-11-01 *Some Thoughts on Your Paper(s)* (to K. Svozil)

Thanks for your letter. You ask two good questions, and unfortunately I don't know the answer to either one! I will however have a good look at your papers and start thinking about them.

I think the Dutch-book argument for probabilities is not so strongly tied to the Boolean structure of propositions (as, say, Cox's argument for the probability axioms is). For instance, just look at Eqs. (1) and (2) in our paper. It seems to me that that part of the argument does not care one iota whether there is a distributive law for this event structure or not. If that's the case throughout the remainder of the Dutch-book mechanics, then maybe this is an important point.

05-12-01 *Dear Prudence* (to C. M. Caves & R. Schack)

Fellow Bayesians,

(Let's see if Carl's knowledge of 1960s music can help him guess the origin of this note's title.)

Anyway, to the real subject. Dutch-book coherence? Dutch-book consistency? Neither of them seem to be an accurate account of what's going on with the theorem. I know neither one of you will accept it, but I think "prudence" gets significantly closer to the mark.

- **Consistent** – 1. In agreement; compatible: "The testimony was consistent with the known facts." 2. Being in agreement with itself; coherent and uniform: "a consistent pattern of behavior." 3. Reliable; steady: "demonstrated a consistent ability to impress the critics." 4. In mathematics, having at least one common solution, as of two or more equations or inequalities.
- **Coherence** – 1. The quality or state of cohering, especially a logical, orderly, and aesthetically consistent relationship of parts. 2. In physics, the property of being coherent, as of waves.
- **Prudent** – 1. Wise in handling practical matters; exercising good judgment or common sense. 2. Careful in regard to one's own interests; provident. 3. Careful about one's conduct; circumspect.
- **Prudence** – 1. The state, quality, or fact of being prudent. 2. Careful management; economy.

How did Mermin's talk go? How did Rüdiger's talk go?

07-12-01 *A Sinking Ship?* (to C. M. Caves & R. Schack)

Fellow Bayesians,

Especially in light of what Carl wrote me yesterday, perhaps you should both have a serious look at Gavriel Segre's Ph.D. Thesis at `http://xxx.lanl.gov/abs/quant-ph/0110018`. van Enk pointed it out to me this morning, and believe me it'll be worth your time! In particular, pay attention to Theorem 5.2.21 "Impossibility of a Subjectivistic Bayesian Foundation of Quantum Probability Theory" on page 193. Maybe we ought to get off this ship before it sinks?

Also, make sure you don't miss reading his acknowledgments on page 5. This is surely the best quote I've ever seen on `quant-ph`!

07-12-01 *A Sinking Ship?, 2* (to C. M. Caves & R. Schack)

Schackcosm 54: *Are you serious? Of course, the thought has crossed my mind.*

The sinking ship remark was a joke. The reference to the (unaccepted) thesis was a scoff (or at least a little). You did read the "acknowledgment" of the thesis, didn't you? Also look at the other contents of the "thesis."

I have never been more confident about anything scientifically than that we are on the right track in our quantum Bayesianism. No Bayesian that I've ever read called a probability assignment a "state of knowledge." What's different about quantum states? Why should they be above the "degree of belief" that every classical assignment is? Taking the "state of belief" appellation seriously is simply the cross we have to bear ... at least as I see it. (Sorry for the Christian motif, Carl.) But I'm also confident that it's not that bad anyway; it's just a question of packaging.

10-12-01 *Lost It* (to C. M. Caves)

I'll try to get in touch with you later today. Sorry we missed each other over the weekend.

Cavesism 52: *I managed to lose your last e-mail, where you expressed the view that no Bayesian views a probability as based on a state of knowledge. In the absence of your full statement, here's a thought on that point.*

I don't balk against "facts" having a say in determining probability assignments. I balk against "facts" *uniquely* determining them.

10-12-01 *The Spirit of Gandhi* (to N. D. Mermin)

Well, you took away some of my fun with the letter you just sent me! Over the weekend, I ran across your talk at the ITP and listened and watched the whole thing: In fact, I was going to send you some direct comments on it. Now some of my steam is taken away.

Still, let me gingerly point out some things I had wanted to point out earlier ... even if they may not be quite as relevant anymore.

Somewhere around 36 minutes into the talk—actually it was in your reply to a question (which sounded to come from Jeff Kimble)—you said:

Merminition 73: *Our presentation, at least in the paper we submitted on the web, can be read as being tinged with a view that quantum states are more than a reflection of knowledge ...*

the implication seeming to be that in the present talk you strove to get around that.

However, at 23 minutes into the talk you said this:

Merminition 74: *Of course there's the question of what it means for the combined knowledge of all observers to constitute a consistent body of knowledge about S, which is an interesting question. I'm taking a kind of dumb-physicist view, which is that there should be—at least in principle—there should be one observer who has a lot of data about various measurements and mutually commuting observables made on The observer having access to all the data will realize that if ...*

I know I have said all this before, but let me "focus in" on these two passages and try to say it again (per your warning about me "focusing out" too often). I would say that what you said in the second passage has nothing to do with it being a "dumb-physicist view" of things. It is, however, the sine qua non of a view "tinged" with making the quantum state more than a simple epistemic entity. For it in essence says, there SHOULD BE (your words) a "right" quantum state, or a range of "right" quantum states, and that has nothing to do with any ACTUALLY EXISTENT observers.

Please do think about the similarities between your "dumb-physicist view" and the old limerick:

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

REPLY

Dear Sir:

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

Yours faithfully, God.

I would say that in supposing THERE SHOULD BE a superobserver, you are supposing that the quantum state should—in essence—already be there without any observer at all. When Bishop Berkeley ran into trouble with the question of where the trees go when there's no observer, he invoked the idea that God was there all along. Your superobserver is in essence a God, who—through his own objectivity—re-endows the quantum state with an objectivity I THOUGHT you were trying to get rid of in the first place.

But maybe that was never your goal.

Martin Gardner said something very clear in this regard in his essay "Why I Am Not a Solipsist," and so let me quote it:

> In this book I use the term "realism" in the broad sense of a belief in the reality of something (the nature of which we leave in limbo) that is behind the phaneron, and which generates the phaneron and its weird regularities. This something is independent of human minds in the sense that it existed before there were human minds, and would exist if the human race vanished. I am not here concerned with realism as a view opposed to idealism, or realism in the Platonic sense of a view opposed to nominalism or conceptualism. As I shall use the word it is clear that even Berkeley and Royce were realists. The term of contrast is not "idealism" but "subjectivism."

(The phaneron, by the way, was C. S. Peirce's term for "the world of our experience—the totality of all we see, hear, taste, touch, feel, and smell.")

If I were to give the BFM paper and your quest to make sense of Peierls (and Bohr and Heisenberg, as evidenced by your talk) a reading, I would say that what you are trying to do is give the quantum state an idealistic interpretation (via the word "knowledge") and thinking that that somehow contrasts with a realistic interpretation (and in doing so might fix everything up quantum interpretation-wise). But—in analogy to Gardner—for me, "realistic" and "idealistic" interpretations of the quantum state amount to the same thing. What I'm worried about is whether one can make any sense of the quantum state at all without simultaneously positing the active agent who makes use of it—I would claim you can't. And thus I'm left with the kind of "subjectivism" (for the quantum state) that frightens so many people.

PS. In case you're wondering why I titled this note "The Spirit of Gandhi," it is because I am hoping you will think of it as a form of nonviolent protest. Too many times in your talk you pointed out what a violent reaction I had to the BFM result, and I just can't think that that helped my reputation as a rational thinker or the seriousness with which a Bayesian-kind of quantum-foundation attempt should be viewed. (But I love you just as much as ever—maybe more with all the advertising you gave me—and there are no grudges.)

10-12-01 *Trumps and Triumphs* (to N. D. Mermin)

Also,

Merminition 75 : *I did urge Carl and Rüdiger to do something about the phrase (in your other paper) "Gleason's theorem is the greatest triumph of Bayesian reasoning" which I read as a claim that Bayesian reasoning is the only way (or at least far and away the best way) to derive and understand Gleason's theorem. They explained that what you really meant was that Gleason's theorem, by providing stringent constraints on possible prior distributions, provided a powerful tool for Bayesian reasoning. But I think they did an inadequate job of removing the ambiguity, which is too bad, since I am rather sure it will put off most of the people you ought to be addressing.*

I'm glad you urged them. Here's what I had said to Carl and Rüdiger in a September 20 note:

> Another thing I guess I really didn't like—and this is only a new one for me, it's not something that had eaten away at me before—is the slogan "Gleason's theorem can be regarded as the greatest triumph of Bayesian reasoning." To say that is to imply that Bayesian reasoning IMPLIES Gleason's theorem. I don't think you mean that. I think what you mean is that it is one of the most valuable additions to Bayesian reasoning ever. But I won't cause trouble here: I know you both like the saying.

But they generally just view me as a trouble-maker.

10-12-01 *Reread (pronounced re-red)* (to N. D. Mermin)

I'm just reading through all my emails of the day again.

Merminition 76 : *P.S. Spent a lot of time talking with Carl and Rüdiger in SB, which I enjoyed very much. I think the first half of your draft joint paper formulates the basis for the "necessary" condition of BFM much more coherently than we did: i.e. it is necessary if there is to be any density matrix that does not contradict anybody's strongly held beliefs. Much better than talking about "subsets of a body of data that anybody with access to all of which would agree constitutes nothing but valid results of measurements, none of which invalidate earlier measurement outcomes...." It's hard even to put into a grammatical sentence. The other four cases strike me as interesting extensions of the case we (and Peierls) were addressing, which concerned existing knowledge prior to any subsequent interventions. My feeling was that there was no serious disagreement among the three of us, but maybe they will give you different reports.*

Thanks, that's a very nice compliment. Here's another way to put it, which might be my preferred way. Suppose two strongly consistent agents want to stand a chance of coming to agreement, just by talking to each other. Then BFM is the necessary condition. If it is not satisfied, then the only way they will ever get anywhere is by "consulting" the system with a (mutually agreed upon) measurement: With it, they can bend the world into something more congenial to both.

14-12-01 *Strong Consistency* (to C. M. Caves & R. Schack)

I know that I'm terribly slow to be coming up to speed on this project. But now I have another possibly frivolous question.

What was the origin of strong consistency? What motivated Rüdiger to invent the concept? And indeed, did either of you ever find it appearing in the literature otherwise? It, of course, has a very different flavor than normal Dutch-book—I know that's no surprise to you—but for me, for the moment anyway, it seems to me to be introduced more for mathematical nicety than anything else.

(I.e., It seems more to be introduced just for the purpose of cleaning things up a bit axiom-wise.) It doesn't have the same operational/pragmatic flair that the rest of Dutch book has.

Indeed, it might be useful to lay this sort of issue on the line in the paper.

I can think of at least one case where one would never want to enforce strong consistency, namely for infinite event spaces. Consider an infinite number of draws from an i.i.d. distribution. One has that the probability of a typical sequence is one; however one should never then say that that implies the bettor believes a typical sequence will occur with certainty.

Reading ever so slowly (but I hope thoroughly),

15-12-01 *Bah, Humbug* (to C. M. Caves)

Humbug. Did I ever tell you I hate Christmas: It's the most intrusive holiday I know of, with people pushing me all over with obligations that they've invented for me. (Susie, Kiki's mom, arrived yesterday, by the way.) Now Thanksgiving, that's my favorite holiday: you cook, you eat, you watch a parade, and you end up rubbing your belly in a deep soporific ecstasy.

By the way, speaking of rubbing bellies, Kiki went into false labor last night. At first, when it was over, I thought, "Thank God it's false." But now, in the light of the morning, I'm thinking, "When are we ever going to get this over with?"

Cavesism 53: *There is evidence that it's out there in the literature (Sklar mentions it without any references), but we haven't dug up any references. In fact, Rüdiger and I had the impression that you might know where to find references.*

This is one of my biggest gripes about not being in a university. I don't have easy access to anything outside of the physics and mathematics journals. (In fact strangely, last night I dreamed that I was back at the University of Texas, surrounded by books, books, books on all subjects, and I was telling myself how glad I was to be back.)

Did you have a look at the Kemeny and Lehman references I recommended in an October 27 note? That at least would be my starting point. Also Bernardo and Smith, as I recall, had a lot of references on Dutch-book—in particular a lot of references dissatisfied with the argument (as Bernardo and Smith, in fact, are).

Cavesism 54: *I don't know about the flair point. I'll grant that strong consistency isn't as compelling as ordinary consistency, but it still registers on the flair scale: in a single trial, you never win, but definitely sometimes lose. Moreover—and this is the crucial point—strong consistency is absolutely essential so that probability assignments and density operators incorporate sufficient information about one's beliefs that one can use them as surrogates for the beliefs in our arguments.*

I wouldn't say that it's not "as compelling", it's just of a different flavor. To say that it's not as compelling, means it's directly comparable to the Dutch-book argument. But it's not clear to me that it is. Presently it seems to me much more like a Cox kind of thing. It says (at least in one case) my outward commitment *must* reflect my internal belief—not because I will be imprudent if I don't do so, but because I don't want my friends to see any discrepancy between my beliefs and my actions.

I believe I understand what you call the "crucial point." But I'm guessing—just a psychological point—you're more attracted to it, because it gives rise to (a better version of) the BFM statement. Thus it gives the impression of giving something very solid. Whereas simple Dutch book lets everything fly in the wind—maybe that's the very thing I'm attracted to it.

In any case, I don't see anything wrong with exploring what strong consistency leads to. I'm just trying to get it straight in my head—and possibly also for the reader—what it's all about.

Cavesism 55: *Chris said, "Indeed, it might be useful to lay this sort of issue on the line in the paper."*

Perhaps you could be more specific. It seems to me that it is pretty well laid out in the paper, but you're looking at it with a fresh eye, so suggestions are welcome.

I'm sorry, I don't quite know how to be more specific right now. Maybe I would just like the reader to see more of the debate about it. And it should come across that it is (possibly) an invention of C and S, not de Finetti or Ramsey.

Cavesism 56: *You're absolutely right here: probability 1 cannot mean certainty and probability 0 cannot mean impossibility in the limit of an infinite number of draws from an i.i.d., but it's not clear to me that the problem is strong consistency. To apply probabilities to infinite sets, you need countable additivity for exclusive events, which doesn't follow from the finite additivity given by the Dutch book. To apply probabilities to continuous sets requires measure theory. These things are additional mathematical structure beyond what the Dutch book can provide. Perhaps the best philosophy is Rüdiger's: probabilities really apply only to finite sets of events; the generalization to infinite sets is an idealization, where additional mathematical structure is added to make things work nicely.*

Point well taken. But on the other hand, I'm still not convinced either. I think all three of us are aligned that the only thing really worth considering, conceptual-wise that is, are finite event spaces. However, it still strikes me as a chink in the armor that I can think of a limiting case where I would not want probability $1-\epsilon$ to have anything to do with internal certainty. That is to say, maybe there's a deep reason that Dutch-book consistency is NOT strong consistency ... and maybe we're missing something by plastering over its unsightliness so quickly.

Cavesism 57: *Let me know if you have specific recommendations. Rüdiger and I have agreed on a number of changes in presentation and a large number of additions on the POVM front. I will be attempting to incorporate these this weekend.*

I'm trying my best in the light of the present events. (But I'm sure you're not holding your breath for me.) The main thing I'm shooting for now, is to make sure I understand every aspect of the paper—in a defendable way, that is—before Mermin arrives Tuesday.

Did, by the way, you understand my issues with the *sufficiency* of the det = 0 conditions? Was I missing something?

19-12-01 *Katie Viola Fuchs* (to the world)

Dear Family, Friends, and Colleagues,

Katherine Viola Fuchs came to the world with Monday's sunrise, 17 December 2001. She was 8 pounds, 19.5 inches, and a delight to her parents' eyes—she is beautiful in every way. And like her big sister Emma, Katie has already made it clear that the future will not just unfold before her, but be made in crucial part by her presence and will.

With the greatest of expectations (and a modicum of pride), we send greetings to everyone on Katie's behalf.

Chris, Kiki, and Emma Fuchs

15-12-01 *One So Far* (to C. M. Caves)

Just looking at the end of Section III, I was reminded that BFM posted there final version of the paper on `quant-ph` this week. So, they have made some changes. It might be worthwhile to check that they didn't sneak in too drastically different a formulation of the question.

Also, there you (or Rüdiger) write, "In contrast, our derivation is couched wholly in terms of the beliefs of the parties and does not appeal to a real state of affairs; it is therefore preferable in a Bayesian approach to quantum mechanics." I'm not sure—at least as hinted by the present structure of the manuscript—that any nonBayesian we have a clue why it's a preferable statement of the problem. "What's wrong with letting the quantum state reflect an objective fact, that what cannot happen cannot happen?," the reader might ask. "Aren't these CFS guys just paining over inconsequential points?"

Personally, I would like to see some discussion, perhaps in the introduction or in Section III, along the lines of those Bernardo and Smith quotes that I like so much. I'll place them below in case you forgot. I think they give the whole motivation for the work.

Finally, the present manuscript gives pretty short shrift to the Peierlsian ideas that got David's blood up in the first place. Nor do we say much about the apparent inconsistency between the goals and the "solution" of BFM in relation to the Peierls quest.

02-01-02 *Solipsism Story* (to C. H. Bennett & J. A. Smolin)

Here's the other story from Martin Gardner's "Why I Am Not a Solipsist."

> Russell once spoke on solipsism at a meeting chaired by Whitehead. As Russell tells it in his autobiography, he said he could not believe he had written those parts of Whitehead's books which he (Russell) could not understand, although he could find no way to prove he hadn't.

04-01-02 *New Breach of Faith* (to C. M. Caves)

Cavesism 58: *I agree wholly with your statement in the following:*

> *Also, there you (or Rüdiger) write, "In contrast, our derivation is couched wholly in terms of the beliefs of the parties and does not appeal to a real state of affairs; it is therefore preferable in a Bayesian approach to quantum mechanics." I'm not sure—at least as hinted by the present structure of the manuscript—that any nonBayesian [would] have a clue why it's a preferable statement of the problem. "What's wrong with letting the quantum state reflect an objective fact, that what cannot happen cannot happen?," the reader might ask. "Aren't these CFS guys just paining over inconsequential points?"*

But I don't quite know what to do about it. Your B& S quotes are nice, but they will just get in the way, in my view, of getting people even to grasp the setting for what we're doing.

Perhaps you misunderstood: I am not asking that the Bernardo and Smith quotes actually be used in the text. What I am asking is that we find a way to convey to the reader why any of this is important. One thing you've got to realize is that more than once, I've heard Mermin describe the reception of the BFM paper with words like these:

Merminition 77: *Sorry, I had the feeling that those few who understood anything at all thought it was pretty obvious, but they were polite anyway.*

What—it seems to me—is our task, is to convey to any readers who had such a reaction as above that they were actually snookered by BFM. The Bernardo and Smith report of what Bayesianism is about builds a context for our efforts—they make it clear why one should expect a hierarchy of conditions (like the ones we explore in the paper) rather than an *absolute* answer.

It is in saying terse, "only established clique"-interpretable (i.e., only preformed-radical-Bayesian interpretable) things like, "In contrast, our derivation is couched wholly in terms of the beliefs of the parties and does not appeal to a real state of affairs; it is therefore preferable in a Bayesian approach to quantum mechanics" ... and just kind of leaving it at that, without any further buffer ... that is going to get us in trouble. Or at least, that is one of my continuing fears whenever I work with you and Rüdiger.

What is being laid in this paper is the groundwork for viewing the quantum state in a way that people—even the Bayesians among us—are not at all accustomed to: Namely, taking the quantum state's subjectivity absolutely seriously and to the extreme. I can't understand how leaning heavily on the motivation for this work can hurt the paper. Indeed it seems far more crucial than the technical results in Section V if you ask me: if no one cares about the results, no one will read them in the first place. (It's not like this stuff can, with almost a single word, be advertised as a new quantum algorithm like Shor's.)

What's a little annoying is that I know that you (from among the three of us) are the one most up to the task of writing a beautiful, yet businesslike introduction. The only thing it seems that you need to be convinced of is that people will actually read this paper if it is well-written (and won't read it otherwise). Strangely, enough, my wacky 45-page papers get read (or at least skimmed): Explain that. It's not the substance a priori (and maybe not even a posteriori), but it might be the style.

Finally, in a last-ditch effort to shore up this point, I'm going to perform a new breach of faith (and just reconcile it with St. Peter when the day comes). [...]

OK, I'll say no more on the subject.

Cavesism 59: *Example: Kimble sat through Rüdiger's talk at Caltech, nodding his head in agreement the whole time, but then it emerged in the discussion afterward that he thought each party had his own copy of the system. He and his student, Andrew Landahl, really had a hard time with understanding that there is any [difference] between "different states" and "different systems." They really just couldn't help using these interchangeability.*

This is an oddity I have encountered before with several people. I don't understand its origin, but it is weird. Even taking an extreme realist view that there are objective quantum systems and objective quantum states, one should be able to detect the matter-property dichotomy in that and not confuse the two. If you have a theory of the confusion's origin, I'd be interested to hear it.

04-01-02 *Once Again* (to C. M. Caves & R. Schack)

Cavesism 60: *It IS interesting that strong consistency is required so that the firm part of your belief structure can be read off your probability assignments. I would have thought that a Bayesian would want to know what assumptions are required to translate beliefs into probability assignments.*

Let me put the relevant part of my take on that down again:

> It [strong consistency] says (at least in one case) my outward commitment *must* reflect my internal belief—not because I will be imprudent if I don't do so, but because I don't want my friends to see any discrepancy between my beliefs and my actions.

"Outward commitment" means acceptable odds for the bettor. What strong consistency says is that there are cases I should lay my precise beliefs on the table (for public view), even if I didn't have to in order to avoid a sure loss.

Nowhere else does Dutch book do this. For instance—with standard Dutch book—I could internally believe $p(X, Y)$, but nevertheless only accept bets according to $q(X, Y)$ so long as both these are coherent assignments. Is it part of the Bayesian creed to also require honest reporting of my internal beliefs? Maybe it is: but it seems to me that that is something on top of Dutch book (and in fact we know that it is).

I'm not "against" strong consistency, I just want to understand what motivates it other than that it leads to more equations in our paper ... and thus leads to a more scientific look.

I'm going to try to get to a real library this weekend and dig up those J. Symb. Logic papers by Shimony etc. Maybe that'll demystify things for me.

07-01-02 *Correlation without Correlata* (to N. D. Mermin)

Let's see what kind of reaction the longer quote below gets out of you. "There are, so to speak, relations all the way down, all the way up, and all the way out in every direction: you never reach something which is not just one more nexus of relations."

> In the rest of this essay I shall be trying to sketch how things look when described in antiessentialist terms. I hope to show that such terms are more useful than terminologies which presuppose what Dewey called 'the whole brood and nest of dualisms' which we inherit from the Greeks. The panrelationalism I advocate is summed up in the suggestion that we think of everything as if it were a *number*.
>
> The nice thing about numbers, from my point of view, is simply that it is very hard to think of them as having intrinsic natures, as having an essential core surrounded by a penumbra of accidental relationships. Numbers are an admirable example of something which it is difficult to describe in essentialist language.
>
> To see my point, ask what the essence of the number 17 is—what it is *in itself*, apart from its relationships to other numbers. What is wanted is a description of 17 which is different *in kind* from the following descriptions: less than 22, more than 8, the sum of 6 and 11, the square root of 289, the square of 4.123105, the difference between 1,678,922 and 1,678,905. The tiresome thing about all *these* descriptions is that none of them seems to get closer to the number 17 than do any of the others. Equally tiresomely, there are obviously an infinite number of other descriptions which you could offer of i7, all of which would be equally 'accidental' and 'extrinsic'. None of these descriptions seems to give you a clue to the intrinsic seventeenness of 17—the unique feature which makes it the very number that it is. For your choice among these descriptions is obviously a matter of what purpose you have in mind—the particular situation which caused you to think of the number 17 in the first place.
>
> If we want to be essentialist about the number 17, we have to say, in philosophical jargon, that *all* its infinitely many different relations to infinitely many other numbers are *internal* relations—that is, that none of these relations could be different without the number 17 being different. So there seems to be no way to define the essence of

seventeenhood short of finding some mechanism for generating *all* the true descriptions of 17, specifying all its relations to *all* the other numbers. Mathematicians can in fact produce such a mechanism by axiomatizing arithmetic, or by reducing numbers to sets and axiomatizing set theory. But if the mathematician then points to his neat little batch of axioms and says, 'Behold the essence of 17!' we feel gypped. There is nothing very seventeenish about those axioms, for they are equally the essence of 1, or 2, of 289, and of 1,678,922.

I conclude that, whatever sorts of things may have intrinsic natures, numbers do not—that it simply does not pay to be an essentialist about numbers. We antiessentialists would like to convince you that it also does not pay to be essentialist about tables, stars, electrons, human beings, academic disciplines, social institutions, or anything else. We suggest that you think of all such objects as resembling numbers in the following respect: there is nothing to be known about them except an initially large, and forever expandable, web of relations to other objects. Everything that can serve as the term of a relation can be dissolved into another set of relations, and so on for ever. There are, so to speak, relations all the way down, all the way up, and all the way out in every direction: you never reach something which is not just one more nexus of relations. The system of natural numbers is a good model of the universe because in that system it is obvious, and obviously harmless, that there are no terms of relations which are not simply clusters of further relations.

To say that relations go all the way down is a corollary of psychological nominalism: of the doctrine that there is nothing to be known about anything save what is stated in sentences describing it. For every sentence about an object is an explicit or implicit description of its relation to one or more other objects. So if there is no knowledge by acquaintance, no knowledge which does not take the form of a sentential attitude, then there is nothing to be known about anything save its relations to other things. To insist that there is a difference between a nonrelational *ordo essendi* and a relational *ordo cognoscendi* is, inevitably, to recreate the Kantian Thing-in-Itself. To make that move is to substitute a nostalgia for immediacy, and a longing for a salvatory relation to a nonhuman power, for the utopian hope which pragmatism recommends. It is to reinvent what Heidegger called 'the ontotheological tradition'.

For psychological nominalists, no description of an object is more a description of the 'real', as opposed to the 'apparent', object than any other, nor are any of them descriptions of, so to speak, the object's relation to itself—of its identity with its own essence. Some of them are, to be sure, better descriptions than others. But this betterness is a matter of being more useful tools—tools which accomplish some human purpose better than do competing descriptions. All these purposes are, from a philosophical as opposed to a practical point of view, on a par. There is no over-riding purpose called 'discovering the truth' which takes precedence. As I have said before, pragmatists do not think that truth is the aim of inquiry. The aim of inquiry is utility, and there are as many different useful tools as there are purposes to be served.

Common sense—or at least Western common sense—has trouble with the claim that numbers are good models for objects in general because it seems counterintuitive to say that physical, spatiotemporal objects dissolve into webs of relations in the way that numbers do. When numbers are analysed away into relations to other numbers, nobody mourns the loss of their substantial, independent, autonomous reality. But things are different with tables and stars and electrons. Here common sense is inclined to stick in its toes and say that you cannot have relations without things to be related. If there

were not a hard, substantial autonomous table to stand in relation to, e.g., you and me and the chair, or to be constituted out of hard, substantial, elementary particles, there would be nothing to get related and so no relations. There is, common sense insists, a difference between relations and the things that get related, and philosophy cannot break that distinction down.

The antiessentialist reply to this bit of common sense ...

08-01-02 *Correlation without Correlata, 2* (to N. D. Mermin)

Merminition 78 : *I don't like the stuff about 17 — very unconvincing. But after that it gets more interesting. It starts to get most interesting just as you cut it off.*

Of course it's not convincing: philosophical mumbles—it seems to me—can't serve that purpose. I was just curious whether it struck any chords with you on how to convey (whatever it is you've been trying to get at with) your "correlation without correlata." Is it a good metaphor? Does it carry any Ithacan soul?

Personally, I thought the analogy was nice from the second I read it. Even the primeness of a number, for instance, can only be defined by invoking the existence of all the other numbers. Numbers just don't have any properties in and of themselves (or at least none that I could think of).

You're not going to QIP at IBM next week, are you?

08-01-02 *Information → Belief → Hope ??* (to N. D. Mermin)

I started reading through the essay again and realized that if I were to fulfill your request, I might just have to copy the whole damned article. I give up! But at least I was kind enough to check that you have the source in your library at Cornell (call number and location below).

The essay is titled "A World without Substance or Essences." It should probably be read in conjunction with the essay previous to it, "Truth without Correspondence to Reality." The book as a whole is entertaining, but it is a little skimpy on firm argument. (Though, the author admits it is an attempt at popularization ... so that kind of makes it OK in my mind.) He's certainly not the devil that Steven Weinberg labelled him. But I will admit, his version of pragmatism may go too far for my tastes: I'll hold on to the final verdict for a while. (William James is still the best bet in my eyes.)

PS. The title to this note is a joke, based on Rorty's own title (and some of the discussions in his book). However, I am seriously toying with the idea of making a distinction between "beliefs" and "commitments." I.e., saying that a quantum state ascription is explicitly a "commitment" rather than a "belief"—a commitment to behaving one way or the other in the face of some experimental data (yet to be gathered). Sometimes commitments explicitly correspond to beliefs (as Dutch-book takes to be a definition), but it seems to me not always the case.

But as I say, I'm just toying with the idea: I know Caves and Schack will have a cow and beat me up if I have enough nerve to say anything about it. So, I'd better be sure of myself.

08-01-02 *Help with ET Quote* (to C. M. Caves & R. Schack)

Cavesism 61 : *I've been trying to remember without success where ET discusses his ability to toss coins a la what we saw from Dan Greenberger in Växjö. Can you help?*

I don't know that I've ever actually read the example: I've just heard stories of it (probably through Rüdiger), and maybe words to the affect as a section heading or something. I have a faint memory that it was somewhere in Jaynes's big book, but I couldn't it in the table of contents. Rüdiger's probably a better person to ask.

One interesting thing I did find in looking for the answer to your question though, is a Jaynesian diatribe against a Dutch-book foundation for probability theory. It's in Appendix A of his book. The fear he expresses there strikes me as not so different from what I was reading into your worries about my fall into "radical Bayesianism."

08-01-02 *Term Origin* (to C. M. Caves & R. Schack)

Here's a little (unfortunately inconclusive) discussion on the origin of the "Dutch book" term: `http://www.fee.uva.nl/creed/wakker/miscella/Dutchbk.htm`.

08-01-02 *Rorty on Religion* (to J. W. Nicholson)

I wish I had had these quotes in stock when we were having our conversation the other night. But I hadn't gotten that far in the book yet ... if I were only a quick reader like you. [Disclaimer: My copying these quotes for your thought (and our continued discussion) neither represents an endorsement for or against their content.]

From the essay "Religious Faith, Intellectual Responsibility and Romance" in the R. Rorty, *Philosophy and Social Hope*:

page 153:

> If one accepts that claim, one will have reason to be as dubious as James was of the purportedly necessary antagonism between science and religion. For, as I said earlier, these two areas of culture fulfill two different sets of desires. Science enables us to predict and control, whereas religion offers us a larger hope, and thereby something to live for. To ask, 'Which of their two accounts of the universe is true?' may be as pointless as asking, 'Is the carpenter's or the particle physicist's account of tables the true one?' For neither question needs to be answered if we can figure out a strategy for keeping the two accounts out of each other's way.

page 156:

> Pragmatists are not instrumentalists, in the sense of people who believe that quarks are 'mere heuristic fictions'. They think that quarks are as real as tables, but that quark talk and table talk need not get in each other's way, since they need not compete for the role of What is There Anyway, apart from human needs and interests. Similarly, pragmatist theists are not anthropocentrists, in the sense of believing that God is a 'mere posit'. They believe that God is as real as sense impressions, tables, quarks and human rights. But, they add, stories about our relations to God do not necessarily run athwart the stories of our relations to these other things.
>
> Pragmatist theists, however, do have to get along without personal immortality, providential intervention, the efficacy of sacraments, the Virgin Birth, the Risen Christ, the Covenant with Abraham, the authority of the Koran, and a lot of other things which many theists are loath to do without. Or, if they want them, they will have to interpret

> them 'symbolically' in a way which MacIntyre will regard as disingenuous, for they must prevent them from providing premises for practical reasoning. But demythologizing is, pragmatist theists think, a small price to pay for insulating these doctrines from 'scientific' criticism. Demythologizing amounts to saying that, whatever theism is good for, it is not a device for predicting or controlling our environment.

page 157-158:

> I said earlier that many readers of 'The Will to Believe' feel let down when they discover that the only sort of religion James has been discussing is something as wimpy as the belief that 'perfection is eternal'. They have a point. For when Clifford raged against the intellectual irresponsibility of the thesis, what he really had in mind was the moral irresponsibility of fundamentalists — the people who burnt people at the stake, forbade divorce and dancing, and found various other ways of making their neighbours miserable for the greater glory of God. Once 'the religious hypothesis' is disengaged from the opportunity to inflict humiliation and pain on people who do not profess the correct creed, it loses interest for many people. It loses interest for many more once it is disengaged from the promise that we shall see our loved ones after death. Similarly, once science is disengaged from the claim to know reality as it is in itself it loses its appeal for the sort of person who sees pragmatism as a frivolous, or treasonous, dereliction of our duty to Truth.

09-01-02 *Doin' the Dutch-Book Zombie* (to H. Mabuchi)

Hey MacArthur boy,

It's a funny thing being a parent of a newborn child. It leads to a kind of zombie-like state most every night: You're never neither really awake nor asleep. And then your mind gets hung up on some little thing—tonight being the Dutch-book argument—and you just repeat it over and over, as if a trance. Does that build any imagery for you?

Anyway, in looking up Andrew's email address last night (or was it this night? — time stops), I ran across your web page. The flattery to be listed among your collaborators! (Keep it there!) But you need to get the affiliation right — Bell Labs, Lucent Technologies.

Philosophically, lately, I've been taken away by William James and John Dewey. I've been reading their stuff with a pretty voracious appetite (and there is a heck of a lot of it). But of course one thing leads to another—just like that marijuana—and last week I found myself picking up a copy of Richard Rorty's *Philosophy and Social Hope*. His flavor of pragmatism goes maybe too far even for me, but he writes well and I find it easy to read him. I guess I just write you all this to tell you he speaks highly of your friend Derrida! One of these days, I am going to get the nerve to approach that man. (By the way, Mermin told me recently that your old professor Bas vF has been reading my papers ... and, apparently, disagreeing with them ...)

I'm going to slip back under the covers now; like Nosferatu I keep a little fresh earth there for comfort.

Just a sleepy note to let you know I miss you sometimes.

> He did the mash
> He did the monster mash
> The monster mash
> It was a graveyard smash

13-01-02 *Princeton Envy* (to H. Mabuchi)

[Mabuchi said,] "sounds like domestic life is treatin' you good!"

Wasn't it Freud who said, once a school girl sees your Princeton, she'll know what's missing from her life and be envious thereafter?

Well I'm not a school girl, but I saw your Princeton the other day (ahem), and it certainly did start a deep yearning in me. What a wonderful place! This was my first real trip there since moving to New Jersey, and I was enchanted all day. I found myself thinking, what I wouldn't give to live the rest of my life in this little cloister.

The thing that really struck me was the immense resources at one's finger tips. I found myself copying a little over $35 of articles in Firestone Library! I couldn't believe it: They had the complete collection of the *Danish Yearbook of Philosophy* and (shockingly) 35 years of the *Transactions of the Charles Sanders Peirce Society*, and I knew that those were just the tip of an iceberg. I was in heaven.

Emma and I play this game: I say, "When you go to college, I hope you'll go to Harvard." She says, "I want to go to Princeton." Or if I say Princeton, she says Harvard—it's always the opposite. (You can see a trend in our relationship starting to form.)

But that's just an aside (to tell you that the grass is always greener). Keep up the good work with all those good students. Get them to read William James' *Pragmatism* and tell them that quantum mechanics is a much better motivation for all that he said there ... but to never lose sight that the real goal is to get to where he wanted to go.

13-01-02 *Reality in the Differential* (to N. D. Mermin)

Merminition 79: *Actually I'm leaving it in the original form for now, but you're giving me a very hard time here.*

Let me say touché, and then little more than that. Strangely, actually, I've been having a little conversation of my own with You this week. And I've been planning to write you—i.e., the one with the little y—all about it. But what happens? Now that I've got a little time this Sunday morning, I'm finding that the inclination is leaving me.

So this note is going to come out far weaker than I had planned. The main thing was to build a conversation around another Rorty quote, and to tell you how pleased I am with the pragmatism movement in general. I'm finally finding a philosophy so close to what I'm looking for that I'm willing to advertise it to my friends.

Somehow they go a little too far for me, but there are so many beautiful gems I keep finding in their stuff that I find it better not to dismiss it outright. The second paragraph below struck me especially last week. It might as well be about my latest quantum interpretation thoughts.

The reality is in the differential. The quantum state represents a (gambling) commitment on the part of the agent; it never represents anything external to that agent. If you're looking for where the "reality" of the external world creeps into the formalism, you should look to how these commitments change. That's what I've been trying to say for a few months now ... but I guess you already understood that (as maybe witnessed by our last real conversation, during your visit to Morristown).

I had a pretty happy-sad week this week you might say. The happiness was that I visited Princeton for the first time since moving up here. I was like a kid in a candy store!! I ended up copying over $35 of articles in their wonderful library (which even subscribes to the *Danish*

Yearbook of Philosophy!). The sadness was in that I had to leave that environment and go home at the end of the day; it wasn't home itself.

I'm going to write you that longer James-Dewey note eventually. But right now I'll just leave you with a reminder that the reality is in the differential. From: R. Rorty, "The Pragmatist's Progress: Umberto Eco on Interpretation," in his book *Philosophy and Social Hope*, (Penguin Books, New York, 1999), pp. 131–147:

> As I see it, the rocks and the quarks are just more grist for the hermeneutic process of making objects by talking about them. Granted, one of the things we say when we talk about rocks and quarks is that they antedate us, but we often say that about marks on paper as well. So 'making' is not the right word either for rocks or for marks, any more than is 'finding'. We don't exactly make them, nor do we exactly find them. What we do is to react to stimuli by emitting sentences containing marks and noises such as 'rock', 'quark', 'mark', 'noise', 'sentence', 'text', 'metaphor' and so on.
>
> We then infer other sentences from these, and others from those, and soon—building up a potentially infinite labyrinthine encyclopedia of assertions. These assertions are always at the mercy of being changed by fresh stimuli, but they are never capable of being *checked against* those stimuli, much less against the internal coherence of something outside the encyclopedia. The encyclopedia can get *changed* by things outside itself, but it can only be *checked* by having bits of itself compared with other bits. You cannot *check* a sentence against an object, although an object can *cause* you to stop asserting a sentence. You can only check a sentence against other sentences, sentences to which it is connected by various labyrinthine inferential relationships.
>
> This refusal to draw a philosophically interesting line between nature and culture, language and fact, the universe of semiosis and some other universe, is where you wind up when, with Dewey and Davidson, you stop thinking of knowledge as accurate representation, of getting the signs lined up in the right relations to the non-signs. For you also stop thinking that you can separate the object from what you say about it, the signified from the sign, or the language from the metalanguage, except *ad hoc*, in aid of some particular purpose.

21-01-02 *R, B, and P* (to A. Peres)

Asherism 3: *I am happy that QIP2002 went well. I saw the papers of Briegel and Nielsen on quant-ph (please remind me the numbers, if you have them ready). I was not favorably impressed, maybe I misunderstood them, and I should read them again.*

Actually, the better work is the Raussendorf/Briegel stuff. If you were not favorably impressed, then I think you should give it another chance. I think it is a beautiful construction, and, in fact, the deepest thing I've seen in quantum computing for 2 or 3 years now. The first paper to start with is `quant-ph/0010033`. Then more details can be found in `quant-ph/0108067`, `quant-ph/0004051`, and `quant-ph/0108118`.

What is deep about this work is that all computations start off with the SAME given entangled state for the qubits. That is to say, a given entangled state is taken as a resource for the task. Thereafter the particular computation one is interested in is enacted by making single-qubit measurements alone: there are no further unitary evolutions. I think that is quite remarkable and quite lovely.

An interesting feature of the Raussendorf/Briegel model (as opposed to the Nielsen and Leung models) is that one need to take into account NO details of the post-measurement state for the measured qubits: they can be thrown away immediately after the measurement. And because the measurements are localized, the post-measurement state of the remainder of the qubits is fixed completely by the POVM (rather than the operation). That is to say, for the relevant qubits in this model, "an effect only has one operation."

Also: Again, I apologize about the typo that caused concern over Petra.

27-01-02 *What Would William James Say?, 1* (to J. W. Nicholson)

Of course I'm reading email in Texas! (Or at least that's what I'd say.) I'll send your regards to my Mom.

El Jeffy 1: *"the universe we observe has precisely the properties we should expect if there is, at bottom, no design, no purpose, no evil and no good, nothing but blind, pitiless indifference."*

That may have been true at one time, i.e., at some stage in the development of the world. That's how it got off to a start so to speak. But now, I go the experimentalist (who does the hard work in helping the theorist construct his theories), and ask him, "What ya doin?" He says, "Twiddlin' knobs." I ask, "How come?" He says, "I'm tryin' to hep this wacky friend of mine. He wants to get a theory of how iodine reacts to this and that? I'm chartin' it out, givin' him some clues." I ask, "How do you do that?" He says, "By twiddlin' these knobs."

The present stage of the universe doesn't look like it's full of blind indifference to me.

The initial condition is always left separate from the theory; there's a reason for that. How else would the experimentalist be able to twiddle his knobs?

27-01-02 *What Would William James Say?, 2* (to J. W. Nicholson)

El Jeffy 2: *Anyway, I was far more interested in your reaction the quote requiring God's absence from standard scientific theories.*

The point was, it doesn't even require the scientific agent's absence from the ultimate gears and pinions of the world, much less a god's. The scientific world view consists essentially of two components: theories and initial conditions. And, it seems to me, it is a tacit assumption of the whole scientific enterprise that the experimentalist can freely set the initial conditions he wishes to.

El Jeffy 3: *If the universe really wasn't blindly indifferent, people wouldn't fly airplanes into buildings, convinced they were on their way to heaven.*

There's something about this sentence I just don't like. I'll try to put my finger on it in the next couple of days.

29-01-02 *Qunix* (to J. M. Renes)

Renesism 4: *in other news, i discovered a really interesting paper (actually an undergrad thesis) by a guy at oxford, who argues against deutsch's view of the quantum world. one thing he takes to task is the notion that information is physical (which i think i've now discarded).*

Yeah, I met Timpson in Ireland and really enjoyed his company. I'll definitely look at his thesis. Right before Rolf died, I had wanted to write him a letter telling how much I had come to disliking the phrase "information is physical." I wanted to tell him that I think a far more appropriate phrase would be "information carriers are physical." In fact, I told Charlie Bennett about this—at the time—and he told me, "Too late; Rolf just had some fraction of his brain removed last week."

Concerning myself, I've gotten further carried down the path of pragmatism. I've even read some of the Rorty blend now. Indeed my latest little epiphany hit me last week (during an operating-systems talk in our center) when I came up with the following slogan: "A physical theory really amounts to little more than a programming language." Its rules, its specifications are more about the ways we've come to naturally manipulate the world than anything intrinsic to that same world. I tried to say this in my Oct 4 letter to Carl ("Replies on Pots and Kettles" in the new mini-samizdat), but I think the new slogan says it better.

29-01-02 *Växjö Contribution* (to C. M. Caves)

You know, if my true love is philosophy—Bennett calls it theology in my case, actually—yours is certainly sports. You're just not going to let this go without one hell of a fight, are you?

Cavesism 62: *So it appears that I'm in a pickle with evolutions that are mixtures of unitaries, since quantum operations don't have unique decompositions. But of course, all the decompositions into things other than unitaries aren't of interest, and I've been able to "show" (a number of half-baked steps here) that mixtures of unitary EVOLUTIONS are unique, at least in a sense that's good enough for me. To put it more precisely, I've shown it for qubits and think I can go further.*

I'm wondering what the "sense that's good enough for me" is? For instance, I already think of the depolarizing channel, where it doesn't matter which x, y, and z axes I use for defining the Kraus operators. But you probably have something up your sleeve that will be more instructive than that.

Cavesism 63: *For pure states the problem shows up as an inability to make a clean distinction between objective and subjective probabilities,*

And for the channel maps, I would say the problem shows up as an inability to make a clean distinction between objective and subjective probabilities. [Just like my double footnote in the NATO paper, I meant this to be taken seriously.] The only difference now is that the probabilities are of a conditional type, $p(y|x)$. But I know that this is too cryptic for you to make any sense of it at the moment, and I already write you too much preachy email: I just need to try to write a damned paper and be done with it.

29-01-02 *A Summer Masterwork?* (to N. D. Mermin)

Merminition 80: *Funny that you should find Rorty appealing. I've liked a lot of what he says too, although he is Public Enemy Number 1 for most of the scientists engaged in the Science Wars. No Geneva convention for him!*

You emerge! It's so good to hear from you again. If you have read Rorty, then why in the hell did you never tell me, "Chris, what you're shooting for in quantum mechanics really sounds a lot like pragmatism!"? You know you really might have saved me—more importantly this program I

dream of—a lot of time! For instance, I might not have wasted years thinking that a pure-state assignment ought to be unique, if I had just read a little James, Dewey, and Rorty.

Anyway, I continue to go further off the deep end. Here's what I wrote Renes this morning:

> Concerning myself, I've gotten further carried down the path of pragmatism. I've even read some of the Rorty blend now. Indeed my latest little epiphany hit me last week (during an operating-systems talk in our center) when I came up with the following slogan: "A physical theory really amounts to little more than a programming language." Its rules, its specifications are more about the ways we've come to naturally manipulate the world than anything intrinsic to that same world. I tried to say this in my Oct 4 letter to Carl "Replies on Pots and Kettles" in the new mini-samizdat), but I think the new slogan says it better.

However, I write this letter for another reason. The way I view it, you play a unique role in our community: You really do "write physics" every bit as much as you exhort your readers to in that nice essay on your webpage. I say this because I would dearly love you to make a statement, a solid statement, of where you think quantum information can have its greatest impact on settling quantum foundations questions. Our community needs this kind of incitement, and you have the writing skills that might even convince someone to do something about it. Even if you wrote a paper of nothing but questions, it would be great and a great service.

Would you think hard about doing that? It could be your masterwork for the year. Jeff Bub and I have finally gotten to the point of organizing a special issue of SHPMP, and your contribution could help set a good tone for it.

Below, let me copy a couple of letters I've already written in this regard. They'll fill you in on all the details of the way Jeff and I see the project. [...]

What say you?

30-01-02 *Sweet Talk* (to N. D. Mermin)

Merminition 81: *Saying that I've read Rorty is a gross exaggeration. I've dabbled around and found a curious mixture of interesting and outrageous assertions. Beware of becoming a "postmodernist" yourself.*

Funny that so many are viewing this as a new kind of onslaught to science. By his own admission, Rorty is not saying anything particularly different from James and Dewey. And my reading is starting to confirm that. So, it looks like the train of thought started before the turn of the (last) century. What is postmodern about it?

That said, the only thing I have ever wanted is a sensible of view of what quantum mechanics is about. If it takes rearranging our thoughts about what the classical world (and classical physics) is about, then so be it. To that extent—I think—I run the danger of becoming a postmodern. The other day Hans Briegel was visiting and he saw James' book *The Meaning of Truth* sitting in the back seat of my car. He ended up asking what pragmatism is about. I said, "You know, these guys didn't know anything about quantum mechanics, but I might venture to say it can be summarized as a Copenhagen interpretation of classical physics."

Merminition 82: *Actually it's interesting the way foundations of QM have come up again and again in the science wars, from Shelly Goldstein's attack on all of physics in the notorious NY academy volume, to Mara Geller's attack on Bohr, to my occasional mutterings that nobody whose*

thought hard about foundations of QM could possibly think science is as simple as Gross and Levitt would like it to be.

1) What is the "notorious NY academy volume"? You've piqued my interest.

2) I love it: you made the same spelling slip in Mara Beller's name in an email of November 28, 1999!

3) Who are Gross and Levitt?

Merminition 83 : *As for your latest attempt to sweet talk me into doing something reckless, if I thought I had anything useful to say on that subject I would have said it, instead of beating "Whose Knowledge" into the ground (and possibly many feet under the ground). I will, however, keep the invitation in mind as I work my way through the second edition of my Qcomp course.*

If I thought it might help, I'd even say "Pretty please, with sugar on top." More seriously, I think the only thing that would be reckless would be for you to NOT use these post-65 years to act a little like a more sober version of John Wheeler and get the physics community going to its next great stage. I know that you believe that quantum information and computing have something deep to contribute to quantum foundations studies. Why do you believe that? Can you articulate it? Why do you think that Peierls might have been on the right track? Or do you even think that? Why have you changed your stand somewhat on nonlocality? Why have you privately backed out a little on your initial statement of the IIQM? What problems did you foresee? What problems were brought to your attention? What part of the interpretation still stands a chance of being useful? What troubles you about Everett and Deutsch's interpretation? What troubles you about Griffiths' interpretation? What troubles you about Bohm and Goldstein's interpretation? What troubles you about Zurek's interpretation? *All* of these people *claim* to have long since solved all the greatest mysteries of quantum mechanics. Why should we not be listening to them? Have you ever gathered all your thoughts on all these questions?

You can't tell me that you don't already have a LOT to say, even while ... all the while ... you are telling yourself that you have nothing useful to say. Most importantly, you have the means at your disposal for saying these things in a way that people will listen and think about them. Even a study in self-indulgence, i.e., a reflection on why you've come to the positions that you have and why you remain perplexed would be *immensely* useful to the community.

Have you ever analyzed why I send so much email to you? I mean, "you" in particular? If I had to put my finger on it, it would be for two reasons: 1) Because I've always felt that your high standard for your own writing has induced me to a higher standard for mine. And 2) because you are the least dogmatic and most clear-thinking devotee of quantum foundations I've ever met. It has made it a pleasure for me to discuss my ideas with you; it has induced me to sharpen and present in a more convincing fashion everything I have ever wanted to say. What I imagine for your contribution in this volume is that same charm working on a public (archival) scale.

I really want to be able to reserve you a spot in the volume.

02-02-02 *Colleague* (to C. G. Timpson)

Well, I still haven't read your anti-Brukner/Zeilinger paper, but I'm writing this small note to tell you that I just finished reading your undergraduate thesis *Information and the Turing Principle*. It's quite a work, and I very, very much enjoyed reading it!

I have to tell you, one of my first reactions after reading about the first third of the thesis was, "Finally there's something sensible coming out of Oxford!" It really was such a relief: I

had been thinking that Deutsch had essentially brainwashed everyone in the quantum information community there (except for possibly Hardy and Steane).

Of course, what I like most is what I see as a significant overlap between our attitudes toward scientific theories, the Church-Turing thesis, the homunculus fallacy, and the misleadingness of the slogan "information is physical." But I learned a lot from you, and you helped me sharpen several points.

Here were some of my favorite pages: 4, 6, 8, 16, 24!, 32, 35-36, 38!, 46!, 47-48!, 52!, 60, 65!, 66, and of course chapter 5. (An exclamation means that something especially intrigued me.)

I know there are a few places where I distanced myself from the phrase "information is physical" in my large Samizdat (`quant-ph/0105039`), but I'm having trouble finding them right now. One is in a letter to Bennett starting at the bottom of page 34. However, the most intriguing moment in my mind is one instance I have not recorded in email before. Let me record it here.

I was visiting Bennett at his weekend home in Wendell, MA one weekend and somewhere in the night I got on a roll about how much I had started disliking the phrase "information is physical." In place of it, I was arguing that a much better, much more accurate call-to-arms we ought to be sending the physics and information-theory communities is that "INFORMATION CARRIERS are physical." Taking that into account is what is behind all the new questions we are asking in quantum information theory. Then I told Bennett, "I am starting to plan a long email that I would like to write to Rolf on this subject. But I know that I'm going to have to word it delicately if I'm going to stand any chance at all of catching his ear and not getting an immediate dismissal as a fool." Charlie replied, "Too late; Rolf just had about a quarter of his brain removed last week." Then he explained that Rolf was just found with cancer in the brain, and that chances were strong he would be dead soon. About a week later Rolf died.

The Shanker 1987 paper looks especially intriguing to me. I'll try to pick it up the next time I'm at Princeton.

By the way, the homunculus fallacy struck me on several levels. I think one might view William James's argument against the correspondence theory of truth in his a little book Pragmatism as a little bit along the same lines, for instance. But it also got me to thinking about one of the things that has long bugged me in the Zurek-style versions of quantum foundations. There, the starting point is how bad the word "measurement" is and how it should be banished from the foundation of the theory. Yet, inevitably (just watch them), whenever push comes to shove in their explanation of the true importance of decoherence, to get the idea across, they start saying things like "in essence the environment 'measures' the system." (Zurek always makes little quote motions with his fingers when he says it.) And that's supposed to lead us to a deeper understanding of that tabooed subject?!?!? (I thought I had put that complaint in a footnote in some recent paper, but for the present I couldn't find it either! I must be losing my memory or losing my mind ... or both!)

Anyway, again, I really enjoyed the thesis. Keep up the good work.

P.S. I also had a look at your webpage. From the schedule of your discussion group, it looks like you're trying hard to make sense of the notions of 'objective chance' and 'propensity' to yourself. I went down that path once—roughly from 1991 to 1996—and it was instructive. I found that I couldn't buy any of the theories, and that's what ultimately pushed me down the long road to Bayesianism. I keep my fingers crossed that you meet the same frustration!!

05-02-02 *Fighting Windmills* (to C. M. Caves & R. Schack)

Thanks for the revealing note on Feller (which I've finally had a chance to read).

Schackcosm 55: *Chris, do you know about any other book or paper that explains a "modern understanding of probability"?*

No I don't really. In fact I don't even really have a strong understanding of where most physicists have gotten their prejudices toward probability. Maybe a good place to start in such a quest is to look at the discussions of probability in the main graduate and undergraduate quantum mechanics textbooks used today (Cohen-Tannoudji or however you spell it, Merzbacher, Liboff, Bohm, and whoever else).

All I remember from my undergrad statistics course was that the professor told us that the Bayesian flavor of statistics was nonsense. Unfortunately, I don't remember the text we used. In fact I got very little out of the course that I remember at all.

06-02-02 *The Commitments* (to C. M. Caves & R. Schack)

Schackcosm 56: *The other problem is that your conversation is far too playful. State assignments are compilations of betting odds. They are COMMITMENTS."*

Dear old friends,

I'm going to try to write a note today that essentially has been sitting in my head for over a month. I'm sorry that I've held on to it for so long, but the great difficulty has been that time has just been stolen from me left and right ever since a few days before Katie's birth. (Katie herself, by the way, is the least of my problems; she's really a dream of a child.) Anyway, I think there were times in this period where the issues were so at the top of my mind that this note could have (or would have) turned out far more passionate and, thus, maybe clearer. But that time has past. Still I'll give it my best shot with the time I can muster today, and also hope that it's not so late as to clash with anything Carl has written for the newest version of the post-BFM paper. Here goes.

In a nutshell, what I'm going to say is that I now take Schackcosm 56 absolutely seriously—indeed probably far more seriously than it was ever meant to be taken.

I used to say that quantum states are "states of knowledge," or "states of information." But, you both know that brooding over the BFM paper caused me to disabuse myself of that. Then I got into the habit of calling quantum states "states of belief" in analogy with what the more left-wing, de-Finetti-flavored Bayesians say of classical probabilities. Now, what I'm going to tell you is that in contemplating the points you *two* think are important in our reply to BFM, I've gotten into the habit of calling quantum states "states of commitment." A quantum state should be viewed most properly as a compendium of commitments, gambling commitments.

Indeed you guys almost nudged me directly there by your love affair (or at least Carl's love affair) with strong coherence. I'll come back to say what I mean by this in much greater detail in a minute, but for the moment let me bring the postulate into a clearing so that you can go ahead and start aiming your arrows at it:

> A quantum state corresponds to a compendium of gambling commitments (i.e., just like the gambling odds of a Dutch-book argument) one is willing to make in various given practical situations. However, the key new point is that these commitments are with respect to ALL THINGS CONSIDERED. There are times when one's commitments correspond to one's (internal) beliefs—and because of that the quantum state remains just as subjective as ever in my mind—but that need not always be the case. There are times (and these are probably the vast majority of all real cases) where the quantum state one ends up ascribing to a system is something less than a compendium of ANYONE's beliefs.

I know that I don't have to remind you that I tried pretty darned hard to choose all my words very carefully in that definition.

Now let me tell you about the sorts of thoughts that lead to this by explicitly replying to some of your old notes to me.

Rüdiger wrote:

Schackcosm 57: *I don't quite understand Chris's problem. Strong consistency DOES have a motivation which is very similar to ordinary consistency, and which DOES have the same flavor. It's just stronger. If you violate strong consistency, you are imprudent in the sense that you accept a bet in which, according to your own state belief, you never win, but you lose for at least one outcome that you believe is possible. That qualifies as imprudent, I think. Actually, I believe that the term "imprudent" fits better here than in the case of ordinary consistency, where "outright crazy" seems more appropriate.*

And then Carl wrote:

Cavesism 64: *I agree completely with the above, especially with the distinction between "imprudent" and "outright crazy." I think a violation of strong consistency is somewhere between imprudent and outright crazy, but I haven't been able to think up the right [word]. People who take imprudent actions expect to win big, I think; they are judged imprudent because soberer people can see that the chance of [a] big win is small, whereas the chance of serious losses is large.*

First, let me try to settle the issues of language in these two remarks, or at least try to say more clearly why I was concerned—actually in a rather offhand way in the beginning (see my note of 12/5/01, titled "Dear Prudence")—with the appropriateness of the usual terms, "coherence" and "consistency." Then I'll tackle the more substantial issue Rüdiger brings up before that.

I once had an officemate who committed suicide. That is an action I would call (and did call) "outright crazy." Seeing the pain and the soul-searching it put everyone through who was near him, I might even have called it "moronic." And that is important. For, the point I was trying to make explicit to you in my earlier (shoddy) note, is that that kind of craziness is of a much less absolute character than the kind of craziness one would be committing by asserting both A and $\neg A$. In the first case, the craziness is conditioned by one's culture and one's customs, or you might say by the instinct for one's survival. In the second, the craziness is in the breaking of a timeless, Platonic, a priori, ideal "law of thought."

To me, the word "inconsistency"—and therefore the word "consistency"—seems far more to connect to such an ideal Platonic stasis than one's willingness to be taken to the cleaners by a Dutch-bookie. Similarly with the word "incoherent." However, being Dutch-book coherent strikes me much more as an expression of the survival-instinct type than anything else. It is a formal expression of "thou shalt not commit suicide." Why should that be considered an inherently "logical" commandment? Why should it have the right to live in the Platonic realm of Boolean logic? If you ask me, I would say it is probably much more a manifestation of simple Darwinian evolution. We try to stay Dutch-book consistent—it is our ideal of behavior—precisely because of the survival tool it represents for our kind. However, we know by recent experience, that some cultures bask in the idea that, at times, there are reasons to override one's personal aversion to suicide. Do you know of a culture that, at times, finds it useful to override Boolean logic in its mathematical proofs?

When I wrote my original offhand note, I never imagined the backlash I would get from you guys. Nor did I intend to make another proposal to change established nomenclature. I thought I would just simply get a reply of the sort, "Yes, we understand your concerns, but in this case

it really is too late to change the nomenclature. A good fraction of the Bayesian community has been using the terms 'coherence' and 'consistency' for almost 70 years. The best you should hope for is an extra paragraph in the paper's appendix assessing why one ought to consider Dutch-book coherence compelling." And I'll still stand by that.

But I actually think there is a point of more substance in this—one that starts to bite much harder when one debates the relative merits of regular Dutch-book coherence versus its stronger cousin (i.e., strong consistency, strict fairness, or what have you). And the new problem is no longer just a problem of language. To say it again, Rüdiger writes:

Schackcosm 58: *Strong consistency DOES have a motivation which is very similar to ordinary consistency, and which DOES have the same flavor. It's just stronger. If you violate strong consistency, you are imprudent in the sense that you accept a bet in which, according to your own state belief, you never win, but you lose for at least one outcome that you believe is possible.*

You don't know how I ALMOST agree with the first sentence of this! In fact, I could agree with it completely if you would just let me change the first instance of "does" to "can."

> Strong consistency CAN have a motivation which is very similar to ordinary consistency, and which DOES have the same flavor.

I said before that strong consistency was of a different flavor than standard consistency, but let me be more careful now—i.e., now that you've helped me sharpen my point. It is not that the flavor is a priori different; it is that in paying the price of strong consistency, you actually get two flavors for the price of one. That is to say, I cannot agree that strong consistency is "just stronger"—it is that and something more.

Here is a very different (and ostensibly much less satisfying) way of motivating strong consistency:

> AXIOM: Whatever an agent's personalistic beliefs, i.e., whatever personalistic probabilities he has managed to write down in his head for some event, when placing a bet (with a Dutch-bookie or otherwise), he MUST place it precisely according to the gambling odds his beliefs afford.

Clearly a special case of this is that when a person believes $p = 1$ for some event, then he MUST bet as if he is certain that it will occur. I.e., he must be strongly consistent.

But you probably ask, "What is wrong with that?"

I will tell you. In doing such a thing, you would throw away loads of freedom, loads of wiggle room that the standard Dutch-book argument generally leaves in your command. In essence you throw away the freedom to concatenate your Dutch-bookie-game commitments with any of a number of other games. Or still another way to say it—though it's not so nice—you throw away your freedom to lie, even when it is in your best, overall, total interest.

An easy case to see this in is the "double agent" Dutch book argument that I wrote you about on 8 Sept. 2001 in a note titled "Negotiation and Compromise" (starting on page 64 of the mini-samizdat). Suppose that Kiki and I draw our money from the same bank account, and that for some given event I ascribe $p = 1$, whereas Kiki ascribes $p = 1/2$. We've talked about our disparity in assignments many times, but I simply cannot convince her to accept the evidence that led me to $p = 1$; she holds her ground, and I know she would do that in any circumstance, come hell or high water. Now suppose I'm later approached by a Dutch-bookie that I am fairly sure has already gambled with Kiki. What should I do? If I don't want to loose my shirt, then I had better not declare my adamant belief in the ultimate occurrence of the event we are betting. Instead,

I should adjust my GAMBLING COMMITMENT to agree with Kiki's (sorely) less than optimal one. (Sorely less than optimal from my point of view, that is.)

If on the other hand, you say that in order to play the game of Bayesian probability, I must be strongly Dutch-book consistent, then I no longer have the option to save my own fortunes in this new game. The point is, standard Dutch-book consistency concatenates well with such a further, ancillary game, whereas strong consistency does not.

But this is just a contrived example. Similar, but maybe better, examples can also be found by looking at the old "Keeping the Expert Honest" game (see pages 20–21 in Ph.D. thesis). And I'm sure there are still other more realistic situations that we could come up with if we just tried.

So, what I am saying is Dutch-book consistency is only the tiniest check on one's gambling strategies, and that is a GOOD thing. A very good thing. If one imposes too much structure—apparently almost anything more than simple DB consistency—then one will be left in a lurch in the real world, a world where negotiation and compromise are the keys to survival.

I truly, truly, truly hope you will see the point of this, but I guess I have gained enough experience in the last email war to be prepared for the worst. Here is what I am mainly afraid of. By holding fast to strong consistency as a reasonable addition to standard DB consistency, one ends up in the quantum case with a nice, tightly mathematical looking theorem (almost) in the BFM style. You cannot tell me that at least the Carl among you is not far more attracted to results like that, than the willy-nilly result one is left with if only standard DB consistency is enforced. At the very least, it makes the paper look far more constructive than destructive ... and that's got to be deemed a good thing, right?

What I guess I am saying is that, concerning what Carl wrote:

Cavesism 65: *I, as you know, like strong consistency and hope that in discussions of it, we can separate the a priori reasons for liking or disliking it from reasons based on the conclusions it leads to.*

I believe that I have done that. However, I hope that you will live up to these hopes too! Every bit of the discussion above (excepting the last paragraph) had nothing to do with quantum mechanics. The argument is purely classical and divorced from the BFM issue. In our post-BFM paper, I certainly have no problem whatsoever delineating the whole hierarchy of conditions, regular DB, strong DB, etc. What worries me though is how the present draft gives pretty short shrift to plain old regular DB — the very one that I myself find the most reasonable (for all the reasons above).

Of course, suspecting that you guys will try to wring me out after spouting this blasphemy, I had hoped to come armed with good knowledge of the literature. Unfortunately, as I already expressed way above, time hasn't been on my side these last few weeks. What I was at least able to do though, was run to the Princeton library one day and amass copies of a lot of papers. I ran from one paper to the next, following the citation trail that each gave. Below is the result: My complete collection of Dutch-book papers.

I wish I could say that I had read these, but for me the skim of a title and abstract doesn't count as "reading" a paper ... as it does for XXX, say. Nevertheless, a couple of points did stand out for me. The most important one is that maybe I'm not alone in thinking that strong consistency goes (far) too far. Hacking (1968) below writes, for instance,

> Abner Shimony called it *coherence*; John Kemeny called it *strict fairness*; today many people speak of *strict coherence*. According to Shimony's definition, a set of betting rates on a series of propositions h_i and e_i is strictly incoherent, when "there exists a choice of stakes S_i such that, if X accepts the series of bets at these stakes, then

> no matter what the actual truth values of h_i and e_i may be, X can at best lose nothing, and in at least one possible eventuality he will suffer a positive loss" De Finetti had a less demanding concept which is called *coherence*. A set of betting rates is incoherent if, no matter what the actual truth values of h_i and e_i may be, X will suffer a positive loss in every eventuality. Logicians usually think that Shimony has improved on de Finetti's concept of coherence, but statisticians, including de Finetti himself, have seldom been persuaded.

After that, he starts writing about nonBoolean algebras, and I haven't had a chance to try to decipher it. But what is really important to me is that this is the first indication I've run across that de Finetti himself thought about strong coherence and then rejected it. I ask, "Why?" I know that Carl has little use for relying on authority—I'm referring to his quick rejection of even wanting to hear de Finetti's opinion—but that is not what is at issue here. de Finetti is someone who thought long and hard about probability from the Bayesian view. I cannot see how it would not be worthwhile to at least hear his arguments. We might save ourselves time, and we might save ourselves from making mistakes that will make us feel foolish.

Then there is a large set of papers debating whether Dutch-book coherence really has to do with "rationality" or rather something else (as I alluded to above). The titles should give those papers away. I've hardly even skimmed those at all, but you can see from above where my present opinion lies. DB consistency defines an expedient for our actions, but it is hardly more rational or logical than that. Skyrms87 in particular, I am told, argues the opposite point of view. If that is a position that is near and dear to your heart, then maybe it would be worth understanding what he has to say.

OK, clearly I'm petering out and it's after midnight now. Soon I won't even be coherent myself; I'm already starting to see signs of it. Let me try to quickly summarize the whole argument in a few sentences, rather than checking and editing all the above to make it more eloquent, more complete, and more connected.

1. DB coherence strikes me as much more a pragmatic requirement than as any rule of rationality (as the law of the excluded middle is).

2. Thus one is more compelled to consider the pragmatic consequences of standard DB versus strong DB.

3. From that, one sees that strong DB is not just more of the same, but carries with it whole new flavors of behavior. In particular it forces us all to be little George Washingtons—"I cannot tell a lie"—when we have a $p = 1$ assignment in mind.

4. Eschewing that, I am forced to divorce our (pragmatic) gambling commitments from our actual beliefs. Our beliefs can be our commitments, but our commitments need not be our beliefs.

5. Thus it is better to say that "probabilities are our gambling commitments, ALL THINGS CONSIDERED." (with apologies to NPR)

6. Quantum states being compendia of probabilities are thus "states of commitment" full stop.

That's the argument. I'm sure this letter is riddled with typos, but I don't want to hold on to it anymore. France Telecom is coming Thursday and I've been tapped to convince them that

quantum information is interesting and that our dabbling in it is a little value-added perk they'll get if they stay our customer rather than running away to Alcatel. Can you believe that?

Anyway, maybe Rüdiger will be a little happy to see this when he gets into the office tomorrow ... to see that I haven't really abandoned you.

1. Brad Armendt, "Is There a Dutch Book Argument for Probability Kinematics?," Philosophy of Science **47**, 583–588 (1980).

2. Patricia Baillie, "Confirmation and the Dutch Book Argument," British Journal for the Philosophy of Science **24**, 393–397 (1976).

3. David Christensen, "Clever Bookies and Coherent Beliefs," Philosophical Review **C**(2), 229–247 (1991).

4. Barbara Davidson and Robert Pargetter, "In Defence of the Dutch Book Argument," Canadian Journal of Philosophy **15**(3), 405–424 (185).

5. Richard Foley, "Being Knowingly Incoherent," Noûs **26**(2), 181–203 (1992).

6. Bas C. van Fraassen, "Belief and the Will," Journal of Philosophy **81**(5), 235–256 (1984).

7. Ian Hacking, "On Falling Short of Strict Coherence," Philosophy of Science **35**, 284–286 (1968).

8. Frank Jackson and Robert Pargetter, "A Modified Dutch Book Argument," Philosophical Studies **29**, 403–407 (1976).

9. John G. Kemeny, "Fair Bets and Inductive Probabilities," Journal of Symbolic Logic **20**(3), 263–273 (1955).

10. Ralph Kennedy and Charles Chihara, "The Dutch Book Argument: Its Logical Flaws, Its Subjective Sources," Philosophical Studies **36**, 19–33 (1979).

11. R. Sherman Lehman, "On Confirmation and Rational Betting," Journal of Symbolic Logic **20**(3), 251–262 (1955).

12. Abner Shimony, "Coherence and the Axioms of Confirmation," Journal of Symbolic Logic **20**(1), 1–28 (1955).

13. Brian Skyrms, "Coherence," in *Scientific Inquiry in Philosophical Perspective*, edited by Nicholas Rescher (Center for Philosophy of Science, Lanham, MD, 1987), pp. 225–242.

14. Jordan Howard Sobel, "Self-Doubts and Dutch Strategies," Australasian Journal of Philosophy **65**(1), 56–81.

15. Lyle Zynda, "Coherence As an Ideal of Rationality," Synthese **109**, 175–216 (1996).

06-02-02 *How Did? What Did?* (to N. D. Mermin)

Merminition 84: *I don't remember the precise content of some of the issues you seem to be addressing, notably what is "strong consistency"?*

Then how on earth did you word your slide (where you gave our version of the theorem) when you gave your talk here in Murray Hill. Now my curiosity is piqued; maybe I didn't notice some inanity in your talk. One only gets something that looks even remotely like the BFM criterion if one assumes strong consistency. If you stick with standard Dutch book, you get precisely what I had been trying to tell you since the very beginning: namely, there are *no* constraints on the density operators required at all.

06-02-02 *Actually, the Both of You* (to C. M. Caves & R. Schack)

This morning I woke up and re-read the long note I sent you last night. In doing that, and looking back at your other notes again, I now think I was too harsh in singling out Carl when I wrote:

> By holding fast to strong consistency as a reasonable addition to standard DB consistency, one ends up in the quantum case with a nice, tightly mathematical looking theorem (almost) in the BFM style. You cannot tell me that at least the Carl among you is not far more attracted to results like that, than the willy-nilly result one is left with if only standard DB consistency is enforced. At the very least, it makes the paper look far more constructive than destructive ... and that's got to be deemed a good thing, right?

That's not fair to him. In particular, both Rüdiger and Carl wrote:

Schackcosm 59: *Of course it is nicer to base one's approach on the weaker concept. What we are discussing in the paper, it seems, is reasons for why the weaker concept does not quite give us what we would like. These reasons are secondary, they have nothing to do at all with the Dutch book justification of strong consistency itself.*

Cavesism 66: *Rüdiger's two paragraphs nicely illustrate what I was hoping for when I wrote that I "hope that in discussions of it, we can separate the a priori reasons for liking or disliking it from reasons based on the conclusions it leads to." The first paragraph is about the a priori reasons, and the second is about why its conclusions are important.*

Let me focus in particular on Rüdiger's sentence, "What we are discussing in the paper, it seems, is reasons for why the weaker concept does not quite give us what we would like." What is it that *we* would like? And, why would *we* like it?

Clearly, I am most happy with the willy-nilly result that standard DB coherence alone gives. I'm glad you guys made that nice and rigorous. For me, it says that B, F, and M were just way off the mark in trying to dictate what various observers MUST ascribe for their quantum states. It tucks nicely with my very first note to them where I registered some protest.

Now, what is nice about the strong coherence version of the argument is that it does at least get us the "if" part of BFM, and thus gives us something solid with which to compare to their campaign. But I suppose, I have always viewed that as a secondary, rather than the primary, point of our criticism. It's role is simply in that it tells us what we have to ADD to pure (de Finetti flavored) Bayesianism, to get something that comes close to resembling BFM.

So, my apologies to Carl, and my consternation to the both of you!

06-02-02 *Definition* (to C. M. Caves & R. Schack)

Let me go back to the NPR thing briefly while I've got a couple of moments. "All things considered," what does it mean?

I know that your knee-jerk reaction is going to be that it is a hopelessly vague term. However, I'm going to suggest that it is no more and no less vague (and mysterious in its origin), than "belief" was in the first place. It is just broader in scope. In fact, I think it amounts to little more than the sum total beliefs one possesses. (What more could one consider?)

Thus in setting a quantum state, one sets it according not only to what one believes about the system of particular interest ... but also according to the situation one believes he will be encounter in the laboratory, the purposes of the information he will gather and how it will be used, who will share that knowledge so gained, etc., etc. It may even depend upon how the financial markets are doing, which political party happens to be in power, and so forth.

It just takes seriously the idea, that to any quantum system, what we say about it—i.e., what we are openly willing to bet on it—depends upon many things beside the system itself.

Maybe all of this goes back to a conversation I had with Marcus Appleby while I was in Northern Ireland, just after 9/11. I tried to explain my new point of view—that a quantum state is a state of personal belief—and he replied that he though that had the right feeling, but that he had some kind of "unpinpointable" fear that maybe the idea didn't go far enough. He seemed to be saying something to the effect that beliefs can never truly be considered in isolation from other beliefs. I didn't understand his worry at the time, but I think everything I wrote you yesterday starts to pick up on this line. In this regard, by the way, Appleby suggested I read a book by Michael Polanyi, *Personal Knowledge*. (Polanyi was apparently also a chemist of some renown, I believe he said.) Though I haven't had a chance to dig it up yet.

In any case, the COMMITMENTS one (potentially) makes in the sum total of all gambling situations quantify the quantum state. They give it an operational meaning—in terms of its "cash value"—which goes beyond niggling over the details and merits of "beliefs" versus "culture" versus "all relevant things considered," etc.

06-02-02 *The Great Quantum Well* (to C. M. Caves)

Cavesism 67: *Not willing to discuss the technical issues much at present because I'm not going to have time to flesh things out for a while. I get a long way by not being interested in Kraus decompositions, which aren't generally convex combinations of unitaries, and I get a good deal farther by insisting on time evolutions, not just single-time stuff. The rest is going to come from having available some operators that rigidify (and thus provide a physical interpretation) for the vectors in Hilbert space, without which the quantum questions (and Hamiltonians) aren't anchored to anything at all.*

I think the last sentence really captures what sets our goals apart. The image I carry around in my mind presently is that the quantum system is very much like an oracle. We ping it, and it provides us with something that we did not have before, something that we could not foresee ... and therein lies its reality. Therein lies its independence from us. Each quantum system is an instantiation of your great quantum well.

But the "interpretation," the "meaning," of the gifts those oracles give us is set from the outside. Completely from the outside. Or at least that's the point of view I'm pursuing.

I just don't see how that line of thought lies anywhere along the lines of your accusation that quantum questions will thus not be "anchored to anything at all."

08-02-02 *Samizdat and Dutch Book* (to B. C. van Fraassen)

I was talking to David Mermin a while ago and he mentioned that you had brought my name up in some email and seemed to be aware of some of my papers. (I hope I got the story straight.) Anyway, that piqued my interest: If true, then you are very likely the only philosopher who has ever noticed my existence! I would certainly love to hear your opinion—both con and pro—of my quantum foundational thoughts (forming as they are). Having the critique of a true-blue philosopher would be most useful for steering me to clarity, or even steering me away from the abyss!

My two most close-to-philosophical pieces can be found on the Los Alamos preprint archive: `quant-ph/0106166` and `quant-ph/0105039`. Or, they can both be found at my (almost empty) website `http://netlib.bell-labs.com/who/cafuchs/index.html` with a couple of other pieces of supporting material.

Also I'm in Princeton from time to time making use of your wonderful library system. Maybe I could drop by for a chat? (Sometime in March or later, actually, since I'll be in Japan until essentially then.)

By the way, I was trudging through various Dutch-book arguments recently, and I came across your paper "Belief and the Will," [Journal of Philosophy **81**(5), 235–256 (1984)] in the process. It looks good. I'll let you know if I form an opinion.

11-02-02 *Oh Modern Wittgenstein* (to N. D. Mermin)

Concerning points 1 and 2 in your Tractatus Quantico-Philosophicus, read the anecdotes below. [See letter to Chris Timpson, dated 2 February 2002.]

I exclusively use the word "qubit" for the physical system, i.e., the "carrier" of the information, i.e., the object of one's belief, i.e., the oracle the receiver consults at his end of the game. I never use qubit to mean a quantum state (and I don't think I ever have), but I know it's a serious problem in the community.

11-02-02 *Oh Modern Wittgenstein, 2* (to N. D. Mermin)

Merminition 85 : *It's the Cbits I'm more concerned with. "bit" – which Charles wants to reserve for the classical physical system clearly has an important abstract meaning as well, relevant to both Qbits and Cbits.*

Aristotle called it matter, the receptacle for accidental properties (presumably some of which could be binary valued). When we turn our attention to a subset of such properties, and are completely ignorant of which of the two is inherent, then we say that we are missing a bit of information.

12-02-02 *The Will to Believe* (to B. C. van Fraassen)

Thanks for the note. You warmed my heart with the sentence, "We were all very intrigued with this 'Bayesian' approach to probability in QM." We (i.e., Caves, Schack, and myself, and sometimes Mermin) know that it's all very much at the beginning stages, but things are starting to fall into place so exponentially fast it evermore gains the air of an inevitability. We've now got a load of material that we haven't published yet (and are working very hard to write up), and I in particular

have made a strong phase transition in my attitude toward quantum time evolutions (i.e., their level of subjectivity versus objectivity). So things are just flying.

Thanks very much for putting me on the meeting list. But also I hope to meet you before then. In any case, know that I am always, always available on email. (And as one of the documents I advertised in the last note will attest, that is even my preferred means of communication!) So, please feel free to get a dialog going if you wish: I would relish it.

13-02-02 *Another Kent Paper* (to G. Brassard)

I think "nonlocality" and especially questions about the restrictions enforced on physical theory—in particular, the description of quantum phenomena—due to no-signaling criteria are red herrings. Our brain pulp is better left for deeper matters. You see, in playing the sorts of games that people have been playing along these lines, the very starting point is to take the idea seriously that the quantum state is a physical property, rather than a description of information, knowledge, belief, betting-odds or what have you. And that is an idea I stopped taking seriously a good while ago.

See Section 6 (and its closing paragraph) in my paper `quant-ph/0106166`. By the way, there is a (relatively minor) technical mistake in that section; let's see if you can find it!

13-02-02 *One More for James* (to C. M. Caves)

Cavesism 68: *Curt? Emphatically so. Snide? I don't think so. But I can see that my habitual curtness has led to more than its usual negative reaction, so let me go back to square one.*

William James likes to say that all beliefs are "numerically additional" to the reality they take as their target, even "true" beliefs.

Thanks for the note.

I similarly need to automate my Outlook better: maybe that'll be one of the great triumphs of our time together in Brisbane.

16-02-02 *Some Things Should Not Pass* (to several friends)

Some things should not pass without our best effort to make them indelible. Yesterday, February 15, Kiki and I had to put our golden retriever Wizzy to sleep. He was the most loving and faithful dog either of us had ever had.

As things happen, Wizzy's last day of life marked exactly eight years from the time he first entered my apartment in Albuquerque; it was my second date with Kiki. I had schemed all day about how I might meet her that evening, and the solution was to cook a meal, a large meal. Hopping around the corner from my apartment to hers, I said, "Would you be interested in dinner tonight? I accidentally made too much." She said, "Sure; I was only going to warm up some potatoes and cheese anyway. I'll be over in about 20 minutes." A couple of minutes later, I got a phone call asking if she could bring her dog with her. I said, "No problem." It was a sweet and touching sight: Wizzy was a dog so insecure at the time, he never left Kiki's side—he didn't sniff around or explore like most any other dog would have; he stayed in bodily contact with her from the moment he entered until the moment he left.

About four months before that night, Kiki had rescued Wizzy from an animal shelter. He must have had a hard life, we surmise, judging from the scar on his head and the fear he had of brooms

at the beginning. We'll never really know how old he was, but comparing him to our other golden, Albert, he was probably 12 or 13 years old when he passed.

The day Kiki met him, she had the intention of looking at two dogs before making a decision of which one to take home. Wizzy was the first. When the attendant let him out of the cage, he so leaned his whole body into Kiki and seduced her with his big, loving eyes, that she knew she couldn't put him back.

Wizzy in fact played a predominant role in my meeting Kiki. For some time I had seen her walking him around the neighborhood, and it dawned on me that since no one was ever with her, she had a chance of being single. I waited for my moment, and it came one morning as I walked across a neighborhood park. I introduced myself by going directly for Wizzy. I said, "What a beautiful dog; what's his name?" She told me, and then I asked in a sort of quizzical way, "Is he a purebred?" She said, "Yes." I said, "I don't think so. I have a golden myself, and they don't look very much alike." Why she accepted a date with me a few weeks later remains a mystery.

This morning I broke the news to Emma, and she became sad. She asked, "Where did he go?" I said, "Back to nature." When she's ready to think about it harder, I'll tell her my (presently) favorite metaphor for what happened: Our finite lives are like little drops of water that have parted from the sea. For a small time we have the chance to move around and determine our courses as we please—to leave a trail behind us. But we all eventually run back into the sea. We never stop being; we just become part of something bigger.

Kiki and I put Wizzy into the ground at sunset yesterday, like Egyptians. We gave him his blanket of eight years so that he would never be cold. We gave him his leash so that he could have an infinity of walks, his bone so that he could have an infinity of chews, and his rubber ring so that he could retrieve it for eternity. I told him that I had always known he was a purebred.

17-02-02 *The Process* (to C. H. Bennett)

Bennettism 7: *My favorite metaphor for death is being dropped into a black hole. The main worthwhile thing left behind is not your physical remains, nor even their information content (which presumably reemerges as Hawking radiation) but rather the relative state you leave behind in the Church of the Larger Hilbert Space.*

I don't suppose it's ever struck you what an excessive sort of universe that would be. In a way, everything appears twice over. Once in the state and once in the relative state. (If you accept the existence of the universal wave function, the one determines the other uniquely.) What so moved God that he should make two copies of everything? (Redundancy for the purpose of error correction won't do as a reply!)

18-02-02 *Psychology 101* (to J. Preskill)

Let me reply to some of your points in a way that doesn't reflect their original order.

Preskillism 1: *In the past I have sensed that you and I differ in how we regard ourselves. I believe that I am just another physical system governed by the same fundamental laws as any other system. You seem to think there is a fundamental distinction between yourself and the system you are observing. To me the Everett view is appealing because it turns away from this egocentrism.*

It's funny, but when I read this, my reaction went in two rather peculiar directions. First I thought, "I wonder if, in the end, the only thing the great quantum foundations struggles will leave

behind is a few psychological observations? If so, what a shame." But secondly, I imagined Galileo hoisting me up to the top of the Leaning Tower of Pisa and dropping me off it along with his two famous stones. Even though I cursed and screamed the whole way down, I went "splat" at the same time that they went "thud."

Here's the psychological thought in a little more detail. One of the things that bugs me about the Everett view is what *I* consider *its* extreme egocentrism! Now, how can that be—both of us accusing the other's view as *the* egocentric view? I'll tell you what I think, trying to express the problem from both sides of the fence.

My side gets to go first. What I find egocentric about the Everett point of view is the way it purports to be a means for us little finite beings to get outside the universe and imagine what it is doing as a whole. And what is it doing as a whole? Something fantastic? Something almost undreamable?! Something inexpressible in the words of man?!?! Nope. It's conforming to a scheme some guy dreamed up in the 1950s.

This whole fantastic universe can be boiled down to something representable within one of its most insignificant components—the brain of man. Even toying with that idea, strikes me as an egocentrism beyond belief. The universe makes use of no principle that cannot already be stuffed into the head of an average PhD in physics? The chain of logic that leads to the truth of the four-color theorem (apparently) can't be stuffed into our heads, but the ultimate operating principle for all that "is" and "can be" can?

It's a funny thing: I don't think I've met anyone who would imagine that mathematics will ever come to an end. Or even that it *can* come to an end. There'll always be new axiom sets to play with, new formal structures to write down. But with physics it's a completely different story. People are always wanting to say, "Well we've finally gotten there." Or, "Even though we're not there, we're pretty damned close." It's OK, even condoned, to have Dreams of a Final Theory. From this point of view, all the mathematics yet to come is worthless as far as the essence of the universe goes; the wad was already shot.

You get the point. It's a psychological one, but it's one that I find overwhelmingly powerful. It is that anytime any of us ever has the chutzpah to say, "Here's an ultimate statement about reality," or even a potentially ultimate one, what we're really doing is painting the world in the image of man. We're saying that the measly concepts we've managed to develop up to this point in time fit the world in a way that none of our previous concepts have, that none in the future will ever do better, and, most importantly, we view this not as a statement about ourselves and the situation set by our present evolutionary and intellectual stage, but rather as a property of the universe itself.

Now let me start moving toward the other side of the fence. The question someone like me—someone who has these kinds of blasphemous thoughts—has to ask himself is, how can I ever hope to be a scientist in spite of all this? What can science and all the great achievements it has given rise to in the last 400 years be about if one chooses to suspend one's dreams of a final theory at the very outset? (Or, to tribute Johnny, how can one have law without law?)

I think the solution is in nothing other than holding firmly—absolutely firmly—to the belief that we, the scientific agents, are physical systems in essence and composition no different than much of the rest of the world. But if we do hold firmly to that—in a way that I do not see the Everettistas holding to it—we have to recognize that what we're doing in the game of science is swimming in the thick middle of things. We're swimming in this undulant sea, and doing our best to keep our heads above the water: All the concepts that arise in a physical theory must be interpreted to do with points of view we can construct from *within* the world.

That is to say, we have to loosen the idea that a physical law is a mirror image of what "is" in the world, and replace it with something that expresses instead how each of us can best cope with

and hope to take advantage of the world exterior to ourselves. This, it seems to me, is something that by its very definition can be stuffed into the human brain. The current state of science is our presently best known means for survival. A scientific theory indeed, from this point of view, is yet another expression of Darwinian principles. Scientific theories evolve and survive because the survivors have a kind of staying power that none of the rest of the competition have. Not because they are part of the blueprint of the universe.

The situation of quantum mechanics—I become ever more convinced—illustrates this immersion of the scientific agent in the world more clearly than any physical theory contemplated to date. That is because it tells you you have to strain really hard and strip away most of the theory's operational content, most of its workaday usefulness, to make sense of it as a reflection of "what is" (independent of the agent) and—importantly—you insist on doing that for all the terms in the theory.

I know you're going to find the last sentence debatable, but that is what I see as the danger in the Everett point of view: You are able—or at least purportedly so—to view the universal state as a reflection of something, but at the cost of deleting all the concrete things it was meant to reflect in the first place. What I mean by this is, if we take any concrete situation in quantum mechanics—a system, a measuring device, and some kind of model for the beginning stages of a measurement—we can indeed construct a Church-of-the-Larger-Hilbert space description of it. I'll grant you that. But try to go the other way around without any foreknowledge of the "measurement": Start with the Church, and try to derive from it that a concrete measurement has taken place, and you encounter an embarrassment of riches. You don't know how to identify the valid worlds, etc., etc. (And, if you ask me, invoking decoherence as a cure-all is little more than a statement of faith that some guy from Los Alamos has the all the answers to all the tough questions the rest of us are too lazy to work out.)

So, I myself am left with a view of quantum mechanics for which the main terms in the theory—the quantum states—express nothing more than the gambling commitments I'm willing to make at any moment. When I encounter various other pieces of the world, if I am rational—that is to say, Darwinian-optimal—I should use the stimulations those pieces give me to reevaluate my commitments. This is what quantum state change is about. The REALITY of the world I am dealing with is captured by two things in the present picture: 1) I posit systems with which I find myself having encounters, and 2) I am not able to see in a deterministic fashion the stimulations (call them measurement outcomes, if you like) those systems will give me—something comes into me from the outside that takes me by surprise.

OK, now let me put myself squarely in your pasture. You worry that having those main terms in the theory refer to *my* (or *your*, or Joe Buck's) gambling commitments, is committing a kind of egocentrism. What respectable theory would refer to my particular vices, my desires, my bank account in making its most important statements?

This is going to surprise you now, but I agree with you wholeheartedly. Even enthusiastically so. Where I seem to disagree is that I do not find this a good reason to promote those vices, those commitments to an unearthly realm and call them "states of the universe" (or relative states therein). Instead, it seems to me to be a call to recognize them for what they are and to redouble our efforts for getting at the real nub of the matter.

Let me try to give you a way of thinking about this that you might respect. What was Einstein's greatest achievement in getting at general relativity? For the purposes of the present exposition, I would say it was in his recognizing that the "gravitational field" one feels in an accelerating elevator is just a coordinate effect—it is something that is induced purely with respect to the description of an observer. In this light, the program of trying to develop general relativity thus boiled down to trying to recognize all the things within gravitational and motional phenomena that should be

viewed as consequences of our coordinate choices. Or to use a phrase I've come to like, it was in identifying all the things that can be viewed as "numerically additional" to the observer-free situation which come about purely by bringing the observer (scientific agent, coordinate system, etc.) onto the scene.

Now the point is, that was a really useful process. For in weeding out all the things that can be viewed as "merely" coordinate effects, the fruit left behind could be seen in a clear view for the first time: It was the Riemannian manifold that we call spacetime.

What I dream for in my foundational program for quantum mechanics is something just about like that. Weed out all the terms that have to do with gambling commitments (I used to call it information, knowledge, or belief), and what is left behind will play a role much like Einstein's manifold.

This much of the program, I hope and suspect you will understand even if you are not sympathetic to it. But, I don't know, you might be sympathetic to it. (Especially if I've done a good job above.) However, it is also true that you have rightly suspected some tendencies in me that go further. In particular, in opposition to the picture of general relativity, where reintroducing the coordinate system—i.e., reintroducing the observer—changes nothing about the manifold (it only tells us what kind of sensations the observer will pick up), I do not suspect the same of the quantum world. This is why I recommend to all my friends that they read William James's little article "The Sentiment of Rationality." It sort of sets the right mindset, even though it has nothing to do with quantum mechanics (other than in the efficacy of taking gambles) and goes much further on religion than I myself would go.

Anyway, here I suspect that reintroducing the observer will be more like introducing matter into pure spacetime, rather than simply gridding it off with a coordinate system. "Matter tells spacetime how to curve *when it is there*, and spacetime tells matter how to move *when it is there*." Observers, scientific agents, a necessary part of reality? No. But do they tend to change things once they are on the scene. Yes. Or at least that's the idea.

Does that mean that the scientific agent is something outside of physical law? Well, to give this an answer, you've got to go back and be very careful to use the picture of "physical law" that I built up at the beginning of the essay. What we are "governed" by, God only knows. He's the one, if anyone, who sits outside the physical universe and has a chance to look back at it whenever he pleases. Our task is to build up as good and solid a set of beliefs as we can from within it. In that way, we increase our survival power, and use our spare time to try to bring forth a few progeny of our own. (I used the word "governed," by the way, because you had used it above.)

If Galileo had dropped me from the tower, I feel pretty confident that I would have gone splat.

Aye yi yi, I wrote a lot. That's the dangers of being jetlagged in a foreign country without one's wife and kids. (I'm in Sendai visiting Ozawa.)

I'm going to have to reply to the other points of your note later.

19-02-02 *Re-Tackle* (to L. Hardy)

I'm in Japan for a couple of weeks at the moment, and I'm finally getting some time put in the 16-hour days again. (Like I used to in the good old days.)

Anyway, I thought I'd tell you, though it is long overdue, I am finally tackling your 5-Axiom paper again. I'm starting to appreciate it much more for sure. If you just weren't so damned non-Bayesian!!! There's a lot of good stuff in it. My main difficulty at the moment is that you have a couple of moves that I know I don't want to allow into my porn: 1) taking mixtures of states (i.e., allowing probabilities of probabilities) as a fundamental step, and 2) invoking extensions to

the Church of the LHS.

But I definitely think you are on the right track. And it's probably just a matter of my searching harder for some Bayesian ways of looking at what you've already done. (I'm doing that as we speak.) The most essential things that strike me are 1) the move to column vectors and thinking of measurement as a decomposition of the state, and 2) invoking a relation between K and N. I think those ingredients are definitely here to stay in my mindset. Also I'm warming up to the continuity axiom. I'll try to write you the reasons why soon. (But I've made promises before.)

Anyway, I'm super- looking forward to your stay.

19-02-02 *Where to Stop?* (to J. Preskill)

You know I've got a million ways of saying why I don't like the Everett interpretation—none of which you find very convincing—but here's a new thought that dawned on me as I was writing my last note to you, and I wonder what you think. Let me try it out on you.

Everett says, "You know Chris, all these silly things you do like leaving measurement as an undefined primitive, etc., will disappear and find a more satisfactory solution if you'll just lay back, relax, and recognize that the quantum states you're working with are really just relative states ... ones derived from the universal wavefunction under one or another decomposition."

I say, "Aha, OK. Then what is this wavefunction of the universe?"

He says, "Well for that, we ought to consult the Hartle-Hawking paper. Here it is: It's $|\psi\rangle$."

Then it dawns on me. How do I know that that state they wrote down isn't just the relative state of our universe with respect to some super-universe? And how do I know that that state is not itself some relative state with respect to some super-super-universe? And so on ad infinitum.

The point is, what principle of science tells you where to stop? None, I'd guess. Is that troubling? I don't know. But it seems a little fishy to me.

Everett tells me, "You've just got to recognize that the wavefunctions you use on a daily basis simply don't have the same ontological status as my universal wavefunction. You might call them 'states of knowledge' in a way. But my universal wavefunction, now that's the real thing; it's here independently of every man, woman, and child." But then I ask, "Well, why does yours get that exalted position? I claim that it itself is a relative state and you can't prove me wrong."

Like I say, I don't know what I think of this yet, but it does strike me as fishy. Once you get into the game of building a Church of the Larger Hilbert Space, who tells you how many pews to put there? That's not something it seems to me you can ever discern from within the universe. It's an article of religion, it seems to me, much like the imagery the appellation seems to provoke.

19-02-02 *One More Before Lunch* (to J. Preskill)

Here goes. Let's see if I can be brief enough to finish in time. No easy task for me!

Preskillism 2: *Still, I'm flattered by your persistence. Or are you (as I can't help but suspect) slyly recruiting an Everettite who will make a weak case?*

No, I was honest in all that I wrote in the flattering note.

... Damn! Didn't make it. It's after lunch now.

What I was going to say was that I was absolutely honest in why I want you to write the sort of paper that you might for the special issue. The point being that if a physicist really, really does find Everett completely adequate to his needs ... and can argue that it's not a superfluous addition

to what he's actually doing when he's doing a calculation, then that would be an interesting datum for the freaky types like me who see it as an ugly picture of the world.

I wasn't slyly recruiting you to make a weak case. But, of course, I actually did have an ulterior motive—something much bigger in my mind than the needs of the special issue—and now that you've forced my hand, I ought to be up front about it. I was banking that if you really did put your heart into making a convincing case for the Everettistas—i.e., the sort of thing that having to write a paper on the subject might draw out of you—then your intellectual honesty would cause you to see how much of the point of view really hasn't been worked out (yet? or maybe ever will be?). I.e., that they have no convincing/relevant argument for the probability rule, that they seem to require a preferred basis, that they seem to require a preferred tensor product structure, that to make sense of two systems, they need to invoke a third, and so on. And when you started to add all those things up, you would also realize that the Everett picture really wasn't much help after all in getting you to the point understanding what measurement is. That's how I was being sly.

19-02-02 *Psychology* (to J. Preskill)

Preskillism 3: *Sure, scientists are arrogant. That our puny brains can grasp anything about how the world works is a miracle, and I can't pretend to be able to explain it. But I believe it is so.*

It's not the claims of "anything" that worry me so much. It's the claims of "everything." I.e., that our puny brains can grasp everything (in the sense of an "ultimate physics") is the thing that seems implausible to me. And if we can't have that, then we—or more realistically, those who are inclined to do so—ought to be asking what it is we're shooting for.

Sorry I hit a nerve.

19-02-02 *Sendai Morning* (to N. D. Mermin)

Thanks for helping pick me back up from my preskillsplat. (That could almost be a real German word.)

I'm enjoying Sendai and Ozawa's company greatly. And I'm once again thinking harder about complete positivity. The fact that the trace-preserving completely positive maps are isomorphic to the density operators on a larger space has got to be a truly deep point (in my quest to shore up my argument that the time evolution map is itself a subjective belief). But I just can't quite figure out how to put that into a convincing physical context.

Also, by the way, I'm going through Hardy's five axioms again. It's making a much bigger impression on me this time around. It's got a lot of good stuff in it. If it just weren't so damned non-Bayesian! The point is, I think it's got a hell of a lot of cleaning up to be done on it, but it really does have potential.

20-02-02 *Out Loud* (to W. K. Wootters)

Thanks for thinking out loud. I'll just respond to one point.

Wootersism 1: *Let me think out loud for minute here about your note to John. I can think of a* pragmatic *reason for being an Everettista. (At those times when I am particularly attracted to the Everett view, this is what attracts me.) Even if we can't hope to* know *reality, if we can* guess *a model of reality, this guessing helps science progress. What Everett gives us is a guess at ultimate reality. So let's guess that Everett is right, and then work to falsify this guess.*

It's about the guessing part. I had meant to cover that case with the word "potentially" in this paragraph:

> You get the point. It's a psychological one, but it's one that I find overwhelmingly powerful. It is that anytime any of us ever has the chutzpah to say, "Here's an ultimate statement about reality," or even a potentially ultimate one, what we're really doing is painting the world in the image of man. We're saying that the measly concepts we've managed to develop up to this point in time fit the world in a way that none of our previous concepts have, that none in the future will ever do better, and, most importantly, we view this not as a statement about ourselves and the situation set by our present evolutionary and intellectual stage, but rather as a property of the universe itself.

That's actually the point you first inspired in me with your aphorism about the dog. There are some things a dog will never understand; there are even questions he can never understand. Why should we expect the evolutionary chain to stop with us? In a way, this cluster of thoughts that I'm starting to think is a rather strong kind of anti-Church-Turing thesis. That is, I think we've gotten into the habit—and Deutsch tried to codify it in his 1985 paper—of thinking that the Church-Turing thesis implies that once you've got a universal machine (people like to say the human brain is one), then you've reached the end of the line. But one should not forget that what Turing was up to in his 1936 paper was to formalize the notion of what is "humanly computable." (This was a point brought home to me by Chris Timpson's excellent undergraduate thesis `http://users.ox.ac.uk/~quee0776/`.)

By the way, not that it matters too much, but I refined one of my paragraphs in the note before I archived it away:

> The situation of quantum mechanics—I become ever more convinced—illustrates this immersion of the scientific agent in the world more clearly than any physical theory contemplated to date. That is because it tells you you have to strain really hard and strip away most of the theory's operational content, most of its workaday usefulness, to make sense of it as a reflection of "what is" (independent of the agent) and—importantly—you insist on doing that for all the terms in the theory.

And that—by the way again—may have been a point also inspired by someone else, namely Schroedinger. Though I haven't been able to completely track its origin in my mind. Somewhere—maybe his 1935 paper in Wheeler and Zurek—he says something like, "understanding quantum mechanics may not require the addition of more variables, but rather taking some of them away."

22-02-02 *Getting the Mindset* (to P. F. Scudo)

You asked for some materials to help you get more familiar with the problem I'd like you to work on.

OK, I'm ready to send you some now. And I'll inundate you, but don't let that frighten you. Only try to understand things to the extent that you've got some free time. (And I well expect you may have none at all!)

The first thing to do is read the Brun, Finkelstein, Mermin paper, "How Much State Assignments Can Differ," `quant-ph/0109041`. This is really the thing that started my thoughts off in the present direction, for I completely disagree with them. Their statement is in ultimate conflict with the Bayesian idea of what a quantum state can be. So, understand their argument.

After that, start reading my new samizdat (i.e., underground publication), which I will send you in a separate email. It's quite large (150+ pages), with plenty of repetition, but it mounts an attack on B, F, and M from just about every direction conceivable. Also there is the fact that as time went on, all the issues became clearer and clearer with me, and so I found crisper and crisper ways of expressing myself. Still reading it (and reading it carefully) might help you get in the right mindset for any number of problems we might be discussing.

The upshot of much of the samizdat is that, for consistency in one's Bayesianism, one must accept that the *assignment* of a POVM (living on a piece of paper) to a measuring device (living in a laboratory) is a subjective judgment at exactly the same level of subjectivity as the quantum-state assignment. Thus one is presented with a cross-roads. Either one accepts pure Bayesianism and gives up the idea that POVMs and quantum time evolutions have objective ascriptions (i.e., gives up the idea that they are independent of the agent assigning them), or one continues with the belief of objective POVMs and time evolutions and adjusts oneself to the idea that probability has to be objective too.

The direction I personally take is that probabilities are subjective, always. They are never objective. Therefore one must make sense of what one means when one speaks of an "unknown POVM" or an "unknown quantum operation." This is where a new kind of de Finetti representation must come in ... and hence your summer work.

But the most important thing for the present is understanding all the motivation leading up to that point. When you are ready to see a sketch of how the theorem ought to go, look at the note of 19 November 2001 to Caves and Schack titled, "A Lot of the Same."

For completeness sake, I will also send you a draft of the paper that Caves, Schack and I are presently constructing to make some of this official. The part that is maybe the most relevant for your education is the appendix on Dutch-book arguments. You might try to understand that. And I'll write you more about that later.

25-02-02 *A Wonderful Life* (to W. K. Wootters)

Thanks for the two notes, and wow, thanks for reading the James essay. Your questions were anything but naive. In fact, they were much needed. In trying to answer them, I think I significantly clarified—to myself even!—what I'm hoping to get at. Besides, I certainly don't have a final stand yet; the whole point of view is in the process of formation and questions like yours really help.

I'll do my best to reply to your questions below, and in the process I think I'll finally compose what I've been wanting to say about your "private-world-within-entanglement" musings. At the end of the note, I'll list some of the open questions on my mind. (These are likely to be the naive ones!)

Wootersism 2: *Of course I'm very sympathetic to the perspective you express in this paragraph ... but couldn't one still argue that as a matter of methodology, the tactic of pretending that we can know the whole story has served science well? We make up a model of the world, and this model gives us something to shoot at. We hang on to the model until we have found an explicit flaw in it (other than the flaw of hubris). And then we move on to a new model.*

I find this an interesting question. On the one hand, I think this strategy does work well in advancing science. On the other hand, scientists (and others) are much too prone to accept as true the pragmatic lie that says we can fully understand the world.

Your note to John P. goes some way toward laying out an alternative methodology. You speak of science in Darwinian terms: the most successful theories survive. How then do we proceed as scientists? I suppose the answer is that we still make up theories and test them, but the theories

are not tentative descriptions of the world. Rather, theories are schemes for making predictions. But you obviously also want to say that our theories tell us something *about reality, even if they are not descriptions of reality. Moreover, our theories will tell us more about reality if we identify and remove from them those aspects that are subjective. So your view of science is not entirely operational. There is realism in the background.*

Have I understood you correctly?

Yes there is certainly a kind of realism working in the back of my mind, if what you mean by "realism" is that one can imagine a world which never gives rise to man or sentience of any kind. This, from my view, would be a world without science, for there would be no scientific agents theorizing within it. This is what I mean by realism: That man is not a priori the be-all and end-all of the world. (The qualification "a priori" is important and I'll come back to it later.)

A quick consequence of this view is that I believe I eschew all forms of idealism. Instead, I would say all our evidence for the reality of the world comes from without us, i.e., not from within us. We do not hold evidence for an independent world by holding some kind of transcendental knowledge. Nor do we hold it from the practical and technological successes of our past and present conceptions of the world's essence. It is just the opposite. We believe in a world external to ourselves precisely because we find ourselves getting unpredictable kicks (from the world) all the time. If we could predict everything to the final T as Laplace had wanted us to, it seems to me, we might as well be living a dream.

To maybe put it in an overly poetic and not completely accurate way, the reality of the world is not in what we capture with our theories, but rather in all the stuff we don't. To make this concrete, take quantum mechanics and consider setting up all the equipment necessary to prepare a system in a state Π and to measure some noncommuting observable H. (In a sense, all that equipment is just an extension of ourselves and not so very different in character from a prosthetic hand.) Which eigenstate of H we will end up getting as our outcome, we cannot say. We can draw up some subjective probabilities for the occurrence of the various possibilities, but that's as far as we can go. (Or at least that's what quantum mechanics tells us.) Thus, I would say, in such a quantum measurement we touch the reality of the world in the most essential of ways.

With that said, I now want to be very careful to distance this conception of reality, from what I'm seeking in the foundation game of quantum mechanics. Here's the way I originally put it to John the other day. Let me repeat a good bit of it so that it's at the top of your mind:

> OK, now let me put myself squarely in your pasture. You worry that having those main terms in the theory refer to *my* (or *your*, or Joe Buck's) gambling commitments, is committing a kind of egocentrism. What respectable theory would refer to my particular vices, my desires, my bank account in making its most important statements?
>
> This is going to surprise you now, but I agree with you wholeheartedly. Even enthusiastically so. Where I seem to disagree is that I do not find this a good reason to promote those vices, those commitments to an unearthly realm and call them "states of the universe" (or relative states therein). Instead, it seems to me to be a call to recognize them for what they are and to redouble our efforts for getting at the real nub of the matter. ...
>
> What I dream for in my foundational program for quantum mechanics is something just about like that. Weed out all the terms that have to do with gambling commitments (I used to call it information, knowledge, or belief), and what is left behind will play a role much like Einstein's manifold.
>
> This much of the program, I hope and suspect you will understand even if you are not sympathetic to it. ... However, it is also true that you have rightly suspected

> some tendencies in me that go further. In particular, in opposition to the picture of general relativity, where reintroducing the coordinate system—i.e., reintroducing the observer—changes nothing about the manifold (it only tells us what kind of sensations the observer will pick up), I do not suspect the same of the quantum world. ...
>
> Anyway, here I suspect that reintroducing the observer will be more like introducing matter into pure spacetime, rather than simply gridding it off with a coordinate system. "Matter tells spacetime how to curve *when it is there*, and spacetime tells matter how to move *when it is there*." Observers, scientific agents, a necessary part of reality? No. But do they tend to change things once they are on the scene. Yes. Or at least that's the idea.

From some of my choices of words, I think you probably got the impression that this thing—this structure within quantum mechanics—that I'm hoping to find at the end of the day is meant to be a model of "reality." Or at least our "current best guess" of what reality is. But no, that's not really what I want. And your questions helped make that much clearer to me. Remember, for me, the mark of reality is its indescribability.

What I'm asking for instead is something like what one finds in the old movie, *It's a Wonderful Life*. That is to say, in our scientific theories, we codify some fraction of what we know about manipulating the world and conditionally predicting the phenomena about us. However, suppose we wanted to get at a measure of our place in the world. How would we quantify it, or at least qualify it? That is, how might we ask how important our lives and agential actions are with respect to the theory we ourselves laid out?

Our only tool, of course, is the theory; for it defines the frame for optimal thinking (and imagination) at any given moment. We can only gauge our measure by deleting the free variable that is ourselves and seeing what is left behind. You surely remember what George Bailey found when his guardian angel granted his wish in *It's a Wonderful Life*. He found that his life mattered. So too is what I suspect we will find in quantum mechanics.

But all of that is the sort of thing I won't be able to say in a conference presentation for quite some time. It's the sort of thing that we discussed once before, in the context of some Jamesian quote. It's the underground reason for the philosophy.

At the level of convincing our peers, let me put it to you this way. Within quantum mechanics, there is an invariant piece which is common to all of us by the very fact of our accepting the theory. That is what we are in search of because in some sense—which need not pertain to a realistic conception of a theory's correspondence to nature—it is the core of the theory. It is the single part that we agree upon, even when we agree upon nothing else. In the direction I am seeking to explore, the quantum state is "numerically additional" to that core. (That is, the quantum state is a compendium of Bayesian "beliefs" or "gambling commitments" and is thus susceptible to the type of analysis James gives in his "Sentiment of Rationality." Our particular choice of a quantum state is something extra that we carry into the world.)

I hope that clears up some of the mystery of my thoughts for you—it did for me. Given John's implicit acceptance of the idea that "a true theory is a mirror image of nature," I should not have said in my note that I agreed with him "wholeheartedly." I do not intend for *any* part of the formal structure of quantum mechanics to be a mirror image of nature (in the sense of a proposed final theory). However, I do not intend to give up the reality of our world either.

From my point of view, the only "true" reality that creeps into quantum mechanics is "in the differential"—i.e., in the changes we induce upon our (personal) quantum states for this and that due to any stimuli we give to or take from the outside world. That, however, is a pretty amorphous thing as theoretical entities go. It is little more than what might have been called in older language,

the measurement "click."

There is a temptation to go further—to say that the POVM element E_b associated with a measurement outcome b is itself an element of reality. But I think that has to be resisted at all costs. There are several arguments one can use to show that the *ascription* of a particular POVM to a measurement phenomenon is a subjective judgment at the same level of subjectivity as the quantum state itself. (In fact the two go hand in hand, one cannot support the subjectivity of the quantum state without also taking the subjectivity of the POVM.) Instead, one should view the (theoretical) ascription of a POVM to an actual measurement device as an attempt to set the significance and meaning of the "click" it elicits. Similarly for the Krausian quantum operation associated with the measurement: It describes the subjective judgment we use for updating our quantum-state assignment in the light of the "click." (If you want more details about these arguments, I can forward you some of my old write-ups on the subject.)

So, you probably ask by now, "What does that leave for the core of the theory? Aren't you throwing away absolutely everything?" And the answer is, "No, I don't think so." Let me give you an example of something which I think is left behind. Recall my favorite argument for why the quantum state cannot be an element of reality—it's the Einstein argument I wrote about in Section 3 of my NATO paper. Once I posit a state for a bipartite system, even though by my own admission my actions are purely local, a measurement on one of the systems can toggle the quantum state of the other to a large range of possibilities. Thus, I say that the quantum state of the far-away system cannot be more than my information or the compendium of subjective judgments I'm willing to ascribe to that system.

Notice, however, that in positing the original state, I had to also implicitly posit a tensor-product space for the bipartite system. Let me ask you this: Once this tensor-product space is set, is there any way to toggle one of the factors from afar just as with the quantum state? As far as I can tell there is not. Thus I would say that the Hilbert space of the far-away system is a candidate for part of the theory's core. Well, the Hilbert space—once the choice of a particular quantum state within it is excluded—really carries no substance beyond its dimensionality d. Thus, in a more refined way of speaking, what I really mean to say is that when I posit a quantum system, I am allowed to also posit a characteristic property of it. It is a property that can be captured by a single integer d.

There are some other things which I can argue will be "left behind" in such an analysis, but I don't want to clutter this note too much. Mainly I presented the example above so that I could give you a clearer sense of how I want to draw a distinction between the rawest forms of "reality" (the surprises the world gives us) and the "core of a theory."

It is the core of the theory (along with the theory as a whole) that I am starting to view in Darwinian terms. But don't we have every right to posit that core as a property of the world itself, at least as long as that belief serves us well? This, as you point out, has been the predominant image of what science is about heretofore.

The only answer I can give you is "yes, we can" (just as indeed we have heretofore). So, your point is well-founded. What I am worried about is whether we *should* posit it so. You say that this view has guided science well in the past. But how do you know? In a world with a view that there is no ultimate law, how do you know that we would not be a thousand years more advanced if we had only better appreciated our role as the substratum of our theories? I think it boils down to the difference between an active and passive view of what existence is about. Or maybe the difference between a positive and a negative view.

To make this point, let me try to put things back into the context of regular Darwinian evolution. Consider the word "elephant." Does it denote anything that exists in a kind of timeless sense, in a way that we usually think—or in my case, previously thought—of physical theories as existing?

If the concept of an elephant is worthy of treating as a candidate for an element of reality, then so too will a theory's core.

Well, if we have bought into Darwinism in any serious way, then I would say, no, there is nothing particularly timeless about the concept of an elephant. There was once a chance that it might not even arise in the world. The "elephant" is merely a function of the selective pressures that cropped up in our world's particular history. And, ashes to ashes, dust to dust, the poor elephant may eventually disappear from the face of the universe, just like so many species that arose in the course of evolution only to be never discovered by a single archeologist.

But now, contrast the evolution of the elephant with the possible future evolution of the human species. The elephant was an accident pure and simple, from the strictly Darwinian view. But I would be hard pressed to apply pure Darwinism to the future of mankind. The birth of my oldest daughter, for instance was no accident. Her traits were selected based on personal visions that both her mother and I had for the future. Similarly, but not so excitingly, with the golden retriever, and all our other domesticated species. The key point is that in the present stage of evolutionary development, we have it within our power to move beyond strict Darwinism. This is what our industry of genetic engineering is all about.

However, we would have never gotten to this stage if we had not first realized that the concept of a species is not immutable. As strange—and as crazy and as scary—as it may sound, this is where my thoughts are starting to roam with physical theories. This does not mean, however, that we can have exactly what we want with our physical theories—that they themselves are little more than dreams. Just as the genetic engineer can make a million viruses that will never have a chance of surviving on their own, there is more to the story than our whims and fancies: There is the ever-present selective pressure from the outside. But that does not delete the genetic engineer's ability to make something that was never here before.

But now, I go far, far, far beyond what I needed to say to answer all your questions. Mainly, I just wanted to emphasize why I intentionally placed the words "a priori" in my definition of reality way above.

I fear now slightly that you're going to realize I'm one of the craziest people you've ever met! And, trust me, I'm not sure I really believe all that I said in the last three paragraphs. But it does strike me as a productive, or at least hopeful, train of thought that someone ought to explore. I guess I offer myself as the sacrifice.

— — — — —

There. I think that's enough of my going around your questions in a rather wide way. Let me now zoom back to the center of one of them for purposes of a final emphasis.

Wootersism 3: *But you obviously also want to say that our theories tell us* something *about reality, even if they are not descriptions of reality.*

I hope you can glean from all the above that I do indeed believe our theories tell us something about reality. But that something is much like what the elephant tells us about reality. It's presence tells us something about the accumulated selective pressures that have arisen up to the present date. A theory to some extent is a statement of history. It is also a statement of our limitations with respect to all the pressures yet seen, or—more carefully—a statement of our limitations with respect to our imaginations for classifying all that we've yet seen. (I for instance, cannot jump off the leaning tower of Pisa unprotected and hope to live; you, for instance, cannot get into your car and hope to push on the accelerator until you are traveling beyond the speed of light.) Finally, to the extent that we the theory users are part of nature, the theory also tells us something about nature in that way.

But for any theory, there is always something outside of it. Or at least that's the idea I'm trying to build.

PS. Way above, I said I would finally say a few words about your "private-world-within-entanglement" musings. But somehow it didn't quite fit in with the flow of the rest of what I wanted to say. So, let me try to present the statement in isolation. From my point of view, the quantum state, and with it entanglement, never pierces into the quantum system for which we posit a parameter d (the "dimension"). Similarly for any bipartite system for which we posit two parameters d_1 and d_2. The quantum state is only about what I'm willing to bet will be the consequences when I reach out and touch a system. Otherwise, indeed, a quantum system denotes a private world unto itself. And similarly with bipartite systems. We have very little right to say much of anything about the goings-on of their insides. (This part of the picture is something I've held firmly for a long time; it even shows up in my Physics Today article with Asher.) Thus, I guess what I'm saying is that I can find a way of agreeing with what you wrote me:

Wootersism 4: *Or maybe there* is *a possibility of testing the above distinction, at least in principle. (It depends on how I finish my interpretation.) Let's consider David Deutsch's thought experiment in which a sentient computer measures, in a reversible way, some property that doesn't have a definite value for the object being measured. Later, the computer will report that he observed a definite outcome but did not record which one, and the various possible paths will have been brought together successfully to exhibit interference. That's David's prediction. I would agree that interference should be possible, but I may disagree about what the computer will report. If it is true that one needs to have a* shared *world in order to have a sensible world, then when the computer was all alone in his private entanglement with the object he measured, who knows what he experienced? Maybe he experienced nothing more about that measured object than what one electron in a singlet pair experiences about the other electron. Maybe he cannot honestly report later that he saw one outcome or another.*

But maybe I'm coming at it from a different point of view.

PPS. I also promised to end with some open questions. But I'm petered out now. And if you've gotten this far, you're probably exhausted too. So I'll just leave it for the future, depending upon how interesting you find the ideas above, or how much you think they're nonsense!

Only two and half days left in Japan.

27-02-02 *An Even Tireder Old Man* (to N. D. Mermin)

I forgot to include you as a recipient of the note below as I had promised. Let me not tell a lie: It's attached this time around.

I made good progress on several fronts in Japan. Maybe the most important—though most trivial—was that I finally found a rather natural Bayesian motivation for the linearity of the probability rule in Lucien's system. (Lucien is a trickster; he sneaks axioms in left and right that he doesn't call axioms.) Anyway, at the very beginning of the paper already, I couldn't accept his motivation for linearity, falling back as it does on the idea that a state refers in an objective way to (a class of) preparations. That is, he didn't take states to be Bayesian beliefs pure and true, and that's crucial to me.

Here's the solution; it's as simple as can be (now that I see it). The upshot of Lucien's formalism is that a measurement is any "I-know-not-what" that causes a refinement of one's initial beliefs (consistent with the other axioms). Thus a measurement is simply an application of Bayes rule

by its very definition—moreover, any application whatsoever within the allowed range. OK, that's good enough for the single observer and already thrills me (because it expresses in a more rigorous way a claim I made in the NATO paper at the level of density operators).

But when there are two observers and you view measurement as nothing but an application of Bayes rule, you have to have some method of saying when they agree that it is the same measurement (so that you don't fall into the infinite regress I tried to warn you of when you were going full-steam-ahead with BFM). Well, here again the solution is simple: Two observers should call a refinement of their beliefs the same measurement, precisely when they draw the same meaning for the data they obtain. Thus as long as both observers ascribe the same $P(\text{data}|\text{hypothesis})$, we will say they "agree" upon the measurement.

And that's it. That does the trick. That and that alone gives linearity. No Gleason's theorem; not even anything fancy. I'll make this prediction right now: Quantum mechanics will turn out to be one of the simplest structures we've ever seen in physics, and for 75 years we've just been too pigheaded to see it.

Somewhere over the pacific,

02-03-02 *Mermin Festschrift, 2* (to C. M. Caves)

Cavesism 69: *I'm inclined to think that the Mermin festschrift would be just the right place for my speculations about the reality of Hamiltonians, especially since he has kind of encouraged this idea. What do you think?*

I think, "I wonder why you ask *me* this!?!?" Are you contemplating that we might have dual submissions? Abbott and Costello, Crosby and Hope, Lewis and Martin, the Smothers Brothers? The problem is, both of us would want to be the straight man.

OK, enough sarcasm. To answer your question, yes, that probably would be a good idea.

By the way, here's a little technical meat for the hungry wolf. Suppose I were willing to go with you on the objectivity of THE interaction Hamiltonian or unitary in a measurement interaction. (I'm not willing in actual fact, but that's beside the point for this question.) If we write down the POVM such an interaction ultimately leads to, then we will get something like this for its elements E_b:

$$E_b = \mathrm{tr}_{\mathrm{A}}\Big((I \otimes \sigma)U(I \otimes \Pi_b)U^\dagger\Big) ,$$

where all the terms have the usual interpretation. I believe, according to you, there should be only one subjective piece to the right hand side of this equation, namely the ancilla's initial density operator σ.

Here's my (innocent, first) question. The subjectivity in σ will afford a range in our interpretation of which POVM was actually measured in any such interaction. What is that range? How large is it?

A corollary question—this one is more rhetorical and less innocent—is this. You know I have never liked the above formulation of what a measurement is because one has to invoke a second measurement (the Π_b) to explain the first (the E_b). This leads to an infinite regress because one can ask (as von Neumann did), where do the Π_b come from. Thus, just to make sure we're on board, let me reconfirm that I got the setting in your mind right in my elaboration above. Namely, that the only truly subjective term you would accept in the displayed equation is σ? The range in E_b is consequent only to that? If that's the case, I would also like to understand how you invoke a stopper at the level of the Π_b. That is, why do the Π_b not have some of their own range, induced by a higher level measurement model?

You can tell I'm still deeply jetlagged.

03-03-02 *De Finetti and Strong Coherence* (to P. F. Scudo)

Thanks for the ghost story.

Scudoism 1 : *Will tell you more as soon as I finish my calculations for this paper.*

Not to worry at all. I have a hard enough time leading a rushed life. You should not expect that I expect that out of you when I cannot live up to standard myself! When you arrive here you'll have plenty of time to practice the macabre.

But still, if you've got the interest, who am I to get in your way. Here's some historical investigative work that you might tackle if you find that have nothing better to do. In fact, your friend Regazzini might be a wonderful resource in this regard. In the draft of the anti-BFM paper that Caves, Schack, and I are putting together, we make a distinction between simple "Dutch-book consistency or coherence" (i.e., the notion that de Finetti and Ramsey first introduced) and "strong Dutch-book consistency." The latter is a notion that apparently Abner Shimony first introduced, though Caves and Schack stumbled across it independently in one of our many wars. (See reference 12 on page 139 of the samizdat I sent you for the original citation.)

Starting on page 133 of the samizdat, I write a fairly strong polemic against the "requirement" of strong-consistency—it seems to me that regular Dutch-book consistency is the most we can demand. Here's my question. As you'll see from the quote of Ian Hacking on page 137, de Finetti was aware of Shimony's addition to coherence and apparently rejected it. Do you think it might be possible to find out where he wrote about this? More importantly, what were his particular reasons for rejecting the stronger notion?

04-03-02 *Sliding Off the Deep* (to H. J. Folse)

I was able to print out your paper without a hitch. And thanks moreover, for your detailed notes on my letter to John Preskill. Let me try to answer some of your questions (in particular the one about Popper and propensities) by sending you still more rubbish. It's in the form of a follow-on letter (to the Preskill one) that I sent Bill Wootters while still in Japan. Of course, I would appreciate any further comments like your last ones if you've got the time!

Let me quickly reply to two of your points explicitly.

Folse-ism 1 : *Do any physicists still think this way? I realize cosmologists talk about a TOE, but surely the conduct of contemporary research is not animated by the thought that we're closing in on some "final" description of the universe? That was true enough a century ago, but I think that the weight of philosophy of science at least in the 20th century has pulled against that sort of image of science – certainly in a post-Kuhnian era. It is curious perhaps that this kind of attitude might still persist in so-called "foundational" studies, whereas I would suppose in something like biological research everyone would have become much more historicized by now.*

Nope, the attitude runs pretty rampant in the quantum information and computing community. (I'm definitely one of the outsiders there.) Have a look at David Deutsch's *The Fabric of Reality*. I think he expresses much of the majority opinion of our little clique there. But I think you'd be surprised to know that the "dreams of a final theory" attitude runs pretty rampant even in such workaday fields as laser physics. I just got the following message from an experimentalist friend of mine whose work is predominantly used for the design of better fiber optics:

El Jeffy 4: *One of the thoughts that continues to strike me is your optimism in the continual evolution in physical theory. I will admit that on bad days I feel like we are in the twilight of physics - only incremental progress is left to be had.*

Of the physicists I have met who have even heard of Kuhn or Rorty—there aren't that many—almost all of them view what little they know of their thoughts (i.e., Kuhn and Rorty's) with a little contempt. (By the way, John Preskill's reaction to my letter was particularly violent. So I think it is the ideas, and not the men, that bug their sensibilities.)

By the way, I have indeed just discovered Rorty and I find him wonderful. I've read the two collections of essays *Philosophy and Social Hope* and *Consequences of Pragmatism*. It won't be long, and I'll move on to the book of his that you recommended. Also since Växjö, I've gotten a good feel for James (I read *Pragmatism*, *The Meaning of Truth*, and Perry's massive biography of him), and also of Dewey (though I've only read *John Dewey: The Essential Writings*).

Thanks for telling me about Kitcher; I hadn't heard of him. And thanks for the pointer to Toulmin; I'll look more deeply into him than I have in the past.

Concerning one of your other points,

Folse-ism 2: *It is arrogant perhaps, but I don't see this as the egocentricity. Every attempt to sketch a conception of the universe from our best theories at any date in human history in effect commits such arrogance. Were the Newtonians of the end of the seventeenth century being "egocentric" to think that Sir Isaac had done nothing less than peer into the mind of the Divine and discerned God's blueprints for the universe?*

Yes. (In my opinion.) And you might interpret James and pragmatism in general as a reaction to that. However, I think in our modern age with quantum mechanics we have a motivation and opportunity in front of us that James did not have. Try to give quantum mechanics a naive realist interpretation—you can do it, or at least both Everett and Bohm tell us we can—and you find yourself contorting yourself beyond belief. It's as if nature is telling us for the first time, "Please don't interpret me in a naive realist fashion. I can't stop you, but please don't."

OK, the sun is rising and the vampire must return to his native soil.

04-03-02 *The Good Questions of Nicholson* (to J. W. Nicholson)

El Jeffy 5: *So. My question to you is two-fold. Do you still feel the same way about particle physics experiments that try to continually increase the energy of interactions between particles? And, if not, what extrema in physical theory should we be poking at in order to continue the evolutionary progress?*

You ask a good question old friend—the second one—and I shall have to think a while before I reply. Or at least compose myself before I reply.

In the case of your first question the answer is easy. Yes, I still feel that way. And the feeling is not that the SSC would not have been worthwhile science per se; it might have been. It was just the cost versus insight-and-control-of-nature ratio that doomed it in my mind. Furthermore, the paradigm the high-energy types presuppose certainly puts me off. They think they are doing something deeper than tabulating the Hamiltonian of an iodine atom, say, but I don't think they are. They are just tabulating the Hamiltonian of another system, and that has the value it always has.

But let me think harder about how to answer your second question. For my present project (i.e., quantum mechanics), it strikes me not so much as poking at the extrema that we ought to be

spending our time on, but in poking at the consistency of the worldview we wish to embed the theory in. To revert to my analogy of the construction of general relativity, it was only consistency that Einstein was seeking when he first started contemplating how gravitation fit within the framework of special relativity. Seeing that it did not (and weeding out all the coordinate effects as I expressed earlier) is what led to the bending of the manifold.

But don't take that as my definitive (or exhaustive) reply. I'm still thinking.

06-03-02 *Poetry on Concrete* (to L. Hardy)

I decided to write this letter in LaTeX so that I would have a small chance of being clearer. A few months ago, you wrote me this:

Hardyism 1: *[M]y basic picture of preparation/transformation/measurement is rather similar, though considerably less poetic, than your picture of humans interacting with a world. ... After the poetry you need to make the journey from vision to concrete construction.*

You're quite right. I think your paper has finally started to mix the right sort of concrete to support a building like the one I want to see built.

In what follows, all I'm going to do is keep true to this word I learned—poetaster—and try to write a little amateur poetry at your construction site. There are three things that really intrigue me in your paper, and one that really confuses me. I'll take them one at a time.

The first is simply the absolutely beautiful ease with which you bring us to a vector space structure. I had known about the concept of "informationally complete" POVMs for years[3]—they're even a crucial part of the proof of our quantum de Finetti theorem[4]—but before your work I had never appreciated so clearly that the probability distributions they generate ought to be taken as the very most primordial representation of a quantum state.

> *What is a quantum state? Nothing more than one's gambling commitments with respect to the outcomes of a fiducial measurement. Full stop.*

Or at least, that's the way I want to view it.[5]

Now, what you write presently is not completely consistent with that characterization. I'm going to do my best to try to make that plain to you. You write, "I don't believe that my approach needs to adopt a particular approach to interpreting probability." There is a sense in which that is certainly true for the mathematics you've already performed. However, if we're ever going to get past this foundational impasse in quantum mechanics, I would say firmly that we cannot leave the story there.

Here is the difficulty. If you take an objective approach to probability, then you are necessarily left with the conclusion that a measurement "here" in general changes something objective (or physical) over "there." And you will never get around that. Just consider any entangled state. Making a measurement on Alice's side changes her ascription—without any considerations of time or distance—of the state on Bob's side. If a state is purely equivalent to a set of probabilities and probabilities are not subjective degrees of belief or gambling commitments, but rather objective

[3] I think the idea can be traced back to E. Prugovečki, "Information-Theoretical Aspects of Quantum Measurements," Int. J. Theo. Phys. **16**, 321 (1977).

[4] See Section IV of `quant-ph/0104088`.

[5] And I say this even though I was saying things right after the Växjö meeting like I said to Khrennikov in the letter of 4 July 2001 attached below. Somehow, I just never took the idea so seriously as I do now, i.e., after reapproaching your paper in Japan. "Fiducial measurement" + "a notion of transformation from one measurement to another that carries the probabilities with it" is what tells the whole story!

and independent of the agent, then something physical must change at Bob's site with that far away measurement. And if you believe that, then you end up with conundrums out the wazoo.

I know that may not be your stand yet, but it is mine. Thus I'm left with the task of putting your mathematics into a framework I can accept. The first and clearest example of a technical mismatch between our views is what we each might consider as a valid motivation for the convex-linearity rule in your Eqs. (41)–(43).[6]

I think if we were to trace the roots of our mismatch, we would find it in that I do not subscribe to your "basic picture of preparation/transformation/measurement" ... which I think you think is common to both of us. Here's the way I put it to Asher Peres when we were constructing our Physics Today paper:

> In general I have noticed in this manuscript that you lean more heavily on the word "preparation" than we did in our letter to Benka. ... Unless I misunderstand your usage of the word, it may actually be a little too anthropocentric even for my tastes. The problem is this: consider what you wrote in the paragraph about the wave function of the universe. It seems hard to me to imagine the wave function of those degrees of freedom which we describe quantum mechanically as corresponding to a "preparation." Who was the preparer?
>
> It is for this reason that Carl Caves and I prefer to associate a quantum state (either pure or mixed) solely with the compendium of probabilities it generates, via the Born rule, for the outcomes of all potential measurements. And then we leave it at that. Knowing the preparation of a system (or the equivalence class to which it belongs) is one way of getting at a set of such probabilities. But there are other ways which surely have almost nothing to do with a preparation. An example comes about in quantum statistical mechanics: when the expected energy of a system is the only thing known, the principle of maximum entropy is invoked in order to assign a density operator to the system. There may be someone beside me in the background who knows the precise preparation of the system, but that does not matter as far as I am concerned—my compendium of probabilities for the outcomes of all measurements are still calculated from the MaxEnt density operator.
>
> To help ensure that I was not jumping to conclusions on your usage of the term, I reread today your paper "What Is a State Vector?" [AJP **52** (1984) 644–650]. There was a time when I agreed with everything you wrote there .... But as of today at least, I think a more neutral language as in our letter to Benka is more appropriate.

Thus what I seek is a picture that involves only measurement and transformation. (And if the truth be known, I'd like to get rid of transformation *in a sense*, leaving only measurement behind from your trinity. But that's a longer story, and I'll only give hints of it below.) However, I do think your formalism is just about up to the task, despite your Figure 1 on page 4.

The way I see it, this thing called the "preparation" is just a token for the *right and true* quantum state you imagine for a system. But from a steadfast-Bayesian[7] point of view, there is no such thing as a right and true probability assignment for anything. Consequently, if a quantum state is to be solely a probability assignment for the outcomes of a fiducial measurement, from the steadfast-Bayesian view, there can also be no such thing as a right and true quantum state.[8]

[6] By the way, I have to take a little exception with your accounting rules for your axioms. In similar treatments from the "operational school," say of Kraus, Holevo, and Ozawa, a convex-linear or affine assumption (of exactly the same spirit as your Eqs. (41)–(43)) is always listed as a distinct axiom.

[7] Some would say "radical Bayesian."

[8] I have argued this point at great length in a correspondence with Caves, Schack, and Mermin predominantly. I'm going to post that on my website `http://netlib.bell-labs.com/who/cafuchs/index.html` at just about the same

Let me emphasize this once more at the purely classical level before going on. For the Bayesian there is no such thing as a *right* probability assignment; there is no such thing as a *wrong* probability assignment. A probability assignment is a set of numbers that signifies which gambles one is willing to make, period. How those numbers are set in any given instance is an issue that has nothing to do with nature. Instead it has everything to do with all the ugly things we try to keep out of science: one's emotions, one's intelligence, one's hopes, the rumors one has heard recently, and so on. In fact, a probability assignment has nothing to do with the world itself—in a sense it floats above the physical world.

What this boils down to is that—*within a Bayesian paradigm*—your argument for convex-linearity cannot make any sense:

> Assume that the preparation device is in the hands of Alice. She can decide randomly to prepare a state $\mathbf{p}_A$ with probability λ or a state $\mathbf{p}_B$ with probability $1-\lambda$. Assume that she records this choice but does not tell the person, Bob say, performing the measurement. Let the state corresponding to this preparation be $\mathbf{p_C}$. Then the probability Bob measures will be the convex combination of the two cases, namely
>
> $$f(\mathbf{p}_C) = \lambda f(\mathbf{p}_A) + (1-\lambda) f(\mathbf{p}_B) \tag{.1}$$
>
> This is clear since Alice could subsequently reveal which state she had prepared for each event in the ensemble providing two sub-ensembles. Bob could then check his data was consistent for each subensemble. By Axiom 1, the probability measured for each subensemble must be the same as that which would have been measured for any similarly prepared ensemble and hence (41) follows.

Here's the difficulty. What does the distribution $(\lambda, 1-\lambda)$ refer to? From the Bayesian view, it can only refer to a belief (or better yet, a gambling commitment) that Bob possesses concerning Alice's actions. On the other hand, the numbers $f(\mathbf{p}_A)$ and $f(\mathbf{p}_B)$ refer to a couple of alternate beliefs a completely different person, Alice, might possess. However—*from the Bayesian standpoint*—there is nothing *a priori* that can be used to rigidly identify Bob's beliefs as a function of Alice's ... as Eq. (.1) above would imply. Alice has every right to believe anything she wants; Bob has every right to believe anything he wants.[9]

For the Bayesian, all the action—all the science—is not in the probability assignments themselves that various individuals might make, but in how those assignments change upon the acquisition of new data (steered by the influence of rationality).[10] In particular, the important things to ferret out are the conditions in any situation where two observers will converge in their beliefs when they agree that they are acquiring the same data. Bernardo and Smith put it in a beautiful way:

---

time I send this note off to you. The file is called "Quantum States: What the Hell Are They?," and the relevant part with respect to my remark starts on page 19 and winds up somewhere around page 97. However, the most relevant notes are the ones to Brun-Finkelstein-Mermin dated 7 August 2001, to Caves-Schack dated 22 August 2001, and to Mermin dated 2 September 2001. I hope you will have a look at those at the same time as reading the present note.

[9]Nor does it help to try to divert the discussion from a two-player situation and stuff all of these beliefs into a single head (Alice's say). For at any instance, Alice believes what she believes. And there is nothing within the Bayesian creed to force what she ought to believe in various other hypothetical situations—for example, where the distribution $(\lambda, 1-\lambda)$ might describe the amount of amnesia she has concerning a previous belief that she's just forgotten.

[10]Read my note to Mermin titled "Reality in the Differential" starting on page 128 of my previously mentioned posting.

> The subjectivist, operationalist viewpoint has led us to the conclusion that, if we aspire to quantitative coherence, individual degrees of belief, expressed as probabilities, are inescapably the starting point for descriptions of uncertainty. There can be no theories without theoreticians; no learning without learners; in general, no science without scientists. It follows that learning processes, whatever their particular concerns and fashions at any given point in time, are necessarily reasoning processes which take place in the minds of individuals. To be sure, the object of attention and interest may well be an assumed external, objective reality: but the actuality of the learning process consists in the evolution of individual, subjective beliefs about that reality. However, it is important to emphasize, as in our earlier discussion in Section 2.8, that the primitive and fundamental notions of *individual* preference and belief will typically provide the starting point for *interpersonal* communication and reporting processes. In what follows, both here, and more particularly in Chapter 5, we shall therefore often be concerned to identify and examine features of the individual learning process which relate to interpersonal issues, such as the conditions under which an approximate consensus of beliefs might occur in a population of individuals.
>
> — pp. 165–166, Bernardo and Smith, *Bayesian Theory*

> What is the nature and scope of Bayesian Statistics within this spectrum of activity? Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way."
>
> — p. 4, Bernardo and Smith, *Bayesian Theory*

And, it is precisely this that I'm going to use to get to your Eq. (41) from a Bayesian starting point. But before I can do that, let me praise the second thing that I see as a deep contribution of your paper. This is the very *definition* of what a measurement is: It is *any* application of Bayes' rule consistent with your remaining axioms.[11]

To make some sense of this, let me put the problem into a notation that is slightly better for my purposes. Suppose the outcomes of a fiducial measurement are labelled by h and the outcomes of some other measurement we might contemplate are labelled by d. Then I will dually write the quantum state (via your identification of states with probabilities for the outcomes of a fiducial measurement) as a function $P(h)$ or as a vector $\mathbf{P}$. What I mean by an application of Bayes' rule is the supposition of a set of probability distributions $P(h|d)$ (or $\mathbf{P}_d$ in vector notation)—one for each d—such that

$$P(h) = \sum_d f_{\mathbf{P}}(d)\, P(h|d) \ , \tag{.2}$$

[11] There is no doubt that I have been predisposed to saying something like this for a long time. For instance, at the top of page 26 of my NATO paper `quant-ph/0106166`, I say, "Quantum measurement is nothing more, and nothing less, than a refinement and a readjustment of one's state of knowledge." The difference is, your paper brings this beyond just some metaphor. There I tried to capture the idea with my Eqs. (57)–(59), which are explicitly in terms of density operators. You on the other hand, do it with the real thing. I.e., you make it absolutely airtight that it is Bayes' rule operating in the background, and not just some noncommutative analog of it.

or alternatively

$$\mathbf{P} = \sum_d f_{\mathbf{P}}(d)\, \mathbf{P}_d \ , \tag{.3}$$

for some other probability distribution of $f_{\mathbf{P}}(d)$ over the variable d. When a particular data d is collected, one—at least tentatively—enacts the transition

$$\mathbf{P} \ \longrightarrow \ \mathbf{P}_d \ . \tag{.4}$$

This is what I claim you have taught us is "what a quantum measurement is."[12]

A measurement is any "*I know not what*" that enacts a transition of the form Eq. (.4). What is the variable d? How is it defined? It simply is not, except as a place holder in a particular instance of Bayes' rule. And that's it: That's the story of measurement.

Now, where does one get the convex-linearity of the probability rule $f_{\mathbf{P}}(d)$ in a Bayesian-happy way? The technique is to consider when it is that two observers will think they are performing the same measurement. That is to say, I could walk into the room and think to myself that the measurement device in front of me gives me warrant for the decomposition in Eq. (.2). You on the other hand, with a completely different set of beliefs and experiences, may think that the device warrants you to the decomposition

$$Q(h) = \sum_d f_{\mathbf{Q}}(d)\, Q(h|d) \ . \tag{.5}$$

When shall we say that we actually agree upon the measurement? Classically, the answer is when the statistical model made use of by each of us is the same:

$$Q(d|h) \equiv \frac{f_{\mathbf{Q}}(d)\, Q(h|d)}{Q(h)} = \frac{f_{\mathbf{P}}(d)\, P(h|d)}{P(h)} \equiv P(d|h) \ . \tag{.6}$$

That is, if we had hold of which one of the fiducial outcomes actually obtained, then we would draw the same meaning from it. Here, by "meaning" I mean how much we would feel warranted in revising our beliefs that a d would have popped up if we had instead performed an appropriate measurement for it.

By the way, notice one thing. If this account deviates from standard Bayesianism any at all, it is only in this. Never once did I invoke the *necessity* of a joint probability distribution

$$P(h,d) \equiv f_{\mathbf{P}}(d)\, P(h|d) \tag{.7}$$

in my description of Bayes' rule. Of course, such a function exists as a mathematical artifice—i.e., it has all the properties of a joint probability distribution—but one should not try to make any meaning for it beyond that. In particular—and, especially with regards to the quantum context—one should not feel it necessary to interpret the function as a degree of belief of the simultaneous *existence* of a true h and a true d.

OK, let me use that now to start talking about convex-linearity again. Suppose there are at least three agents on the scene, Alice, Bob, and Charlie. And suppose Alice subscribes to Eq. (.2)

[12]Note carefully that I used the word "tentatively" in this description. That is because there is a possibility of the further quantum phenomenon that measurement can be more invasive than supposed classically. What this means operationally is that the $\mathbf{P}_d$ may not ultimately concern the original fiducial measurement, but may instead be about a completely different fiducial measurement at the end of the process. See my discussion centered around Eqs. (57)–(59) of `quant-ph/0106166`.

for her description of what the device in front of her is about, whereas Bob subscribes to Eq. (.5), and Charlie still further subscribes to,

$$R(h) = \sum_d f_{\mathbf{R}}(d)\, R(h|d) \ . \tag{.8}$$

If that is as far as it goes, then there is nothing whatsoever we can say about the relation between $f_{\mathbf{P}}(d)$, $f_{\mathbf{Q}}(d)$, and $f_{\mathbf{R}}(d)$. Indeed there need be no relation whatsoever.

However, suppose Alice, Bob, and Charlie share the minimal belief that actually the same measurement is being performed with respect to each of their descriptions. Then by definition

$$P(d|h) = Q(d|h) = R(d|h) \ . \tag{.9}$$

Fine. Now consider those cases where, as it turns out,

$$P(h) = \lambda Q(h) + (1-\lambda) R(h) \ . \tag{.10}$$

Letting

$$G(d) = f_{\mathbf{P}}(d) - \lambda f_{\mathbf{Q}}(d) - (1-\lambda) f_{\mathbf{R}}(d) \ , \tag{.11}$$

we see immediately that

$$\begin{aligned} G(d) &= \sum_h \Big[P(h)P(d|h) - \lambda Q(h)Q(d|h) - (1-\lambda)R(h)R(d|h) \Big] && (.12) \\ &= \sum_h \Big[P(h) - \lambda Q(h) - (1-\lambda)R(h) \Big] P(d|h) && (.13) \\ &= 0 && (.14) \end{aligned}$$

since

$$f_{\mathbf{P}}(d) = f_{\mathbf{P}}(d) \sum_h P(h|d) \equiv \sum_h P(h)P(d|h) \ , \tag{.15}$$

and so on.

And that's it, from this perspective.

$$P(h) = \lambda Q(h) + (1-\lambda) R(h) \qquad \Longrightarrow \qquad f_{\mathbf{P}}(d) = \lambda f_{\mathbf{Q}}(d) + (1-\lambda) f_{\mathbf{R}}(d) \tag{.16}$$

when and only when the agents who hold the beliefs $\mathbf{P}$, $\mathbf{Q}$, and $\mathbf{R}$ "agree" to the meaning of the d-"clicks" the measuring device will elicit.

Part of me fears that you're going to view all this as little more than rhetoric. It took me five pages of explanation to do what you did in one paragraph. So let me try to reiterate the point of this exercise from my perspective one last time, before I throw in the towel. Here's the point. If you believe that the quantum state is rigidly (or even loosely) connected to reality, then—it seems to me—you will never find a way out of the conundrum of "unreasonableness" associated with "state-vector collapse at a distance." I.e., our community will always be left with a search for the *mechanism* that makes it go. Our community will always be left with the embarrassing questions to do with its clash with Lorentz invariance. And, maybe most importantly, we will be left with the nagging question of why we can't harness this mechanism for more useful purposes.[13] I view

[13] If we can't see angels, why posit them? If we can't see superluminal communication, why posit it? Alternatively, if we do posit angels, the natural question to ask is how might they help save our souls. I would be suspicious of a world where the angels served little purpose other than rounding out a theology and not aiding in our souls' deliverance.

these questions as a distraction from the ultimate goal we all ought to have, of building a more interesting, more fantastic physics.

In connection to the present discussion, I would claim that THE preparation is little more than an anchor for such a rigid connection. It is, even if implicitly, a reversion back to the old issues that led to all the trouble in the first place. Thus I am compelled to find a way to absolutely disconnect the quantum state from the physical world. Yet I am still required to make sense of what it is that we're doing when we practice quantum mechanics. For this, the trail has already been blazed by (radical) Bayesian probability theory, and thus I take that as my cue. Getting at Eq. (.16) from within a subjectivist framework is one of those first steps you just have to take ... and then hopefully never have to think about again.

OK, with that, I'm ready to praise you my third and final time in this letter. But I think you'll probably hardly feel it's a praise. Let me tell you another goal of seeing how much of quantum mechanics can actually be run through with complete airtight consistency from the subjectivist standpoint. And that is to pick the theory apart. For, you see, I see no difficulty whatsoever in imagining that any theory can consist of two basic components—the completely subjective and agent-dependent, and the completely objective (or intersubjective if you will) and thus, agent-independent. What is the distinction between these components (I hope you ask)? It is this: Once an agent posits one of the objective components in any particular instance (whether it's "really" there are not), there is no move *within* the theory that will allow him to change that supposition.

Let me give you an example of the latter. Posit a bipartite system with Hilbert spaces $\mathcal{H}_{d_1}$ and $\mathcal{H}_{d_2}$ (with dimensions d_1 and d_2 respectively) and imagine an initial quantum state for that bipartite system. Now, I would say that the quantum state must be a subjective component in the theory because the theory allows me localized measurements on the d_1 system that can change my quantum state for the d_2 system. In contrast, I would say that the number d_2 that was posited for the second system is an objective component in the theory. There is nothing I can do at the d_1 site that will allow me to change the numerical value of d_2. The only way I can change that number is to scrap my initial supposition. Thus, to that extent, **(on first pass)** I have every right to act as if the numbers d_1 and d_2 are potential "elements of reality."[14]

Here's where I really think you sell yourself short by advertising your system as an extension or generalization of classical probability theory (with classical probability theory as a special case that's gotten by deleting one of the axioms). For I would say that your framework of "states" as vectors and "measurements" as applications of Bayes' rule is *classical probability theory*, full stop. Or, I should just say "*probability theory*, full stop"—for, the word "classical" seems to imply that it is a subject somehow within empirical science (rather than "law of thought" that antecedes science). In showing me that even quantum "measurements" can be viewed legitimately as nothing more than applications of Bayes' rule, you have done me a great service. For you demonstrate to me more clearly than ever that the concept of POVM ought to be put onto the subjective side of the shelf when I tear quantum mechanics into its two components. But your other intriguing axioms—like the simplicity and composite-system axioms—which you think give the possibility of generalizing upon classical probability, I would say are nothing of the sort. Instead, I would say they express just the opposite. These axioms seem to me to say something about what we are positing of nature. They express something that is not subjective and is not "law of thought." They in fact form part of the restriction to probability theory that I asked for over and over in my `quant-ph/0106166`. Thus drawing those axioms out explicitly strikes me as great progress.[15]

[14] Please note that I emphasized the qualifier "on first pass." The reason for this emphasis will become clear if you read the letters to Preskill and Wootters that I have pasted into the present letter to you.

[15] And that is why this minor spanking counts as a praise!

Thus to come back to my example of Hilbert-space dimension. As I have already explained, I would say that quantity is a (potentially) objective feature of nature. But now, after understanding your paper better, I would say the same of your composite-system axiom. In fact, that axiom is a way of elucidating the very meaning of the dimension d. As such, it forms part of the "manifold-structure analogy" behind quantum mechanics which I tried very hard to explain to Preskill and Wootters in two further letters I'll paste below. (I hope you will read them along with this note, as I think they greatly elucidate what I was hoping to convey in the previous four paragraphs.)

There. Three praises; take them for what they're worth. They are the three things that really intrigue me about your paper. But I've only started my digestion process. I'll leave a discussion of the confusions until we get some single-malt in front of us next week.

15-03-02 *Ice Cream and Reciprocity* (to C. H. Bennett)

Thanks for taking care of Lucien last night. After a night of alternating between sweat and shivers, I seem to be on the road to recovery. A comment and a question.

Comment. I think you said that I should make an overlay for my quantum-axiom slide that says "Give an ice-cream reason, if possible," for each of the axioms. By this, you were indicating that my "Give an information theoretic reason, if possible," is a rather arbitrary thing to be asking. But, I say that it is not arbitrary precisely because the main object of our attention in the theory, the quantum state, specifies *probabilities*. It specifies how we ought to be taking nature into account when we make our mortal decisions. The quantum state does not specify flavors of ice cream. Thus it seems to me like an entirely natural question to ask: If the main object of the theory is of an information theoretic (or decision theoretic or call it what you will) character, then how much of its support structure might also be of the same character?

Question. On the other hand, I rather liked what you said about wanting to base quantum mechanics on the idea that interactions are more symmetric in this theory than in classical physics. In interaction, both observer and observed are changed in the process. Could I get you to write your own version of that in a small paragraph, so that I can have something solid to read and think about?

27-03-02 *Still More Zing!* (to J. Gea-Banacloche)

Thank you for the long, beautiful note. I loved it, and it makes me so wish that I had had enough time to get to the rest of my talk in Dublin—i.e., the part where I try to give some substance to the word "Zing!" I introduced on one of the early slides. (Recall, "Zing!" was meant to be a place holder for the answer to the question "What is real about a quantum system?")

Gea-Banaclochesm 1: *You seem to have a pretty good idea of how to make most of the postulates (on probability, tensor space structure, and even wavepacket reduction) flow in a more or less natural way from some reasonable information-theoretic ideas, once you are given the basic formal structure of operators and Hilbert spaces. The main question would appear to be, where does this formal structure come from, and what does it actually say about the physical universe?*

That is the main question. And—*in spirit*—I believe our proposed answers appear to be essentially the same:

Gea-Banaclochesm 2: *the basic physical fact at the heart of quantum mechanics is the uncertainty principle, which one could formulate very generally as follows–*

(P1) The nature of quantum mechanical systems is such that, even when we have all the possible information we can have about them, we cannot, in general, predict the outcomes of all the possible experiments we could carry on them. Specifically,

The only issue in my mind is how to carry out the word "specifically" in a way that would satisfy the aesthetic I'm seeking. In particular, I would really like to pin down where the noncommutativity comes from in a way that does not make a priori use of the notions of "compatibility" and "incompatibility." In other words, I'd even like "incompatibility" to be a secondary notion, rather than a primary one. I think it is possible, and my present thinking is that it can be made to come out in a pretty way as a natural consequence of the mismatch between the number of bits that can be reliably stored in a quantum system and the number of measurement outcomes required for an "informationally complete" representation of a quantum state. (That is to say, something along the lines of the mismatch between the numbers N and K in Lucien Hardy's treatment in `quant-ph/0101012` and `quant-ph/0111068`.)

In any case, I have a discussion of "Zing!" in several places in my paper `quant-ph/0106166`. Especially in the closing section. Since then, I've come quite a way toward what I was trying to express above, but you'd have to dig harder for that—it's not exactly published properly yet. The place to look at the moment is in the file titled "Quantum States: What the Hell are They?" posted on my webpage (link below). The upshot is the following (working) statement: Each quantum system can be postulated to have an intrinsic amount of "sensitivity" to our experimental interventions upon it, and that sensitivity can be captured by a single parameter d (call it the dimension). From that, everything about the algebra of observables for a system follows from a basic statement about the very meaning of Bayes' rule in that context.

I'm working hard to get some of this in a proper paper presently.

By the way, I loved your Teilhard de Chardin quote:

> The history of the living world can be summarized as an elaboration of ever more perfect eyes within a cosmos in which there is always something more to be seen.

In my own way, I tried to express something similar in two pieces that I've come to be pretty proud of. Have a look at my letter to John Preskill titled "Psychology 101" starting on page 143 and my letter to Bill Wootters titled "A Wonderful Life" starting on page 149 of the file I mentioned above.

I hope that you yourself made it home safely and comfortably from Madrid, and also that you found your family doing well there. Emma's chicken pox are already clearing up: So maybe I was gone just the right amount of time!

28-03-02 *Quantum-Information Information* (to R. Duvenhage)

I just want to write you a very short note to tell you how much I enjoyed your paper `quant-ph/0203070`, "The Nature of Information in Quantum Mechanics" and to express how much similarity I think I see between our points of view. In particular, I think you expressed some things so very clearly that I would love to co-opt your phrases!

Here's where I think we agree the most:

1. "A measurement is by definition the reception of information by the observer."

2. "This renders many problems surrounding the measuring process in quantum mechanics no more difficult than in classical physics." And consequently,

3. Your discussion of the Heisenberg cut.

You can find some reflection of these ideas in my own paper `quant-ph/0106166`, though not put quite so succinctly as in yours. In particular I'm thinking of my discussion on pages 27 and 28 of that paper, following the earlier discussion on page 11. Also, I agree with your point about B's information being "invalidated" in your discussion on page 13 of your paper. Similar ideas make an appearance on pages 39 through 41 of mine. Finally, I also enjoyed your discussion of the linearity of time evolutions. That was the sort of thing, I was trying to describe in my notes of 22, 26, and 27 September 1999 on pages 408, 284, and 285, respectively, of my samizdat `quant-ph/0105039`.

However, I think I've come a long way since that paper and those notes. In particular, I think I'm no longer really willing to say that "quantum collapse is a noncommutative [generalization of] conditional probability." I think there is a sense in which quantum collapse is *precisely* an application of Bayes' conditionalization rule, modulo only a final redefinition of what the posterior probability is relevant to. What I mean by this in more detail can be found in my letter to Lucien Hardy beginning on page ??? of my collection "Quantum States: What the Hell Are They?" (which can be found on my webpage, link below), especially toward the end of the letter. In fact, I'm presently striving to write that up in an updated version of `quant-ph/0106166`, and I hope to place it on the server soon.

Anyway, I really want to point out the similarities in our thoughts rather than the contrasts. I think you've done the physics community a good service with your paper. It's very well written, and a lovely piece actually.

29-03-02 *Building with Bayes* (to B. C. van Fraassen)

Sorry to take so long to acknowledge your note: I've been running around Ireland with some bad phone connections and probably a few too many pints.

But I'm back now, and I did find your flyer waiting in my mailbox. I tacked it to the wall, but the subject's not likely to attract any of the physicists in my immediate vicinity. I would like to bring a visiting student with me though. Her name is Petra Scudo, and her present email address is `scudo@techunix.technion.ac.il`. She, as things turn out, did some undergraduate work in Pavia under a guy named Regazzini, who in turn was a student of Bruno de Finetti. While Petra is visiting (for a month and a week), I'm going to have her work on trying to pin down a kind of quantum de Finetti representation theorem for time-evolution maps. I.e., a theorem in answer to the question, "What is an 'unknown' quantum operation?" (In this context, the term "quantum operation" refers to the generalization of unitarity that is common in quantum channel theory — namely, the trace-preserving completely positive linear map. Sometimes people call it "open-system dynamics" but, from the Bayesian perspective, it is little more (nothing more?) than a noncommutative generalization of a conditional probability, connecting as it does the prior (quantum-state) assignment to the posterior (quantum-state) assignment.

Technically, I've made a breakthrough of sorts recently. I now know how to think of quantum collapse as *precisely* an application of Bayes conditionalization (importantly, followed by a redefinition of which measuring instrument the posterior probability assignment is relevant to). I call this a breakthrough because until recently (for instance see page 25 of my NATO paper, `quant-ph/0106166`), I continued to think of collapse as a noncommutative *analogue* of conditionalization. But now, using a representation of the quantum state similar to the one Hardy harps on, I can see that what is going on is the true-blue thing (i.e., simply Bayes in disguise). At the moment I'm working hard to get this written up in a sensible way ... or at least give my readers a hint of it, until I can do it properly.

By the way, in coming to all this, I've taken a more radical Bayesian turn than I had expected

at the outset. I.e., though I started my career in an Ed Jaynes kind of "objective Bayesian" camp, I'm now finding myself in the left of the de Finetti camp and maybe a little beyond that. In case it interests you, I've documented a lot of this transition in a new samizdat which I've placed on my webpage `http://netlib.bell-labs.com/who/cafuchs/`. The title of the file is "Quantum States: What the Hell Are They?" and it contains a lot of new correspondence with Mermin and others along these lines. (As an aside, I've significantly revamped my webpage; it's not so minimalist anymore, and maybe thus a little more attractive.)

While I'm here and I've invested this much into a long letter to you, let me make the thing even longer by tacking on two pieces from the above-said collection. I'll put them immediately below—one is a letter to John Preskill and one a letter to Bill Wootters. Both letters should be self-contained. Anyway, I place them here because I had forwarded them to Henry Folse a while ago, and he wrote me back a rather excited letter saying that I'm starting to explore some philosophical ground not so dissimilar from where Bas van Fraassen has gone. I wish I were in a position to judge the validity of that! But I just haven't read enough yet. (I'm trying to, believe me. But, being a physicist, I've got a lot of material to catch up on.) Anyway, until then, I'll keep my fingers crossed that maybe I can get some reaction directly from the horse's mouth. Are there similarities between our views? And what other pieces of your work should I be reading if there are?

Finally, concerning your seminar with Elga, I'd love to attend! So, please do keep me on the list.

06-04-02 *The Early Morning* (to A. Plotnitsky)

I laid in bed this morning, reading and thinking about your review of Feyerabend's book. What a pleasant way to wake up. I think you are right: the most important issue is, what is the right balance of construction? It plays a role—I am convinced—but how big of a role?

You flatter me by sending me your newest book! How can you be so productive, while all the rest of us just twiddle by? I will cherish the book and have it read before you know it.

07-04-02 *My (Hard-Earned) Pomposity* (to G. L. Comer)

Comerism 1: *Really? There's nothing to be learned from it* [Penrose's book]*? I mean I read Hawking's thing, didn't agree with everything, but did learn a few things.*

Sure, there's plenty to be learned by way of mathematical ideas and mathematical definitions and results. It's his implicit philosophy that I shun now, a philosophy that he's not even completely aware of (I think). And it's that philosophy (and the very troubles it causes) that makes up the two books whole reason to be. [...]

Let me come back to this Penrose thing again. It has to do with the words "rational," "logical," and "sane." Penrose is a believer in a kind of other-worldly realm beyond the physical world called the realm of mathematical truth. In that realm logic and rationality are defined in a timeless fashion, in a way independent of human frailty.

I've now come to think that that is hogwash. In positing such a thing, one misses the whole point of this wonderful world — that it is still under construction, and can be made *to some extent* in the image we want to make it. By this account, what is rational and logical is not in what is timeless and "right," but in what gives us the most *long-term* survival value. Survival value so that we can give rise to the most progeny (genetically and intellectually); survival value so that we can stand a chance to shape things. And that, as Darwin taught us, is not something set intrinsically, but is a function of our cumulative environmental pressures.

09-04-02 *Thanks* (to H. J. Folse)

IMPORTANT: See the correction to the present note in my note "Doing It and Doing It Right," dated 15 April 2002, to Henry Folse.

Folse-ism 3:

> That is, the quantum state is a compendium of Bayesian "beliefs" or "gambling commitments" and is thus susceptible to the type of analysis James gives in his "Sentiment of Rationality." Our particular choice of a quantum state is something extra that we carry into the world.

Whoa, this paragraph eludes my comprehension. I thought the measurement chose the state of the outcome.

That's a long story. Yes, measurement's can determine states, but my latest greatest realization is that they determine states in a way not so different from the way probability distributions $p(x)$ and $p(y|x)$ determine $p(y)$. $p(x)$ is the classical analog of the initial quantum state. $p(y|x)$ is the classical analog of the projection operator in the collapse rule. $p(y)$ is the classical analog of the final state. To the extent that in the Bayesian view *all* probabilities—even conditional ones like $p(y|x)$—are completely and utterly subjective, so are the meanings of the outcomes of one's quantum measurement (i.e., so are the basis vectors to which the collapses occur).

10-04-02 *Bitbol-ization* (to J. Bub)

Bubism 1: *What seems to me closely related to Bitbol's position is your emphasis on replacing the idea of a physical theory (you say 'law') as a mirror image of what is in the world (in what Pauli called a 'detached observer' sense), with an opposing view that takes seriously the fact that our science can only reflect points of view we can construct from within the world. I agree with you that this is the right way to look at quantum mechanics (although in my book I argued for the 'detached observer' view, which I identified with an Einsteinian view). Now I would say that quantum mechanics is not so much a descriptive theory of new sorts of non-classical objects (particles that are also wave-like, or particles with properties that hang together in a non-Boolean way, for example), as a theory of mechanics constrained by certain explicit limits to the process of objectification. (So, as Bohr said, there are no quantum objects. Quantum mechanics is not about how nature is, but about what we can say about nature. In this sense, it's a mechanical theory at the information-theoretic level, unlike classical mechanics – and the claim is that we are stuck at this level just because we are not 'detached observers.') Bitbol talks about the 'blinding closeness' of the world in the title to one of his books – a much more apt image than d'Espagnat's image of a 'veiled reality.'*

Sorry myself for taking so long to get back to you. Bitbol sounds interesting. I've put in a good word for him to Gilles; we'll see what happens. I absolutely love that phrase "blinding closeness."

Bubism 2: *Back to Bitbol and your earlier letter to me with replies to Preskill and Wootters. The essential point there seems to me very close to the neo-Kantian view about quantum mechanics that Bitbol has been developing in several books and articles.*

At the risk of getting into deep waters with a philosopher, I would say the point I'm pushing has a much deeper affinity to the philosophical tradition of pragmatism (James and Dewey's versions in particular) than anything of a Kantian flavor. But I've learned my lesson about saying the words "Copenhagen interpretation" to physicists. (See the story on the bottom of page 70 in the big Samizdat.) And similarly I'm learning a lesson about saying the words "James," "Dewey," and "pragmatism" in front of philosophers. The reactions I've gotten from Timpson, Brown, Donald, and Butterfield! Even when, in other contexts, I got such pleasant reactions from them about the ideas I was talking about . . . namely pragmatism without the explicit label. Sometimes a few words are worth a thousand (mental) fences.

11-04-02 *Zing D* (to C. M. Caves & I. H. Deutsch)

I am finally writing up my contribution to the Växjö conference proceedings, and in it I plan to make a statement about which elements of quantum theory I would be willing to call "ontological" if push came to shove. As Carl knows well, my favorite is the D. That is, for each system, when I ascribe to it a Hilbert space of some dimension D, what I am really doing is ascribing it an integer parameter of some (potentially) ontological significance. I do not let myself, however, assume such a significance for the states *in* the Hilbert space or the operators *on* it.

When I have that discussion, I would like to cite the stuff I saw Ivan present at the ITP meeting. (I saw it via the web.) Do you have paper written on that? If so, what are its coordinates? What I'll say is something like: If you're looking for the magic ingredient that powers quantum computing, it's not going to be first and foremost in the subjective elements of the theory. It's going to be in those things that stand a chance of being objective.

By the way, Ivan. I remember seeing one slide where you talked about the various points of view for just the question above—i.e., what might give quantum computing its power. In it you had one bullet devoted to "information-disturbance tradeoff." That was flattering, but surely it must be such a minority opinion as to not be an opinion at all!! (However, you ought to know what I'm aiming for: that that and D are the same thing, after all. So thanks in advance!)

13-04-02 *Quantum Locality* (to R. Garisto)

Thanks for sending me your draft; I will try to have a deeper look at it after my present project lets me come up for a bit of air. But, yeah, you're right, I can already tell I'm going to disagree with it. "What is the speed of quantum information?" It doesn't have a speed. It could only have a speed if you endow the state vector with an objectivity it does not have. QUANTUM STATES DO NOT EXIST (in and of themselves). They merely express the gambling commitments one is willing to make when one encounters a quantum system. But that does not leave the world empty; it just means that the quantum state is not part of its substance. Why is it that I should choose my gambling commitments to be in accordance with the structure of quantum mechanics? When we can answer that, we will have learned something clean and simple about the substance of the world. But until then, encumbering the world with an idea—nonlocality—that is clearly wrong-headed (without a heck of a lot of contrivance to try to make it go) will only distract us from the straight course to that great goal. What is it that makes quantum mechanics go? It is something deeper and far more interesting than the quantum state—that much I firmly believe.

I'm glad to hear that things are going well with you cancer-wise. Every time I think of you, I cross my fingers mentally for your health and happiness.

15-04-02 *Doing It and Doing It Right* (to H. J. Folse)

I was just looking my last note to you over again, and I was appalled at what I had written. Namely, I really botched my description of the classical analog to collapse. How, I could do that, I don't know. And I am ashamed of myself.

Here's the proper way to say it, and in fact the way I am just writing it up for a paper:

> In Section 8, "**What Else is Information?**," I argue that, to the extent that a quantum state is a subjective quantity, so must be the assignment of a state-change rule $\rho \to \rho_d$ for describing what happens to an initial quantum state upon the completion of a measurement—generally some POVM—whose outcome is d. In fact, the levels of subjectivity for the state and the state-change rule must be precisely the same for consistency's sake. To draw an analogy to Bayesian probability theory, the initial state ρ plays the role of an a priori probability distribution $p(h)$ for some hypothesis, the final state ρ_d plays the role of a posterior probability distribution $p(h|d)$, and the state-change rule $\rho \to \rho_d$ plays the role of the "statistical model" $p(d|h)$ enacting the transition $p(h) \to p(h|d)$. To the extent that *all* Bayesian probabilities are subjective—even the probabilities $p(d|h)$ of a statistical model—so is the mapping $\rho \to \rho_d$. Specializing to the case that no information is gathered, one finds that the trace-preserving completely positive maps that describe quantum time-evolution are themselves nothing more than subjective judgments.

16-04-02 *My Own Homeopathy* (to C. H. Bennett, T. A. Brun, C. M. Caves, P. Grangier & N. D. Mermin)

I thought of all of you with a smile as I was writing the footnote below for an upcoming paper. I'll send it to you now. Maybe I really do practice homeopathy. (Wait till you see the undiluted version.)

> In the previous version of this paper, `quant-ph/0106166`, I variously called quantum states "information" and "states of knowledge" and did not emphasize so much the "radical" Bayesian idea that the probability one *ascribes* to a phenomenon amounts to *nothing* more than the gambling commitments one is willing to make with regards to that phenomenon. To the "radical" Bayesian, probabilities are subjective all the way to the bone. In this paper, I start the long process of trying to turn my earlier de-emphasis around (even though it is somewhat dangerous to attempt this in a manuscript that is little more than a modification of an already completed paper). In particular, because of the objective overtones of the word "knowledge"—i.e., that a particular piece of knowledge is either "right" or "wrong"—I try to steer clear from the term as much as possible in the present version. The conception working in the background of this paper is that there is simply no such thing as a "right and true" quantum state. In all cases, a quantum state is specifically and only a mathematical symbol for capturing a set of beliefs or gambling commitments. Thus I variously call quantum states "beliefs," "states of belief," "information" (though, by this I mean "information" in a more subjective sense than is becoming common), "judgments," "opinions," and "gambling commitments." Believe me, I already understand full well the number of

jaws that are going to drop by the adoption of this terminology. However, if the reader finds that this gives him a sense of butterflies in the stomach—or fears that I am or will become a solipsist[1] or a crystal-toting New Age practitioner of homeopathic medicine[2]—I hope he will keep in mind that this attempt to be absolutely frank about the subjectivity of **some** of the terms in quantum theory is part of a larger program to delimit the terms that actually can be interpreted as objective in a fruitful way.

1. P. Grangier, private communication, 2001.
2. C. H. Bennett, T. A. Brun, C. M. Caves, and N. D. Mermin, various vibes, 2001.

19-04-02 *Lovely Circuits* (to N. D. Mermin)

Is there a typo in your paper, third paragraph? Two consecutive "before"s. If it's a clever construction, I didn't get it.

What do you use to draw your lovely circuits? I'm contemplating putting a figure of a circuit in my present paper, and I've never taken that bold step before.

"What the story demonstrates is the ability of entangled states to store interaction in a highly fungible form that need not be cashed in until the need arises." In comparing entanglement to classical correlation and making a point similar to yours, I once called entanglement "all-purpose correlation." This was great fun because it set me up to mention "Martha White's All-Purpose Flour," a product I remembered from my youth. What tickled me the most was that I got to cite Lester Flatt and Earl Scruggs, who sang the "Martha White Theme" during commercial breaks at the Grand Ole Opry. You might know them for the Beverly Hillbillies theme. It's the little things that keep me going, you know.

I'll tell you how if the paper stirs me.

21-04-02 *Walton's Mountain* (to G. L. Comer)

Today is the gloomiest, most drab day as far as the weather is concerned. But I like that. I always feel academic and reflective on such days. Kiki tells Emma, "If nature doesn't supply Daddy with a few clouds, he makes his own."

Lucent has been doing awfully poorly again in the market. [...] And the rumors are flying that about 10,000 more are going to get the pink slip Monday (after this quarter's losses are announced). [But] I'm not feeling too panicked at the present though. [...]

[F]or the present, I'm just happy being a high-payed philosopher, and I'm not worrying too much.

And the philosophy I've been doing! I can't put the pragmatists down; I read them in the morning, I read them in the night. I am so taken by the thoughts of James et al. I don't know how I could have missed these guys for years! It's a crying shame and certainly has stunted my growth. The pragmatists are all about the things John Wheeler was about (in his heyday), but oh so much deeper. John was an absolute amateur in comparison.

23-04-02 *Music in the Musician* (to G. L. Comer)

Thanks for the thoughtful note. I found myself thinking about it on and off all last night, in both my periods of wake and sleep. I think you expressed the issues to do with chemistry versus consciousness especially vividly.

I think we just have to get rid of this imagery that we are "made" of atoms. Or none of us are ever going to make any progress in our emotional lives OR our physical understanding. By my present thinking, a much better imagery is this. Take me and an old log: we both float in water. That is to say, we have that much in common. But there are a heck of a lot more things that we do not have in common. For any two entities, we can always find some characteristics they have in common, if WE are willing to ignore all the ways in which they are distinct. And that, I think, is the story of atoms. The atomic picture has something to do with what we all have in common. (Or, maybe more potently, it has something to do with what is common in the PART of existence that we have chosen not to ignore for the moment.) But to see the atomic picture shine through, we have to dim down all the things that are unique in us. Who said the particular shape of that rock is not important? Who said the pain you are feeling is only epiphenomena?

Such a picture of what physics and chemistry is about is every bit as consistent as the worldview Steven Weinberg, say, would have us believe. And I would say that it is more so; for it gives us a power and a hope for control in our lives that his can't even imagine.

Let me do two things for you. First, I'll paste in two old emails, that have to do with your music-on-the-page versus music-in-the-musician imagery (which I think it is so apt and so beautiful). Mostly I'm pasting them because your note caused me to go back and read them this morning. And I'm just reconfirming that I'm on the same wavelength.

But then I want to quote William James. (That will come in a little later note.) I know you're not much in the mood to read any philosophy right now. But if you read the RIGHT stuff, I cannot see how it cannot help. My side of the conversations with you, in any case, is just a poor reflection of what William James already said with such flare.

17 December 1997, to G. L. Comer, "It's a Wonderful Life"

Good holidays to you. This morning, as I was driving to work, it dawned on me that roughly this day 10 years ago, I was conferred my degrees at the University of Texas. Time does fly.

It made me think of a little anecdote about John Wheeler that I heard from John Preskill a few days ago. In 1972 he had Wheeler for his freshman classical mechanics course at Princeton. One day Wheeler had each student write all the equations of physics s/he knew on a single sheet of paper. He gathered the papers up and placed them all side-by-side on the stage at the front of the classroom. Finally, he looked out at the students and said, "These pages likely contain all the fundamental equations we know of physics. They encapsulate all that's known of the world." Then he looked at the papers and said, "Now fly!" Nothing happened. He looked out at the audience, then at the papers, raised his hands high, and commanded, "Fly!" Everyone was silent, thinking this guy had gone off his rocker. Wheeler said, "You see, these equations can't fly. But our universe flies. We're still missing the single, simple ingredient that makes it all fly."

Merry Christmas.

02 December 1997, to J. Preskill, "Flying Equations"

I couldn't help but think of the anecdote about John Wheeler's (non-)flying equations you told the other day when I came across the following little passage (presumably Biblical in origin):

"I forbade any simulacrum in the temples because the divinity that breathes life into nature cannot be represented."

23-04-02 *Installment 1* (to G. L. Comer)

From *Pragmatism*, pages 30–32:

And do not tell me that to show the shallowness of rationalist philosophizing I have had to go back to a shallow wigpated age. The optimism of present-day rationalism sounds just as shallow to the fact-loving mind. The actual universe is a thing wide open, but rationalism makes systems, and systems must be closed. For men in practical life perfection is something far off and still in process of achievement. This for rationalism is but the illusion of the finite and relative: the absolute ground of things is a perfection eternally complete.

I find a fine example of revolt against the airy and shallow optimism of current religious philosophy in a publication of that valiant anarchistic writer Morrison I. Swift. Mr. Swift's anarchism goes a little farther than mine does, but I confess that I sympathize a good deal, and some of you, I know, will sympathize heartily with his dissatisfaction with the idealistic optimisms now in vogue. He begins his pamphlet on 'Human Submission' with a series of city reporter's items from newspapers (suicides, deaths from starvation, and the like) as specimens of our civilized regime. For instance:

"After trudging through the snow from one end of the city to the other in the vain hope of securing employment, and with his wife and six children without food and ordered to leave their home in an upper east-side tenement-house because of non-payment of rent, John Corcoran, a clerk, to-day ended his life by drinking carbolic acid. Corcoran lost his position three weeks ago through illness, and during the period of idleness his scanty savings disappeared. Yesterday he obtained work with a gang of city snow-shovelers, but he was too weak from illness, and was forced to quit after an hour's trial with the shovel. Then the weary task of looking for employment was again resumed. Thoroughly discouraged, Corcoran returned to his home last night to find his wife and children without food and the notice of dispossession on the door. On the following morning he drank the poison.

"The records of many more such cases lie before me [Mr. Swift goes on]; an encyclopedia might easily be filled with their kind. These few I cite as an interpretation of the Universe. 'We are aware of the presence of God in his world,' says a writer in a recent English review. (The very presence of ill in the temporal order is the condition of the perfection of the eternal order, writes Professor Royce (The World and the Individual, II, 385).] 'The Absolute is the richer for every discord and for all the diversity which it embraces,' says F. H. Bradley (Appearance and Reality, 204). He means that these slain men make the universe richer, and that is philosophy. But while Professors Royce and Bradley and a whole host of guileless thoroughfed thinkers are unveiling Reality and the Absolute and explaining away evil and pain, this is the condition of the only beings known to us anywhere in the universe with a developed consciousness of what the universe is. What these people experience *is* Reality. It gives us an absolute phase of the universe. It is the personal experience of those best qualified in our circle of knowledge to *have* experience, to tell us *what is*. Now what does *thinking about* the experience of these persons come to, compared to directly and personally feeling it as they feel it? The philosophers are dealing in shades, while those who live and feel know

truth. And the mind of mankind—not yet the mind of philosophers and of the proprietary class—but of the great mass of the silently thinking men and feeling men, is coming to this view. They are judging the universe as they have hitherto permitted the hierophants of religion and learning to judge *them*. . . .

"This Cleveland workingman, killing his children and himself [another of the cited cases] is one of the elemental stupendous facts of this modern world and of this universe. It cannot be glozed over or minimized away by all the treatises on God, and Love, and Being, helplessly existing in their monumental vacuity. This is one of the simple irreducible elements of this world's life, after millions of years of opportunity and twenty centuries of Christ. It is in the mental world what atoms or sub-atoms are in the physical, primary, indestructible. And what it blazons to man is the imposture of all philosophy which does not see in such events the consummate factor of all conscious experience. These facts invincibly prove religion a nullity. Man will not give religion two thousand centuries or twenty centuries more to try itself and waste human time. Its time is up; its probation is ended; its own record ends it. Mankind has not aeons and eternities to spare for trying out discredited systems."

25-04-02 *Short Thoughtful Reply* (to C. H. Bennett)

Thanks for the picture of the skunk cabbage. I've always wanted one. I'm sorry I wasn't able to reply to you earlier, but with all the students visiting, etc., I've had a gazillion things going on at once.

Let me give you a very short reply, for what it's worth.

Bennettism 8: *My main wonder about your beliefs is why do you find it so important to emphasize the subjectivity of quantum states, . . . What difference does it make in any case?*

I am just trying to do what scientists always try to do: understand how things "hang together." I.e., build a (satisfying) picture of the world that has nothing to do with my personal qualities. It just so happens, that my favorite problem happens to be different from your favorite problem.

Bennettism 9: *Do you think Katie really exists, or is she just a mathematical symbol for a set of bets you would [be] willing to make?*

Of course I think there is a sense in which Katie exists (i.e., some large remnant of Katie as she is now would be here even if I were killed tomorrow). I just make a distinction between all the stories I might write about her in my samizdats and whatever it is that she *is* in herself. What I don't understand about you is why you find that such a foreign concept.

In particular, I don't think I could ever write a sentence like this:

Bennettism 10: *My main wonder about your beliefs is why do you find it so important to emphasize the subjectivity of quantum states, but not other kinds of information, such as the dinner you just ate, your shoes, or other people?*

I don't think I've ever thought of the dinner I just ate as the information I just ate. Presumably there is something substantial to broccoli independently of my subjective judgments about how it tastes. Information, as motivated by Shannon, has something to do with the concept of surprise. If I believe strongly that broccoli tastes bad, then I will be surprised if I find that I actually like it. In that sense I will find that I have gained a lot of information when my subjective judgment

makes a transition from its old value (Yuck!) to its new value (Mmm!). But that has nothing to do with broccoli as it is completely independently of me.

Even if I thought of a quantum state as an objective rather than a subjective quantity, I still don't think that I could ever talk as you did above. There is a difference between "systems" and "properties." And there is something in your language that seems to blur the distinction.

To make this concrete, take a classical description of a pendulum's motion in terms of phase space. I would never call the phase space point (x, p) "the particle." The particle is what *carries* the property (x, p). Within classical physics, both the particle and its property might as well be assumed to exist even when there are no physicists about. But let me ask the same thing about the Liouville distribution for a *single* instance of the particle. Can the single particle be said to "carry" a Liouville distribution in the same way it "carries" the coordinates (x, p)? I think you would be hard pressed to say that it does. For if I were to delete the physicist who is ignorant of the phase-space point the particle actually possesses, then I would delete the Liouville distribution. But I would not delete the value (x, p)—whichever one it is—that the particle can be safely assumed to have.

And that is all I am striving for in quantum physics. To figure out what properties we might safely ascribe quantum systems even when there are no physicists about. I think there are awfully good reasons for thinking that "the" quantum state is not such an "objective" thing. And thus the quantum state carries more of an analogy to the Liouville distribution than to the phase space point. However, that does not mean that I think our beloved theory gives us no hint of what the properties are that I can safely treat as objective. It is just that, among them, I do not see the quantum state.

Bennettism 11: *When you say a quantum state is just a set of bets you would be willing to make, what is the ontological status of you the bettor? Are you just a collection of bets some other bettor would be willing to make?*

Don't blur the distinction between the system and the state!

There, that's my short reply. As I say, I wish I could have replied to you earlier, but I had so many things tying me up. I wonder if I could make a birthday wish of you? Since my birthday was the 21st, would you give me this much of a present? Just read the parts of the paper I posted for `quant-ph` tomorrow, to do with Preskill and Wootters. I'll send it to you shortly. It's not long reading, and it's not hard reading. (Certainly no harder than a New Yorker.) And just give me two binary digits of satisfaction: After reading those passages, would you 1) say that you still do not understand my views of Katie, and 2) does it still look like a tower of turtles to you?

With enduring friendship (and a picture of skunk cabbage hanging on my wall),

26-04-02 *Transformation Rules* (to A. Peres)

Let me quote the piece of the paper you were concerned about:

> But the contexts are set by the structure of the Positive Operator-Valued Measures: one experimental context, one POVM. The glue that pastes the POVMs together into a unified Hilbert space is Gleason's "noncontextuality assumption": where two POVMs overlap, the probability assignments for those outcomes must not depend upon the context. Putting those two ideas together, one derives the structure of the quantum state.

What I was referring to with the point about the overlap is that one can derive the quantum probability law purely from the following simple assumption: there exists a function f from positive operators to the unit interval, such that the value of f sums to one over all positive-semidefinite resolutions of the identity. With this assumption (and nothing else, like continuity or differentiability), one gets that there must exist a density operator ρ such that $f(E) = \mathrm{tr}(\rho E)$. This is the modification to Gleason's theorem that I describe in `quant-ph/0106166` (section 4). So, it's not a completely trivial result of the standard probability rule; instead one can take it as an assumption and get the standard rule back out as a little gift.

Anyway, with this linear rule, comes for free the idea that probabilities transform linearly when one changes from one (sufficiently informative) POVM to another (sufficiently informative) POVM. And it is the transformations from "context" to "context" that Andrei has been making such a big deal about.

I hope that clarified a little bit.

03-05-02 *Accepting Quantum Mechanics—The Short of It* (to B. C. van Fraassen)

I'm feeling horribly guilty because I promised you a note over two weeks ago and everything—just everything—has conspired against me getting it constructed. I'm sorry. But still I want to write you something before I meet you next week. Let me try to do what I can in the next hour and then call it quits until Monday: The details of what I am about to say will be in a paper that I plan to have finished and can bring with me then.

van Fraassenism 1: *Now when it comes to theories that give us probabilities, whether absolute or conditional, I'll agree with scientific realists that literally read they say that there are objective probabilities in nature. But accepting such a theory does not involve believing that. Rather it involves appointing the theory as an 'expert' for guidance of our subjective probabilities concerning observable events. The metaphor of 'expert' is cashed out (as by Haim Gaifman) as follows. Suppose that I appoint Peter as my expert on snuffboxes. That means for my subjective probability P and Peter's subjective probability q the constraint:*

$$P\Big(A \,|\, q(A) = x\Big) = x \tag{.17}$$

with generalizations of this to intervals, odds, conditional probabilities, for statements A that are about snuff boxes.

Thus the issue of whether there are objective probabilities in nature or whether to believe in them is finessed: there are only the theory's probabilistic pronouncements *accepted as input and my own subjective probabilities.*

That is clearly not how you are approaching it overall. But perhaps there are connections? I'd like to know how the QM probabilities are fed into your subjective probability as a whole – I wonder if it will not be similar. After all, even if a quantum state is read as a compendium of probabilities, and you say something like "this material is in quantum state such and such", your own subjective probability function has a domain much larger than facts pertaining to this material.

Yes, you are right, I don't like (ultimate) experts, and I don't think quantum mechanics has any more need of them than weather forecasting, say. In fact, I think any attempt to hold on to objective probabilities—even in the finessed form that you talk about, where there is a higher authority in whose judgment we place our faith—will only get in the way of our finding the deeper

heart of quantum mechanics. Thus, I hold fast to the idea that there is no right and true quantum state EVER—just as de Finetti held fast to the idea that there is no right and true probability distribution EVER—and I take it as the very definition of my foundations program to see what is left behind. From my view, the theory does not pronounce probabilities; it only pronounces what we ought to be doing with them once we have set them just as subjectively as the next guy. (And to make the issue as pointed as possible, I even mean this for pure quantum states.)

What does it mean to accept quantum mechanics then? The imagery I am starting to build looks something like this.

If one generalizes the notion of quantum measurement to the one that has become essential in quantum information theory—namely to the positive operator valued measure (POVM)—then, for each quantum system, one can contemplate a *single*, fiducial quantum measurement for which the probabilities of its outcomes completely specify the system's quantum state. That is to say, whenever I write down a subjective probability distribution $p(h)$, for the outcomes h of such a fiducial measurement, I completely specify a quantum state ρ (pure or mixed). Imagine now that that fiducial measurement device is tucked away safely in some vault in Paris at the International Bureau of Weights and Measures.

A quantum state can be viewed as very literally nothing more than my subjective judgment for what would happen if I were to ever bring my quantum system up to that standard measurement device. What now can one say about a real-world measurement device, like one that we might have here at Bell Labs? Well, bringing my quantum system up to it will generally evoke a click of some sort that I might label d. Using all that I believe of the device, all that I might believe of Lucent's technical prowess, etc., etc., I would be a bad subjectivist if I didn't allow the click to update my beliefs about what would happen if I were to approach the fiducial device. Thus I end up with some updated probability distribution $p(h|d)$. (I.e., some updated quantum state.)

In what sense is this subjectivism connected to Bayesianism? This in part is what this paper of mine is about that I'll give you a copy of Monday. One can show that the usual story of quantum collapse can be viewed as the conjunction of two things: 1) Bayesian conditionalization, and 2) a final rotation of the axes of the probability simplex for the fiducial measurement. That is to say, quantum collapse in this description is only a pretty damned mild generalization of the Reverend Bayes.

But, again, what does it mean to accept quantum mechanics? It is this. If one studies the properties of these kinds of fiducial measurements, one finds that for no initial quantum state (in the usual Hilbert space picture) and no outcome is it ever the case that $p(h) = 1$. That is, when one accepts quantum mechanics, one eschews certainty for the outcomes of a fiducial quantum measurement. In fact, the set of allowed distributions $p(h)$ forms a convex set that is strictly contained within the probability simplex (i.e., the set of all imaginable probability distributions over an appropriate number of outcomes).

Thus, accepting quantum mechanics is not accepting the existence of an expert, but—in large part—accepting the two ingredients above:

1. voluntarily accepting a restricted range for one's beliefs $p(h)$

2. accepting a slightly modified form of Bayesian conditionalization for updating one's beliefs (i.e., standard Bayes + rotation)

The NEED for 1) a restricted range and 2) a minor modification of Bayes, is where I say we should be looking if we want to be looking for the "meaning" of quantum mechanics. What is it about the world as we view it that compels us to accept those two ingredients? That I see as the

important question. And the ever more convoluted moves I see from some of our friends who want to hold on to a nonsubjectivist view of the quantum state, I see as a waste of good brain power.

That is to say, I agree with you in that, "Be a realist if you want to be." But I add to it with respect to the interpretation of quantum mechanics, "Don't do it for those parts of the theory where it is not productive to do so." If you're looking for a little realism in quantum theory, fine, but then look for it in a more clever place than in the state vector.

Anyway, that's my present take.

And I lied: that took me an hour and 35 minutes. I hope it's a little clearer at least for the extra time. See you Monday!

PS. You wrote:

van Fraassenism 2: *The two articles of yours that we took up in our discussion group in the fall were clearly only the beginning, and you have now taken the program much farther.*

Can you tell me which two articles you're referring to? That would give me a clearer vision of which views I've changed since your reading and which views I need to be careful not to let be propagated in your mind.

05-05-02 *Wigner and Clones* (to A. Peres)

I just skimmed your new paper. Near your sentence, "Why wasn't that theorem discovered fifty years earlier?" I think you ought to cite Wigner as a case example. Below is a little review of his paper I published at `quickreviews.org`.

The original volume might be hard for you to get hold of, but the paper can also be found in Wigner's later collection *Symmetries and Reflections*, page 200. The spot where he just misses getting the no-cloning theorem—i.e., the spot where he actually gets it wrong—is in the second paragraph after the paragraph containing Eq. (5). (In the S&R version of the paper, it's at the very bottom of page 205.) He writes, "Let us denote the n vectors which represent living organisms by v^k ... Then every linear combination of the v^k will also represent a living state."

08-05-02 *The QMP* (to H. M. Wiseman)

Thanks for the letter, and particularly, thanks for skimming the paper. I chose a provocative title for the paper in an attempt to draw in the crowd, but the content of the two letters to Preskill and Wootters was pretty serious for me. What I'm looking for, in particular, is a way to make sense of science from a point of view that says at the same time, "Don't even think the terms in your theory ARE or CAN BE a reflection of reality. For the universe is big, and your head is small. And maybe reality is not static and unchanging—and thus describable in any finite terms, like in terms of GR and QM—anyway."

That's a sweeping statement, but I think it may be one of the two great lessons of quantum mechanics. (I'll keep my opinions secret on the other great lesson.) Anyway in particular, the way I see it, we as a community should be working as hard as we can to STOP trying to see the wavefunction as anything of a reflection of nature. Wavefunctions live in our heads. They live and die in our heads. And when they do their changing, they do that in our heads too. That's the point of view I'm trying to run through with as much consistency as possible. I.e., how can I hold to it, and still have quantum mechanical practice be what we all think it is? How can I hold to it, and not have everything that we say of nature be just a dream?

I'm sorry I've delayed so long in replying to you, but I wanted to do it right, and that required that I finish up a paper so that I could point you toward it. The paper's finished now, and I posted it on `quant-ph` this morning; it'll appear tomorrow. But if you wake up before `quant-ph` does, you can also get it at my webpage (link below). The title is, "Quantum Mechanics as Quantum Information (and only a little more)." The parts that are particularly relevant to our discussion are Section 4.2 and all of Section 6.

Wisemanism 1: *He implies that the same problems apply in classical theory. I disagree. The problem is that noncommutative algebra does not apply to the information in our brains. ... I'm not saying that it is impossible to do that, but it is an extremely difficult problem which he dismisses.*

Let me give an analogy that I think is apt. I would say Bohr's great genius in developing his model of the hydrogen atom lay solely in one little move: In dismissing the research program his predecessors laid out before him. That is, in dismissing the idea that the atom's spectrum required a mechanical explanation. Beyond that, the rest of his work was just details. Likewise, I think it is the case here.

I think that wavefunction collapse simply calls out for no explanation at all. And that is because I see it as nothing but a variety (and, in a way, an extension) of Bayesian conditionalization. To that extent, the problem—or, by my view, the nonproblem—was already there long before quantum mechanics ever showed up on the scene. Imagine that physical theory really were like the Newtonians wanted it to be: nice and deterministic, Laplace's demon and all that. Now consider a weatherman immersed in the world. He wakes up in the morning, thinks about all the weather readings he has taken the previous days, complains about the fact that his computer can't do as much number-crunching as he would like it to do, scratches his butt, decides whether he is feeling optimistic or pessimistic (that, of course, might depend upon how his girlfriend has been treating him lately), and so on and so on. He churns that all into a big mental pot, and finally comes up with a set of numbers $p(h)$ to describe his beliefs about what the weather will be doing tomorrow. In fact, at the same time, he'll probably come up with a whole set of numbers $p(h, d)$ to describe not only the weather tomorrow, but also the weather today.

Now, suppose he goes and has a look at the weather today and finds that it is d. Then, using Bayes' rule, he will update his belief for tomorrow from $p(h)$ to $p(h|d)$. It's a gut-wrenching, horribly discontinuous transition. But does it call out for an explanation? And if it does, does it call out for an explanation that has anything to do with the system the weatherman is modeling, i.e., the earth's atmosphere? For after all, in this world, when the weatherman looks out the window, one value of d was true and always remains true; one value of h was true and always remains true. The only discontinuous change is in his beliefs ... and those beliefs are presumably a property of his head.

So, yes, I think Duvenhage is absolutely right on this point. It's a point I've been trying to make for years—it's what Asher and I were up to in our *Physics Today* article—but of course sometimes it takes a long time to get the expression right. And I think Duvenhage did a particularly nice job on this particular score.

Now you and I both know that there's something more going on in quantum mechanics than there is for the weatherman. It's just that I don't think that "something more" is localized in the issue of "collapse." As far as the discontinuous change of belief goes—if you ask me—that happens both quantumly and classically, and in fact has nothing to do with any particular scientific theory. It is pre-science; it is simply a part of one's living and changing his beliefs in response to stimuli from the world outside himself.

By my present thought, part of the "something more" that goes on in quantum mechanics (as opposed to simple Bayesianism) is that we no longer have the right to assume that the things we do

in our laboratories to change our beliefs (i.e., quantum measurements) leave the world unscathed in the process. I.e., we ought to be taking into account that when the world stimulates us, we stimulate it back in the process. But how do we take it into account in our description? My answer is that there's only one place to put it in a formalism, and that is in a further change in BELIEF up and beyond that dictated by Bayes' rule.

Thus, quantum collapse deviates from Bayes' rule, but not because it has anything to with something going on outside our heads. It deviates from Bayes' rule because the subject matter we are talking about when we are doing quantum mechanics (i.e., quantum systems) have an implicit action-reaction principle that Bayes, Cox, Ramsey, and de Finetti overlooked when they first worked out the calculus of belief change—i.e., Bayes' rule.

Now, you say things like,

Wisemanism 2 : *The problem is that noncommutative algebra does not apply to the information in our brains. . . . If we could believe that non-commutative algebra did apply to our knowledge then of course that would solve the quantum measurement problem. But how can we reconcile that with our experience?*

but, by my present view, using words like that is a red herring. (And Duvenhage is much more guilty of it than you.) The way I would say it now—please read the sections in the new paper I told you about—is that any kind of noncommutativity in quantum mechanics is just an artifact of a certain representation. The usual representation is a useful one to be sure—I could hardly calculate anything without it—but for the present issue I think it detracts from the clearer understanding we can hope to obtain if we'll just suppress it. (The "it" meaning the usual representation.) In fact, I think we're only going to get that understanding by exploring quantum mechanics as 1) a restriction on the space of probabilities (the probability simplex), with 2) a conditionalization rule that goes just a bit beyond Bayesian conditionalization.

And all this causes me to reject it when you say,

Wisemanism 3 : *The QMP is to find (i) a cut, and (ii) a way to bridge the cut, between the quantum systems that do have non-commutative information (in his terminology) and classical systems that do not.*

That is not the quantum measurement problem for me. People, experimentalists, scientific agents, have information or lack information when they are trying to talk about something outside themselves. Systems are just systems, and when they are treated as such, I would say the concept of information has nothing to do with them. Information is something I have or lack about a system. For instance, when I am speaking about you—thinking of you as a physical system—I lack quite a bit of information in the sense that your behavior could surprise me at any moment. But that means nothing about any kind of "information" intrinsic to you.

So, when you say what you said above IS THE QUANTUM MEASUREMENT PROBLEM, I would say that that is a problem that comes from trying to think of the quantum state, or the algebra of observables, etc., as literal properties of the things you are describing . . . and failing to recognize that the quantum state is a property of the observer and not the system. The quantum state is the full compendium of gambling commitments you would be willing to make about what the system will cause a measuring device to do.

What I myself see as the quantum measurement problem is to give compelling reasons for the two items I listed above. By compelling, I mean in the terms I lay out in the paper. When we can finally do that, then we will finally understand what properties we are really assuming for the "reality" of a quantum system.

Anyway, I hope that helps explain my position. I'll attach another piece of correspondence below with Bas van Fraassen that takes a different tack on some of the same issues. Maybe that'll help supplement the paper. I hope you'll get a chance to take a look at it. By the way, I quote one of your papers in Footnote 33.

Thanks for giving me the opportunity for trying to say these things a little more clearly. And certainly feel free to question or comment on anything that still doesn't make sense. (Anything that doesn't kill me might just make me stronger!) Hey, I'm coming to Brisbane May 25 to June 17. Do you think we'll get chance to talk sometime during then?

10-05-02 *Go Ask Alice* (to G. L. Comer)

Well the days have come and gone, and now the week is just about over. I finally finished up my extension of the NATO paper posted on the LANL archive the other day. (I thank you in it, by the way. Have a look at `quant-ph/0205039`.) And the last of my visiting students, left yesterday. So I guess I'm getting ready for some kind of denouement in the coming week. [...]

Your note has had me singing that old Jefferson Airplane tune in my head all morning. (I got up a little before 5:00.) Torturous old man. I did really like that line of yours "Dualism is a Degeneracy." It strikes a chord in me with respect to my efforts to overthrow the idea that a scientific theory is (potentially) a mirror image of nature. I.e., it is not even potentially.

But I guess I have a hard intellectual time with the idea you express with: "[L]anguage is necessarily limited; beautiful, but just crude enough that my BElongINGs can never be completely shared." You can blame it on the Richard Rorty in me. You presume that there is a "person on the inside" that goes deeper than what can be built from language. That if we were able to conceptually strip all the rest of the world surrounding Greg Comer away, there'd still be something left. It's a long story, but I guess I don't buy that. The self is just a local "center of narrative gravity," Rorty put it. See his "Ethics without Principles" in his book *Philosophy and Social Hope*.

14-05-02 *No BC's Role* (to R. Schack)

Thanks for the flurry of letters. They provoke a lot of thoughts in me and I am grateful.

I'm not sure how I'm going to reply to you, maybe just randomly (as the details occur to me).

Schackcosm 60 : *In my discussions with classical cryptographers, I am often forced to concede that QKD is really quite limited in scope. You either assume an unjammable channel (which I believe cannot be assumed if you use the internet for communicating) or you use classical authentication, which means you share some initial key. Given these limitations, I am not convinced that QKD deserves such an exalted status as suggested by the Bub quote above.*

Has anybody taken the Brassard/Fuchs speculation any further?

Good point, of course. But I never meant for those two "axioms" to be read so literally. Perhaps my best expression of the idea is captured in a letter to Jeff Bub, 10 December 2000. It starts at page 100 of the old samizdat. The main point is that I see information-disturbance as the key idea, along with the commitment that information can never be completely locked away. Read those passages; I think they'll clear some of this up.

That said, precise versions of the no-bit-commitment ideas are coming to the top of my head again for their foundational value. Namely, as part of the extra assumptions that might get us to proper density operators in the "Wither Entanglement?" entanglement section (rather than simply

linear operators). The thing that really powers the no-bit-commitment theorem is the Hughston-Jozsa-Wootters result that localized measurements on one system of two (described by a bipartite pure state) can "induce" any decomposition one wants for the other system's density operator. It turns out that the "pure states" of those other wacky operators in my bipartite-Gleason construction don't necessarily have this property. So, it looks like an assumption of such a nature might get me a little closer to the goal. (Though—even if I found it completely acceptable, and I'm not sure I do—it still wouldn't get me all the way to the goal.) But all of this is a long story, and maybe it'd be easier to talk about at a chalkboard.

14-05-02 *More Toenails* (to R. Schack)

Schackcosm 61 : *Why are you so harsh on entanglement? In my quantum information lectures, I postulated the tensor product structure, as a natural formalism to deal with local operations on several particles. Entanglement is then derived, an unexpected consequence of the tensor product structure. You make this line of reasoning more compelling by showing that there is really no alternative to the tensor product structure. Far from being withered, entanglement emerges invigorated from your analysis.*

The main point is that entanglement can be thought of as secondary to the structure of quantum observables on localized systems. To that extent, one realizes that one can focus on the structure of simple observables in one's foundational efforts and forget about entanglement. In other words, it seems to me entanglement is not, as Schrödinger said, "the characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought." It is derived and secondary.

Now what is the structure of observables? The thing I try to argue in the paper is that a measurement is *anything* that gives rise to *any* convex decomposition of a one's original density operator. In that sense, measurement is nothing more than an arbitrary application of Bayes' rule.

Entanglement thus arises from the more basic idea of Bayesian conditionalization in conjunction with the idea that the allowed probabilities for a standard quantum measurement device do not explore the whole probability simplex.

Let me put two notes below that might clarify where I'm trying to go with this. I'd like to think that they add nothing beyond the paper, but it has been my experience that I can just never say enough. (I know what you're thinking: "You've got it backwards. If you'd just say it all in fewer words! That's what you really need.") [See notes to van Fraassen and Wiseman, dated 3 May 2002 and 8 May 2002, respectively.]

14-05-02 *Emphasis De-emphasis* (to R. Schack)

Schackcosm 62 : *I am not sure I like your emphasis: You describe what I think is the exciting part as a "further readjustment of the posterior state".*

Yeah, I think I agree with that. There's no doubt that that's the most exciting part for me. That extra adjustment strikes me as capturing our beliefs about how we are stimulating the system (rather than how it is stimulating us). And to that extent, I would like to emphasize it. However, I guess I chose the tack I did because I wanted people to stop thinking of quantum collapse as something so different from "mere conditionalization."

Schackcosm 63: *Personally I like the concept of a compendium of probabilities better than your bureau, but it is very interesting to see how far one can get with the Bayes rule alone.*

Again, here, it is a point of emphasis. The message I am trying to get across is that the structure of observables comes from Bayes' rule. They (measurements) are not defined independently of it. The SQMD struck me as an effective way to badger that point. "Compendium of probabilities" really stands for "compendium of ways of applying Bayes' rule." That is, I think it builds a stronger case for the idea that it is Bayes' rule all the way down when it comes to quantum measurement. The theory gives us no overt means to identify the objective thing that goes on behind a quantum measurement outcome. The only grounding we have is to *declare* a point somewhere off in the distance for which we will do no further updating. And that is the role the SQMD plays.

14-05-02 *Deletions and Their Obverses* (to R. Schack)

Schackcosm 64: *I think that taking William James and Darwinism seriously means to acknowledge that quantum mechanics is most likely to be superseded one day by a theory with even more cash value. This new theory may not have any of the features that we regard as the core of quantum mechanics. How then is Darwinism or pragmatism going to tell us anything about the foundations of quantum mechanics?*

You're right, I think you are definitely missing what I was hoping to express. But that probably just means I didn't express it so effectively.

No, I did not mean that Darwinism and pragmatism tell us something about quantum foundations. Just the opposite. I see quantum mechanics as giving us a great hint that there is still something deeper. Quantum mechanics is the first rip in the old fabric that told us our place in the world is a nullity. That is to say, I think quantum mechanics only gives us even better arguments for pragmatism.

Schackcosm 65: *It looks to me as if your desire to find the objective core of quantum mechanics is against the spirit of both Darwinism and pragmatism.*

I think the core of the theory—I don't think I ever used the words "objective core"—is just our best guess of what we cannot change with our present level of skills. It is our best attempt to imagine what it would be like if we were not here. Obversely, when we have ferreted the core out, we will have a quantitative indication of how much of the world we can hope to control (given our present skills, present evolutionary level, etc.)

My desire for delimiting the core is expressed particularly in the passages below (taken from the paper). Maybe this makes no sense without A) watching the movie *It's a Wonderful Life*, and B) reading the "Sentiment of Rationality." Have you done either of these things? Probably not.

Your point certainly makes it clear to me that my paper is not self-contained! (The movie is an American classic and I think a large fraction of Americans have probably seen it; but James is another story.)

14-05-02 *Imaginations* (to R. Schack)

Continuing again ...

Schackcosm 66 : *Actually, I found the part where you mention the selection of traits for your daughter outright disturbing, without any compelling connection to the discussion on quantum mechanics.*

Yes, me too. Because read in the wrong way—and maybe there was no other way to read it—it surely evoked images of Nietzsche (at the least) and Nazi Germany (at the worst).

But on the other hand, I don't know how to draw a meaningful distinction between our tools and ourselves. (Remember my point about the prosthetic hand in the paper?) Should we stop pursuing medical research because it goes against the grain of nature? Should we stop pursuing genetic techniques for controlling AIDS? Should we stop falling in love based on an attraction to our partners' complementary (positive) traits to our own? Traits that we would like to see (even if subliminally) appear in our children?

I guess I say no, no, no. Instead we need to cultivate at the same time a respect for everything in its time. Children of Down's Syndrome, say, deserve respect, not deletion. The Nazi experiment with nature was an atrocity. But, at the same time, I would say we cannot stop pursuing progress in genetic engineering.

The point you make is a deep one. I have no solution. Only an intuition of fear and promise, both at the same time.

If you get a chance, read Richard Rorty's book *Philosophy and Social Hope*. It's all about fear and promise and quite easy to read.

14-05-02 *More Still* (to R. Schack)

Schackcosm 67 : *Everything you write suggests that you want more than just the theory with the highest cash value: you are looking for some form of absolute truth, something that transcends looking for the theory that makes the most accurate predictions.*

Yes, I guess so. I am looking for an indication that the world can be moved. I am looking for an indication that the only law in physics is the law that there are no laws. I am looking for an indication that the world is still writable.

I think if you want to call those things the pursuit of an absolute truth, you can. But they're only absolute in a pretty negative sense.

See, I told you you provoked a lot of thoughts in me.

14-05-02 *Qubit and Teleportation Are Words* (to C. H. Bennett and others)

I doubt I will be of any use in constructing a short dictionary definition for the word teleportation, but let me try to explain my difficulty with the word "property" with regards to both quantum states and quantum entanglement. I do this with a little trepidation, but on the other hand, you're the ones who brought me into this discussion and I feel I ought to say something.

The trouble I have with the word "property" has to do with one of the main points Charlie brought up in his talk this weekend. What instantaneously and physically changes about Bob's system when Alice performs a measurement on hers? Charlie told us *nothing* and I agree with that. But then, I look in the *American Heritage Dictionary* and find:

property:

1) Something owned; a possession. ...
4) a. A characteristic trait or peculiarity, especially one serving to define or describe its possessor. b. A characteristic attribute possessed by all members of a class. See synonyms at **quality**.
5) A special capability or power; a virtue.

If you think of a quantum state as a property owned by the system of which it is about, then you—Charlie Bennett in particular—are obliged to continue propagating this thing you told us was a misconception. At the completion of Alice's measurement, there is a new quantum state for Bob's system. If the quantum state is interpreted as a "special capability or power" for the system at Bob's end, then you cannot get around the conception that Alice's twiddle caused a change to something localized way over there.

And that, it seems to me is dangerous business. What I am saying has nothing to do with hidden variables. It just has to do with the word "property". The trouble only has to do with the idea of a quantum state as a kind of feature possessed by a quantum system.

If you want to think of the quantum state as a property of something, it seems to me the best you can do is speak of it as a property of Alice's head (or Bob's head, or whoever's). For, the quantum state represents the *predictions* she can make about measurements upon the system in question. Similarly I could say all the same things about entanglement.

To my view, toying with the idea that a quantum state is a property, is to toy with a kind of pantheism or anthropomorphism that my materialist mind won't tolerate. Do all rocks have souls? You'd laugh at that, but it seems to me that's about what you're attempting to do in thinking of the quantum state as something possessed by the system itself. "That rock judges his chances of reacting to the measurement device to be such and such!" What could be more anthropocentric than that?

Bennettism 12: *"Properties" can reasonably be taken to be much broader than hidden variables, and may include all sorts of conditional and post-selected behaviors, eg "how a system would behave if I measured its Z spin component, after having watched my favorite horse lose at the race track."*

But, as I see it, conditional properties fare no better than the nonrelational type in this regard. At the completion of Alice's measurement, there is a new quantum state for Bob's system, and thinking of it as a conditional property in Charlie's sense still means something physical changed at Bob's end. This is because the system did not have that particular (conditional) property before Alice's action.

The main point is this: Whatever a property for a quantum system is, it should not be something changeable by someone's twiddles far, far away. For instance, take the dimensionality posited for a quantum system by the ascription of a Hilbert space to it. I think this is a perfectly good candidate for a property of a quantum system; it's one I would endorse. For, once set, there is nothing Alice can do at a far away location to change it.

So there, quantum systems do have properties—or at least I'm willing to bet they do. It is just that the properties do not include among them "the" quantum state ... and it is the quantum state that is transferred in the process of teleportation.

What I find miraculous about teleportation is that Victor (the guy who ascribes the original, unknown state) can transfer his predictions from one physical system to another at the cost of only two bits of physical action on the target system. In that sense, it is Victor's description that is teleported from one system to the other with almost nothing whatsoever traveling in between. But that'll never make it into a dictionary.

Of course, this is an ongoing debate between Charlie and me. I'll paste below one piece of our correspondence that has to do with the present conversation—it itself focused on the issue of properties. [See note to Bennett dated, 25 April 2002.]

By the way, I agree with Bill that the word "object" is better than "system." *American Heritage* writes this in its first two definitions for the word:

> **object**:
> 1) A material thing.
> 2) A focus of attention, feeling, thought, or action.

14-05-02 *Chris's World* (to J. A. Smolin & C. H. Bennett)

Good to see you in the morass.

Chris's world: It's a funny place with all these fancy words like "ascription" and "posit" to remind us that there's a head in the background of every quantum state, but there's no instantaneous action at a distance there—no one would have ever thought there might be. I know the language drives Charlie bonkers, and it probably drives you bonkers too. But mostly the complaints just remind me of what I used to hear in my hometown in Texas when the seatbelt law was first enacted. "Why that Majatek boy was thrown clean from the car! Not a scratch on him. If he'd have had a seatbelt on, his head would have been nothin' but mush now; we'd be at his funeral today. Damned politicians puttin' their noses into places where they ain't got no business."

So you see I view the language as a safety measure—one that I think will allow us to drive farther, longer, and ultimately faster. But first you've got to learn how to drive with a belt on.

Smolinism 1 : *One is naturally forced into Chris's world of saying all that changes is what people predict about the state, but that's a property properly defined as above.*

Chris would never say this. When I write down a quantum state, I think of it as my judgment or prediction for which of one or another measurement outcome will turn up. I don't predict things "about the state." I don't know what the TRUE state is or could be, and thus my ignorance cannot be about it. In contrast, I would say the ignorance is always about further measurement outcomes. Or a pleasing picture is that one can ground the ignorance with respect to a single device sitting in the National Bureau of Standards if one wishes. See Sections 4.2 and 6.1 of the fat paper I put on `quant-ph` last week (`quant-ph/0205039`).

That said, I also see the suppression of another crucial issue here—one that also flies in the face of the word property, especially with your emphasis on DEFINE in definition 4. If you will allow me to call a mixed density operator a quantum state, then I know even you will agree that there is no unique quantum state for a system. Thus it cannot be a property. So, I think you'll be left with being only willing to call a pure state a property. So be it. (I wouldn't do that, but I'll let you do that for the time being.) But now, let's go back to Alice and Bob. By your account, first Bob's system has no property, then Alice measures her system, and—Zing!—now Bob's system does have a property. I.e., first it has no quantum state, then it does. How does the system know it ought to have that property if there's no action at a distance? Alternatively, if it doesn't know it, why call it a property of the system?

It is because of that conundrum that it seems to me to be more fruitful to just give up thinking of the quantum state as a property of the system it targets.

I think a good analogy can be found in *classical* information theory. A homework problem in a textbook gives you a discrete memoryless channel by specifying the transition probabilities $p(y|x)$.

Then it asks that you calculate the channel's capacity. One goes through all the work and gets a number. From that, one starts to get the feeling that the capacity can be an objective property for a real physical channel. Why else would it take so much work to calculate something if it weren't real? But it can't be real in any absolute sense. For, with respect to a Laplacean demon, there is never any noise in the channel at all; he can predict which bits will be flipped and which won't. The point is, the capacity is only objective WITH RESPECT TO a *subjective* judgment $p(y|x)$. Similarly, I would say with all quantum states. Just because a textbook says calculate such and such a property—the entanglement of formation, the distillable entanglement, or whatnot—of a quantum state, one finds that one gets into the same habit of thinking those properties have no subjective component.

You can do so, but then it's your burden to explain to young school children and journalists—who think of a property as something like the color red, a ball either has it or doesn't—what changes in Bob's system when Alice performs a measurement.

15-05-02 *Marburg, Strasbourg, Blunderburg!* (to A. Peres)

Thanks for catching those mistakes. I'm a little ashamed that I made them. And I am flattered that you are reading the paper; thank you for that independently.

Yes, an older version of the paper will appear in the Växjö proceedings. The version on `quant-ph` has 25 or 30 spots where the language has been changed somewhat. I have contemplated trying to send the final, final version to Andrei. But I don't know that it is worth my while. With Andrei's whirlwind methods, I suspect it is too late for me to send him revisions in any case. Of course, the only version people will ever read—if they read that much—will be the `quant-ph` version.

I just downloaded your no-cloning history to see how you took into account Wigner's paper. I was a little surprised when I didn't see you mention him. I would guess that you made that decision because you did not muddy Wigner's name. Still, though, I think his blunder only adds to the importance of the theorem (despite its mathematical triviality), and I wish the readers could have seen that at the same time that you defend your decision on FLASH.

Kiki and I arrived back home from Ithaca late Sunday evening, with a hobbling car. ...

The meeting in Ithaca was quite nice, with quantum informationists, quasi-crystalists, bioligists, magazine editors, science policy advisors, etc. I think the best thing that happened to me was that I met Michael Berry for the first time. He introduced himself as I was talking to Philip Pearle. He said, "Oh, you're Chris Fuchs?" I thought, "Michael Berry knows of Chris Fuchs?" It was quite a surprise to me. Anyway, he proceeded to tell me how much he liked our Physics Today piece, and how he quotes from it to his students. During the meeting, I also gave him copies of my two Växjö pieces and he gave me some good feedback on both. Sometimes, I need confidence builders like that.

15-05-02 *Berry, Etc.* (to A. Peres)

Asherism 4: *I didn't quote Wigner. Why kick a dead horse?*

I accept your decision. But still I contend that you missed a good opportunity. The point is not to kick a dead horse, but to glorify a living theorem. Wigner was one of the greatest minds in physics this century—nothing can take that away—but yet he came to within an inch of the theorem and then missed it. I think that signifies something interesting psychologically. Perhaps it also signifies what a radical departure quantum mechanics is from classical lines of thought.

15-05-02 *Bayes, POVMs, Reality* (to A. Shimony)

It was good talking to you this weekend. If you wouldn't mind committing your story of meeting de Finetti to email, I will see to it that it is archived forever in one of my samizdats. (See my webpage; link below.)

Also, if you could send me your mailing address, I will send you copies of my two new papers on Bayesianism, POVMs, and good candidates for quantum reality. Alternatively, if you are accustomed to downloading things from the quant-ph archive, here are the links:

1. "Quantum Mechanics as Quantum Information (and only a little more),"
   `http://xxx.lanl.gov/abs/quant-ph/0205039`

2. "The Anti-Växjö Interpretation of Quantum Mechanics,"
   `http://xxx.lanl.gov/abs/quant-ph/0204146`

I wrote them both in an attempt to be entertaining. I hope you find them so.

Shimony's Reply

> Here is what I recall of my conversation with de Finetti. In 1971 the 3rd International Congress of Logic, Methodology, and Philosophy of Science was held in Bucharest (where I had the dubious pleasure of meeting Caesescu in the receiving line at the Palace of Ministers; what an ugly hard face he had.) De Finetti was there, and I believe that I introduced myself, saying that I had some questions. The only question I recall was why he didn't use the strong version of coherence. He said that he was aware of the option of using it rather than the weak version, but he didn't like the consequence of the substitution: namely that $C(h/e) = 1$ only if e entails h. This consequence is part of my (2') in Sect. 5 of my "Coherence and the Axioms of Confirmation", p. 136 in vol. I of my *Search for a Naturalistic World View*. I don't recall the details of his objection to this principle, but I think he said that it would cause trouble if one had a nondenumerable set of mutually exclusive possible hypothesis, as in the case of probability on continua. I vaguely recall agreeing with him that there would be a problem, because I discuss the problem on pp. 137–140, op. cit. I vaguely recall saying (or maybe just thinking) that epistemic probability doesn't apply well to nondenumerable sets of hypotheses, but thinking that the propensity interpretation of probability, usable in stat. mechanics, could properly deal with nondenumerable sets of possible outcomes. De Finetti surely would not have liked this discrimination, since he believed that the only clear sense of probability was epistemic, and in particular personalist. In my later paper, "Scientific Inference", op. cit. I suggest pragmatically reasonable strategies for dividing the entire set of hypotheses into as denumerable set, or even a finite set, by properly lumping subsets of hypotheses. This strategy seems to me in the right direction, partly because it is part of a program taking the Bayesian formalism as only a framework, which has to be supplemented by pragmatic and by a posteriori considerations.

16-05-02 *King Broccoli* (to J. A. Smolin & C. H. Bennett)

I'll close with this statement. But then after your rebuttal—if you care to make one—we should probably take this offline. I suspect no one cares to explore the issue further (not even you and not even me). But you said something so nicely, I thought I should emphasize it.

Smolinism 2 :

> [CAF wrote:] [F]irst Bob's system has no property, then Alice measures her system, and—Zing!—now Bob's system does have a property. I.e., first it has no quantum state, then it does. How does the system know it ought to have that property if there's no action at a distance? Alternatively, if it doesn't know it, why call it a property of the system?

It always had the property that IF Alice measured one thing, then it would behave as state ϕ. Conditional properties like that do, of course, imply a sort of action at a distance, but what's so bad about that? It's not the sort of violate-the-speed-of-light action at a distance that we should be concerned about. In the end, it means just what you want it to, except for the word property. To use a classical example, suppose I have a box that comes from the factory with either a red ball or a blue ball in it. Surely one can say that a property of the box is that it has either a red ball or a blue ball in it—that's the entire definition of the box. Now if I call up the factory and they tell me what color the ball actually is, never touching the box, does the box change? Was I wrong to call that other thing a property? But everyone understands what's going on. Entanglement doesn't really bring in anything new here. (Alternatively you could formulate it like Charlie sometimes does and say well, it was ALWAYS a red ball, but the measurement result travels backwards though time and fixes things up so it is ok to say "always," but I'm sure you won't care for that).

You said that perfectly, and, of course, I especially liked the concession that this choice of words does entail a kind of uninteresting action-at-a-distance ... but action-at-a-distance nevertheless. Maybe my point is just, how is the non-quantum-practitioner supposed to know where to draw the line between the uninteresting and the interesting versions of the effect? Between the science and the science-fiction? How will he ever be able to shake the nagging feeling that we ought to be able to harness the uninteresting version and turn it into the interesting one? (For that matter, how will the quantum practitioner? Nicolas Gisin comes to mind.)

About the particulars of your box example, I think the common man would be hard pressed to take the textbook definition of a problem—like the one you describe above—as a property possessed by a physical system. How can the box containing the red ball know I've embedded it in a problem where the possible colors are {red, blue}, or instead in a problem where they are {red, blue, green}? People think of physical properties as the things physical systems carry around with them independently of the rest of the world. You may say that this is a limiting conception of the word "property," but I'm pretty sure it's the conception most people use. They would call the set {red, blue} a property of the problem you've defined, not of the system. Within classical physics, they would say the ball has whatever properties it really has (say, red OR blue) ... and it is the physicist's task to figure out which of the two it is.

The standard retort you and Charlie give me is that I am doing nothing more than encumbering the language by saying "a quantum state is ascribed to a system" rather than saying "a quantum state is possessed by a system." But, come on, the word count is the same. It is not that I am encumbering the language; it is just that I am beating on a prejudice you don't want to let go of. Or maybe to be more conciliatory, it is that you cannot imagine that this kind of language could ever be useful, whereas you think there are loads of examples where your own language has led to triumph. But I think Charlie's talk the other day about the public's perception of quantum teleportation as a kind of instantaneous action-at-a-distance (in the science-fiction sense) is a case in point.

Things are only interesting or noninteresting with respect to a context. I think there is a sense in which quantum teleportation is less interesting with respect to the conception that a quantum

state captures a state of knowledge rather than a property possessed. What could be less interesting to say than that, "Quantum teleportation is the transference of one's predictions about one object onto another object that has never interacted with the first"? Maybe it's only this that's keeping you in the old bounds. Is it that if you keep a little bit of the science-fiction imagery alive, it'll help fuel the physics?

"In any case, none of this matters for doing physics," you say. But I think it does in the long run. (Certainly, you've got to concede that there's something that fuels me—and it hurts to think that you might think it is nothing more than irrationality.) When Charlie sent me the picture of a skunk cabbage in reply to these very issues, I found myself thinking of King Broccoli. The story goes that one day, by divine providence, it came to King Broccoli that broccoli, the vegetable, his namesake, actually tastes good. Good in a way that hitherto only gods and angels had known. Every child who had ever said, "Yuck, I don't like broccoli; it tastes awful!" was simply wrong ... or at least that's what the king realized. King Broccoli, being the head of state, decided to do something about it. Henceforward, all gardens in the kingdom should have a patch devoted solely to broccoli. It really wasn't much of a burden on the national product (except, perhaps, for the psychiatrists who had to treat all the movie stars who had never felt fulfilled in their broccoli experience). But think of the diversity of vegetables the kingdom might have raised if its citizens hadn't been encumbered with the king's notion that broccoli had an objective, but never verifiable, taste?

Moral? Maybe there's none. But it is a documented fact that the Kingdom of Broccoli eventually fell and was replaced by a liberal democracy (where the ideals rather than the particulars have an objective status).

Everyone in this mailing list knows by now—though Charlie and John seem to keep forgetting this, or maybe they've never let it sink in—that I think quantum mechanics is just the hint of something much deeper, some fantastic physics yet to come. But I also don't think we will ever stumble upon that physics until we truly get rid of our classical prejudices: seeing quantum states as "properties"—it seems to me—is one of these.

Signing off,

Chris

My own disclaimer: Though I implied above that quantum teleportation becomes less interesting within a subjective conception of the quantum state, I think effects like quantum cryptography become *more* interesting from this view. So there is a tradeoff.

17-05-02 *No Nasty* (to T. Rudolph)

I won't be nasty; I enjoyed your note.

Your thinking has a lot of the flavor of the paper: A. Peres and W. H. Zurek, "Is Quantum Theory Universally Valid?," Am. J. Phys. **50**, 807 (1982). I'm not sure if you're aware of it. Asher once told me that he himself still likes the paper a lot, but Zurek basically disavows it now.

Rudolphism 1 : *what I'm trying to understand is the physics analogue of Turing's construction – what is it that I, a regular physicist, am doing in my interactions with the world and my construction of theories to explain those interactions?*

I like this question a lot, actually. Below is some of my own attempt to ask the same thing. In this regard, I suggest you read Richard Rorty's book, *Philosophy and Social Hope*. I think if I

were to read it again, I would realize how much of an influence it's had on my own thoughts. In fact, my Anti-Växjö paper may be little more than a condensation of it.

I guess if you were to ask me now—in my present state of mind—I would say we are doing more than constructing theories. We are constructing the world (in part). See other note below. But beyond that, I don't know how to say more presently.

Keep thinking about your question! Don't listen to Spekkens.

By the way, I won't be around for essentially a month: Today, I leave for Texas until Thursday. Then the Saturday after that I'm off to Brisbane for a 3 weeks. I return to Bell Labs June 17. (I'm still looking for volunteers on the fence.)

17-05-02 *Dueling Banjos* (to W. K. Wootters)

Thanks for the Hartshorne quote. I had forgotten that I had sort of agreed with that a bit. Here's the way Martin Gardner put it in his essay "Why I Am Not a Solipsist":

> In this book I use the term "realism" in the broad sense of a belief in the reality of something (the nature of which we leave in limbo) that is behind the phaneron, and which generates the phaneron and its weird regularities. This something is independent of human minds in the sense that it existed before there were human minds, and would exist if the human race vanished. I am not here concerned with realism as a view opposed to idealism, or realism in the Platonic sense of a view opposed to nominalism or conceptualism. As I shall use the word it is clear that even Berkeley and Royce were realists. The term of contrast is not "idealism" but "subjectivism."

(The phaneron, by the way, was Peirce's term for "the world of our experience—the totality of all we see, hear, taste, touch, feel, and smell.")

Thus, in making the transition from the first paragraph to the first sentence of the second paragraph in the excerpt from my old letter below, I was making a non sequitur.

Let me ask you this about your "idealistic current." Does it run counter to what my first paragraph below says and what Gardner says above? I guess that's the main point for me.

I suppose if I were to start to label things, then this thing I was telling you about the other day—"the sexual interpretation of quantum mechanics (SIQM)"—would be a kind of dualistic theory. I said it metaphorically this way: When things bang together, something is created that is greater than the sum of the parts. Or again: When things—that's the materialistic aspect—bang together, something is created—that's the mentalistic aspect, for it is like an act of the will or a decision. But that's just a thought that's hitting me at 4:00 in the morning. (So trust it less than even the usual things that come out of my mouth.) I hadn't thought about it in this way before, and I'm not sure I want to continue to thinking about it this way. In general, I don't like dualisms. (Though even saying that is paradoxical; for I think I like "pluralisms" in the sense of James.) The excerpt from an old letter far below gives a slightly longer introduction to the idea of the SIQM.

17-05-02 *Slide Show* (to N. D. Mermin & C. H. Bennett)

Boy you got me into a stink, didn't you, by getting me into that word debate! If you hadn't done that, I might have had some time to answer your other questions this week.

Below for your continued amusement, I'll include a side conversation I had with Charlie. Maybe this debate is at least edifying in some ways. Charlie sees me as hair-splitting; I see Charlie as being half-hearted and inconsistent.

I just read these words in a Martin Gardner article:

> A third aspect of aesthetic theory that bores me even more are all those tiresome disputes, in book after book, about whether aesthetic values are subjective or objective. Here the situation is not quite the same as that of truth. In previous chapters I have argued that the least confusing way to talk about truth is to assume that the world and its structure are not mind-dependent. But beauty, so far as humanity is concerned (we will not consider what beauty may mean to birds or apes, to creatures on other planets, or to gods), obviously requires a human mind. Where is the red of an apple? As I have said, it is in the mind if by red we mean the sensation of red. It is on the apple if by red we mean the structure of a surface that reflects a pattern of visible light which causes a mental sensation of red.
>
> I see no difference between this antique quibble and the question of whether beauty (however defined) is a property of an art object or a sensation in a brain. If by beauty you mean the pleasure aroused by a beautiful object, of course it is subjective. If by beauty you mean the structure of an object capable of arousing aesthetic pleasure, then the beauty is a part of the object. Or you may prefer a third approach and ground beauty in the combined dynamic structure produced by the interaction of an object and a mind. It is all such a weary waste of words. The last approach is the one taken by John Dewey in his influential book Art as Experience. Although I found fault with Dewey's attempt to redefine truth in pragmatic terms, I find his approach to aesthetics (essentially the same, by the way, as Aristotle's) a sensible way of speaking. Again, it is not a question of Dewey being right or wrong. It is a question of the most useful way to talk about aesthetic values.

And I find that I agree with most every word of this (except for the part about the Jamesian-Deweyan theory of truth). But the issue at stake with quantum mechanics goes much deeper than this, and it annoys me to no end that our friend Charlie lumps me in with the art critics.

If you have any words of wisdom that could take a little fire out of our relations, I'd love to hear them. Maybe I'll CC this note to Charlie too.

By the way, this was not intended to be the subject of this note. I wrote instead to tell you that I have now posted the slides from my talk at your party on my website. Maybe you'll enjoy seeing the second half, if not hearing it.

> Charlie,
>
> **Bennettism 13:** *Surely you must have something more sensible to say than some of us who have spoken.*
>
> It would have been more neutral to say, "than those of us who have spoken." If I were trying to read between the lines, I might be tempted to write a note just like this one. But I'll refrain from reading between the lines.
>
> Chris

> Dear Chris,
>
> Sorry. Point well taken. You are a perpetual stimulus to me, if not always in the ways you hope, and I would miss it terribly if you stopped. I should be more grateful. Like the other day, when John was discussing your automotive metaphor, you inspired me

to think that all the cautions you would have us take against quantum misconceptions are—for me, if I did them—like driving a car with the parking brake on all the time.

-CHB

Charlie,

Bennettism 14: *Like the other day, when John was discussing your automotive metaphor, you inspired me to think that all the cautions you would have us take against quantum misconceptions are—for me, if I did them—like driving a car with the parking brake on all the time.*

Then it seems to me you should at least be consistent in your behavior. I interpreted your talk Sunday as a genuine concern for the perceptions the masses hold about some of our favorite quantum effects. Do you have a concern, or do you not? Is it that the world really has some kind of instantaneous action at a distance—like John's note yesterday supported—and we're just not allowed to say the phrase in polite company, or does it not? If the world does not, then so be it. But if it does, why should we try to so hard to delete the phrase from polite conversation? The main point I always wonder is how well you really have these issues worked out in your own head. If you care about misconceptions, then care about them—I say in this slightly grumbling state—and if you don't, then don't. But I have trouble understanding your mix of halfheartisms.

Chris

Dear Chris,

I think the world does not have instantaneous action at a distance, and it's important to find ways of speaking that do not encourage the frequent misconception that entanglement provides a means of faster than light communication. I am less interested, and think you should be less interested, in what seem to me to be hair-splitting arguments about "properties" or "changes in one's state of knowledge" that merely reflect different ways of talking about situations in which we entirely agree about the predictions of outcomes for any experiment. That seems a matter of aesthetics only, and the protections you find so reassuring I find merely annoying, like a parking brake, since they complicate my language and don't prevent me from making any wrong predictions, just what you would call wrong or fuzzy interpretations. I would say that if a way of thinking such as my own does not lead to wrong predictions, then it is not fuzzy in any serious way. Or to put in another way, the distinctions you would have me make because they seem so important to you are in fact less real than the wave function you don't want me to believe in.

Talking with you first thing in the morning is way better than coffee.

-CHB

17-05-02 *More Balking* (to R. Schack)

Schackcosm 68:

[CAF wrote:]

> Rüdiger said, "I am still convinced, despite your severe scolding, that a priori, without looking at the consequences, strong and weak coherence are similarly compelling axioms."

I don't see how you can make this distinction. It seems to me, axioms are only compelling or not compelling insofar as their consequences.

Then why would there be a difference between postulating the Kolmogorov axioms and postulating coherence? Why would we bother with the Dutch book arguments? Why do you bother looking for an information-theoretical reason for the quantum axioms?

It seems to me that the compelling reason to accept the axiom of (ordinary) coherence is that "whatever the consequences are, I certainly do not want to violate coherence, because I don't want to hand over money". It seems to me that a discussion of whether strong coherence is a compelling axiom has to be carried out at this level.

But that is not what is at issue. Strong coherence and regular coherence are *two different theories*, and a theory's worth can only be assessed by looking at the whole thing.

It seems to me a very simple issue. In the theory of strong coherence, there is a kind of normative behavior for all agents that regular coherence does not have. The theory says, in effect, "Thou shalt not tell a lie." If an agent writes down $p = 1$, then in the rigid world of strong coherence, I can trust his statement to be a reflection of his true inner belief. The world of regular coherence does not have that.

You can say that one theory is just more of the same with respect to the old theory. But that is to only look at one aspect of the problem. In another aspect of it, the two theories become qualitatively different.

There is a sense in which my mother-in-law is just a faster version of Kiki at times. That is, she's just more of the same when it comes to cooking, artistry, and a couple of other aspects. But I fell in love with Kiki, not my mother-in-law. This is because when I move past a few isolated features of my mother-in-law, I discover she is qualitatively different from the woman I love.

How can you reject that as a reasonable line of thought?

If you thought at the outset that probabilities could be proper properties of things—like Shimony does—then you might indeed accept strong coherence. For strong coherence grounds the very meaning of $p = 1$. A "$p = 1$"–statement is a TRUE statement. But if you start out from a strongly subjectivist stance on probability, then the only leg you have to stand on is the kind of argument you and Carl give: It is just more of the same of regular coherence; it is just slightly more sensible or cautious betting behavior. But I counter that by saying, carried through consistently and without exception, strong coherence makes it nigh on impossible to embed one's particular bets about particular events in the larger framework of all bets and all events.

It is an issue worthy of debate, I agree, whether my reason is a decent reason to reject strong coherence. (I think it is, clearly.) But it's not fair to say in distinguishing horses and zebras, we should all close our eyes and focus on tactile differences, eschewing all the visual information around us.

17-05-02 *The Divinity that Breathes Life* (to W. K. Wootters)

Wootersism 5: *I know you will say that your realism is not the same as that of classical physics. But in what way, ultimately, is it not the same, other than by the absence of determinism? I think*

indeterminism is crucial, but I think one needs the further step of restoring the subject-object relation as fundamental.

To the extent that I could write something like the passages below, I don't think so. [See note to Renes, dated 20 November 2001.] Or, say, to the extent that I find myself liking this little quote, "I forbade any simulacrum in the temples because the divinity that breathes life into nature cannot be represented," I don't think so. I think there's more than simple indeterminism in my forming view; there is something lower level than determinism and indeterminism both.

But what I am not settled on is whether the "divinity that breathes life" is the subject-object relation. I will think hard about your point, and I will think harder about the impression my writings give.

20-05-02 *Dizzy in Texas* (to J. A. Smolin)

Smolinism 3: *Now you guys are trying too hard not to think like Chris. Nothing is magic about the "instantaneous" first stage of teleportation, only knowledge is being changed. If I have two boxes, one with a ball in it and one without and send one far away, when I open the remaining one I instantly know if the ball is in the other one. Big deal. When I measure something, I find something out. Wow!*

I shouldn't let your note encourage me. (The sober side of myself won't.) But there is a way to make the sum content of all quantum measurements look exactly like your analogy above, even in a formal way. The mathematics may interest you, even if not the philosophy. You can find it in Sections 4.2 and 6.1 of the paper I put on `quant-ph` last week (`quant-ph/0205039`). Or you can see it sketched quickly in the Merminfest talk posted at my website.

The trick is to represent the quantum state as a probability distribution with respect to a fixed, fiducial informationally complete POVM. That is, one imagines a "standard quantum measurement device" sitting in the National Bureau of Standards beside the standard meter and the standard kilogram, and the quantum state captures nothing more than one's judgment for how the device will react if one were to throw one's quantum system into it. With that picture in mind, all a regular, everyday measurement does is update one's judgment concerning the outcomes of the standard measurement. The twist that comes with quantum mechanics (over regular probability theory) is that the update rule is Bayes' rule plus a little more ... it's not just Bayes' rule full stop.

25-05-02 *Fun with Feyerabend* (to M. A. Nielsen)

I'm just putting some notes into my computer before I set off for travel again. At your recommendation, I've paid a little more attention to Paul Feyerabend. The quotations below summarize a little of what I see as likable and usable in his philosophy.

See you tomorrow.

From: P. Feyerabend, *Conquest of Abundance: A Tale of Abstraction versus the Richness of Being*, edited by B. Terpstra (U. Chicago Press, Chicago, 1999).

> *Humans as Sculptors of Reality.* According to the first assumption, our ways of thinking and speaking are products of idiosyncratic historical developments. Common sense and science both conceal this situation. For example, they say (second assumption) that atoms existed long before they were found. This explains why the projection

received a response, but overlooks that vastly different projections did not remain unanswered.

A better way of telling the story is the following. Scientists, being equipped with a complex organism and embedded in constantly changing physical and social surroundings, used ideas and actions (and, much later, equipment up to and including industrial complexes such as CERN) to *manufacture*, first, metaphysical atoms, then, crude physical atoms, and, finally, complex systems of elementary particles out of a material that did not contain these elements but could be shaped into them. Scientists, according to this account, are sculptors of reality—but sculptors in a special sense. They not merely *act causally* upon the world (though they do that, too, and they have to if they want to "discover" new entities); they also *create semantic conditions* engendering strong inferences from known effects to novel projections and, conversely, from the projections to testable effects. We have here the same dichotomy of descriptions which Bohr introduced in his analysis of the case of Einstein, Podolsky and Rosen. Every individual, group, and culture tries to arrive at an equilibrium between the entities it posits and leading beliefs, needs, expectations, and ways of arguing. The separability assumption arises in special cases (traditions, cultures); it is not a condition (to be) satisfied by all, and it certainly is not a sound basis for epistemology. Altogether, the dichotomy subjective/objective and the corresponding dichotomy between descriptions and constructions are much too naive to guide our ideas about the nature and the implications of knowledge claims.

I do not assert that any combined causal-semantic action will lead to a well-articulated and livable world. The material humans (and, for that matter, also dogs and monkeys) face must be approached in the right way. It *offers resistance*; some constructions (some incipient cultures—cargo cults, for example) find no point of attack in it and simply collapse. On the other hand, *this material is more pliable than is commonly assumed*. Molding it in one way (history of technology leading up to a technologically streamlined environment and large research cities such as CERN), we get elementary particles; proceeding in another, we get a nature that is alive and full of Gods. Even the "discovery" of America, which I used to support the separability assumption, allowed some leeway, as is shown by Edmondo O'Gorman's fascinating study, *The Invention of America*. Science certainly is not the only source of reliable ontological information.

It is important to read these statements in the right way. They are not the sketch of a new theory of knowledge which explains the relation between humans and the world and provides a philosophical grounding for whatever discoveries are being made. Taking the historical character of knowledge seriously means rejecting any such attempt. We can describe the results we have obtained (though the description will always be fatally incomplete), we can comment on some similarities and differences that have come to our attention, and we can even try to explain what we found in the course of a particular approach "from the inside," i.e., using the practical and conceptual means provided by the approach (the theory of evolution, evolutionary epistemology, and modern cosmology belong in this category). We can tell many interesting *stories*. We cannot explain, however, how the chosen approach is related to the world and why it is successful, in terms of the world. This would mean knowing the results of all possible approaches or, what amounts to the same, we would know the history of the world before the world has come to an end.

And yet we cannot do without scientific know-how. Our world has been transformed by the material, spiritual, and intellectual impact of science and science-based technolo-

gies. Its reaction to the transformation (and a strange reaction it is!) is that we are stuck in a scientific environment. We need scientists, engineers, scientifically inclined philosophers, sociologists, etc., to deal with the consequences. My point is that these consequences are not grounded in an "objective" nature, but come from a complicated interplay between an unknown and relatively pliable material and researchers who affect and are affected and changed by the material which, after all, is the material from which they have been shaped. It is not therefore easier to remove the results. The "subjective" side of knowledge, being inextricably intertwined with its material manifestations, cannot be just blown away. Far from merely stating what is already there, it created conditions of existence, a world corresponding to these conditions and a life that is adapted to this world; all three now support or "establish" the conjectures that led to them. Still, a look at history shows that this world is not a static world populated by thinking (and publishing) ants who, crawling all over its crevices, gradually discover its features without affecting them in anyway. It is a dynamical and multifaceted Being which influences and reflects the activity of its explorers. It was once full of Gods; it then became a drab material world; and it can be changed again, if its inhabitants have the determination, the intelligence, and the heart to take the necessary steps.

and

> Pauli's views have much in common with the general picture that emerged from Aristotle's principle. In this picture we start with a world (which I shall call the primal world, or Being) which behaves in its own way and not necessarily in accordance with any one of the laws that have been discovered by scientists. (Here we still have an element of realism.) Humans are part of the primal world, not detached aliens, and they are subjected to its whims: Being can send scientists on a wild-goose chase—for centuries. On the other hand, it permits partial independence ... and it provides some of those acting independently (not all of them!) with *manifest worlds* they can expand, explore, and survive in (manifest worlds are in many respects like ecological niches). Inhabitants of a particular manifest world often identify it with Being. They thereby turn local problems into cosmic disasters. But the manifest worlds themselves demonstrate their fragmentary character; they harbor events which should not be there and which are classified away with some embarrassment (example: the separation of the arts and the sciences). The transition from one manifest world to another cannot be described in either except by excising large regions originally thought to be real—a good case for applying the notion of complementarity. Bell's request that a fundamental theory should not contain any reference to observation is satisfied, but trivially so. Being as it is, independently of any kind of approach, can never be known, which means that really fundamental theories don't exist.

25-05-02 *Becoming William James* (to G. L. Comer)

A thought struck me today as I was flying from Newark to LA reading Lloyd Morris's book *William James: The Message of a Modern Mind*. It is this: I think there's nothing I presently want more than to become a modern (quantum) incarnation of William James.

Let me leave you with a lovely quote from the end of Chapter 2.

> He conceived the individual's life, and all social progress, as a form of perpetual experiment. But he did not preach reckless faith. The "will to believe" for which he

argued is best defined as courage weighted with responsibility. Contingency signifies that no precaution can absolutely eliminate all hazard of shipwreck. The individual must take everything into account that may tell against his success, and make every possible provision to minimize disaster in the event of his failure. But having done so, he must act. And in this circumstance, James preached the right of the individual to indulge his personal faith at his personal risk. The part of wisdom would always be to believe what is in the line of one's needs, for only by such belief is the need fulfilled. Over a wide area of existence, possibilities and not finished facts are the realities with which we have actively to deal. So James argued, and pointed out that "as the essence of courage is to stake one's life on a possibility, so the essence of faith is to believe that the possibility exists." But his doctrine subordinated faith to action, for the real utility of faith is to make action genuinely dynamic. "These, then, are my last words to you," he told a group of Harvard students. "Be not afraid of life. Believe that life *is* worth living, and your belief will help create the fact."

Now I'm off to Brisbane. For my own self, I believe it is worth becoming William James.

27-05-02 *Late, but Never Too Late?* (to K. Svozil)

I hope you will excuse my horribly late reply to your letter of 4/19/02. I have just gone through a month of hell of traveling almost constantly, and I have gotten pathetically behind in all my correspondence. (Just check with Johann Summhammer there; he is also in my queue! I'm hoping to get to him later in the day, if not tomorrow.)

Thanks for the continued interest in having me around for the quantum structures meeting. Regrettably, I think I'm going to have to bow out of the possibility of coming. Presently I'm in Australia, separated from my wife and children for a month, and this trip is making me realize that I shouldn't take on any further travel than the stuff I'm already committed to for the summer. It's a shame really, because I am getting ever more involved in IQSA kinds of ideas, and it would be a great opportunity for me to learn a lot more about what is already "out there" mathematically ... just waiting for me to plaster over with some words of Copenhagenish flavor. Beyond that though, I would love to have a chance to express some of my point of view to that audience. I think my talk at the quantum structures session at the AMS meeting in Atlanta two months ago or so went exceedingly well in that regard, and I found myself really enjoying conversations with Dave Foulis, for instance, afterwards.

I finally had a chance to read your paper "What Could Be More Practical than a Good Interpretation?," by the way. There are certainly large parts of it I agree with—I don't know if that will shock you or not. However, there was more to my paper with Peres than just the title! I do get a little shocked when I find people reading the title of the paper as its sum content. Here's the way I put it to Philippe Grangier a few months ago:

> **Grangierisme 6:** *By the way I also disagree with your point of view that "Quantum Theory Needs No 'Interpretation'," Phys. Today* **53***(3), 70 (2000). The fact that a physical theory ALWAYS needs an interpretation is in my opinion a central difference between physics and mathematics.*
>
> You won't find a disagreement with me here. The title and closing sentence of that paper were meant to be tongue-in-cheek plays on something Rudolf Peierls once said:

"The Copenhagen interpretation *is* quantum mechanics." The whole paper is very definitely about an interpretation, and why one does not need to go any further than it to make sense of quantum mechanics as it stands. My paper `quant-ph/0106166` and the large (more personal) collection `quant-ph/105039` is about going the next step, i.e., what to do once we have established the belief that quantum states are states of knowledge.

When we do finally dig up an ontology underneath quantum mechanics, I'm quite sure it will be an interesting one!

And here was the way I put it to Paul Benioff a year before that:

> **Benioffer 1 :** *To me that is an interpretation of QM. Interpretations are what give otherwise empty theories their meaning.*
>
> You're quite right about that. What Asher and I wrote about is indeed a kind of interpretation of the quantum mechanical formalism. The title and the ending words of the article were more for attention-getting than anything else. Also, though, the words were meant to be a small slap in the face to some of the extremes people have gone to (like Everett worlds, Bohm trajectories, and Ghirardi-Rimini-Weber stochastic collapses) just to hold on to a philosophic view that came around long before quantum mechanics was ever heard of. (Talk about people being set in their ways!)

I think the best renditions of my present views can be found in my new papers, `quant-ph/0205039` and `quant-ph/0204146`. I certainly would appreciate any comments you have on those. Especially I would love for you to articulate the weak points you see in the ideas. It is an evolving point of view. And what I want out of the effort is just what you suggest in your paper. I want new calculations, new effects to go search for, new mathematics, and *really* a new world view in total.

Will by chance you be at either the Oviedo, Spain meeting or the QCMC meeting at MIT this summer? Maybe we could talk then?

28-05-02 *Anti-Anti-Växjination* (to J. Summhammer)

Thank you for the beautiful long note of 4/29/02. Please allow to apologize for taking so long to reply. I've been almost completely incapacitated in my email efforts for a few weeks now by travels, family vacations, company duties, etc. Certainly I appreciate the efforts you take to read my things, and definitely your questions and comments are the best products of that!

Let me comment on a couple of your points.

Summhammerism 5 : *But this 'core of the theory' is always tentative. New information, a wider frame of thought, can change it. And yet it is hard to deny that it captures something about that which has been observed. It is like clouds in the sky. For some time they do look like an animal, or the face of a witch, and anyone with eyes will agree, and a few minutes later they are gone. The immutable part here, as well as in physical theories, does seem to be the rules of thought. By the term 'core of the theory' you seem to want to say that they contain a timeless truth. But I have difficulties believing that a physical theory could ever achieve the degree of timeless truth as exhibited, for instance, by mathematical theorems, which are particular expressions of the rules of thought.*

Not quite. I would not say that this thing "the core of a theory" contains a timeless truth. Indeed I tried to be careful to squash that idea when I wrote:

> **Wootersism 6:** *But you obviously also want to say that our theories tell us* something *about reality, even if they are not descriptions of reality.*
>
> I hope you can glean from all the above that I do indeed believe our theories tell us something about reality. But that something is much like what the elephant tells us about reality. Its presence tells us something about the accumulated selective pressures that have arisen up to the present date. A theory to some extent is a statement of history. It is also a statement of our limitations with respect to all the pressures yet seen, or—more carefully—a statement of our limitations with respect to our imaginations for classifying all that we've yet seen. (I for instance, cannot jump off the leaning tower of Pisa unprotected and hope to live; you, for instance, cannot get into your car and hope to push on the accelerator until you are traveling beyond the speed of light.) Finally, to the extent that we the theory users are part of nature, the theory also tells us something about nature in that way.

Thus even the core of the theory is as historical and contingent as the elephant. And just as the elephant could disappear from all historical record, so could the theory and, with it, its core.

Summhammerism 6: *I did not understand what you meant with the last paragraph on page 16 ('However, we would never haven gotten to this stage ....') Sounds as if you could envision that we could set the laws according to our wishes. Reminded me of an old view of evolution: The will (the basic entity) wanted a hand for this particular species, and so it came about ... Sometimes I like this idea, because it permits to look for other patterns and correlations in the history of life than 'mindless' Darwinism.*

Nor did I understand it really. But, yes, I suppose I'm imagining that we might have some control in shaping the "laws" of physics. However—and this is important—that control should be no stronger than the kind of control we might have for shaping a species with genetic engineering. The species has to be able to survive on its own after being produced in a trial. If it can't survive on its own, then one would be loath to call the monstrosity so produced a species to begin with. And so too with what I imagine for this malleable universe.

28-05-02 *Little Toes and a World of Experience* (to W. K. Wootters)

I'm finally down in Australia, with a little time to think. I'll be down here for three weeks. Kiki and the kids are in Munich, visiting Kiki's mom and dad.

If you don't mind, I'd like to ask you a couple of questions about your last letter.

Wootersism 7: *I think the sort of world envisioned by classical physics is in fact impossible. If we really understood what it takes to make a world exist* really*, and not just on paper, I think we would see that one needs the subject-object relation in order to hold things together. This is not to say that one needs dualism. In Whitehead's system, everything is both subject and object, depending on the point of view. There is no dualistic separation into two kinds of entity. But everything is related to something else as subject, and everything is also related to something else as object. In*

classical physics, there is no such relation. As Schrodinger points out, from the very beginning we eliminate the very notion of an experiencing subject. I agree with Schrodinger that this is too extreme an abstraction. In making this abstraction, we have removed something essential from our view of the world.

I suppose I too have a gut feeling that your first sentence above is on the right track, but I wonder if you have an actual argument in mind for supporting the case? Also let me ask you this. Would you say the same thing about a world governed by quantum mechanics? Or do you think the quantum world differs from the classical world in this respect? Maybe, in a refinement of this, let me ask the same thing not about the quantum world in general, but the quantum world in the vision of the many-worlders, say David Deutsch and Charlie Bennett. The way I understand what they envision for quantum mechanics, it has never struck me as so very different from what I would call classical physics. (It is just now that the world as a whole has this thing called a "state" and it is it that goes along according to some mathematically precise law.)

Wootersism 8: *I would like to think that my view avoids the line of reasoning that takes the mathematical description as the essence of the world. As John Wheeler says, a set of mathematical laws will not "fly" by itself. I think the necessary added ingredient is something like* experience. *And that's what I find in Whitehead's view.*

Would you flesh out this thing you call the "subject-object relation" a little more? What do you mean by it? Let us focus on a simpler system than one usually thinks of as a sentient being. Say a rock or the little toe on my right foot. When we think of these systems in their capacities as subjects, rather than objects, what is it that defines those capacities? What are their characteristic traits?

When I think of a person as a subject, I think of him carrying around sets of probability distributions for this and that. That is, I think of a subject as something that can carry beliefs. However, I guess, I have a hard time thinking of a rock or my toe as carrying around beliefs. I also think of a subject as something that can play an active role in shaping other parts of the world because of those beliefs.

However, maybe you mean something completely different when you're thinking of a rock as a subject.

Beyond that, let me ask about this word "experience." What do you mean by that? I hope to try out one of Whitehead's shorter books soon but I haven't gotten to that yet. However, I did read another small book on James on my flight over and it got me into some territory that I haven't yet seen of his. Namely, his stuff from *A Pluralistic Universe* and *Radical Empiricism*, neither of which I've read yet. The book I read was *William James: The Message of a Modern Mind*, by Lloyd Morris. It's only 91 pages, and I found it an engaging little thing (at least for giving me a quick overview of all of James's views, even if not the arguments he used for getting there). Anyway, Chapter 5 was titled "A World of Pure Experience" and what he described there seemed to have some overlap with what you expressed in your last note. Morris wrote:

> "My thesis," James declared, "is that if we start with the supposition that there is only one primal stuff or material in the world, a stuff of which everything is composed, and if we call that stuff 'pure experience,' then knowing can easily be explained as a particular sort of relation towards one another into which portions of pure experience may enter. The relation itself is a part of pure experience; one of its terms becomes the subject or bearer of the knowledge, the 'knower,' the other becomes the object known."

> This doctrine is essentially monistic. But it is radically unlike the monistic doctrines of either idealism or materialism, which respectively affirm that mind and matter are the ultimate substance of reality. Pure experience is neither mind nor matter, but is the ground of both. In itself it is, as James asserted, neutral.

Morris also said that this had some feed-in to Whitehead's later thought, but he didn't elaborate. So I'm guessing there will be some overlap between this and what you're thinking about. But still I'm having trouble understanding what all this might mean, especially since I'm having trouble envisioning the mental life of my toe.

Now the other day I said the idea of a random outcome in a quantum measurement might be viewed as having a mentalistic aspect. From the outside, it looks like a random occurrence; however, from the inside one might think it looks like a "decision." But right now, I'm wondering what even I really mean by this.

Thinking of you from way down under.

28-05-02 *Australiocentrism* (to M. J. Donald)

Thanks a million for the letter of 4/27/02. Please let me apologize for not replying before now. I'm about a month behind in my email due to excessive travels, company business, etc. Just yesterday I arrived in Australia for a three-week stay without the family. So I'm hoping to finally get caught up a little!

Anyway, you flatter me by reading my drivel and taking the time to comment. But I wish you didn't think the universe is doing nothing.

One question:

Donaldism 1: *I'm an idealist because in the course of making that explanation I'm prepared to throw away any ontological presuppositions.*

How do you define idealism? And how would you contrast your flavor of idealism with this little thing Martin Gardner says in his essay, "Why I Am Not a Solipsist":

> In this book I use the term "realism" in the broad sense of a belief in the reality of something (the nature of which we leave in limbo) that is behind the phaneron, and which generates the phaneron and its weird regularities. This something is independent of human minds in the sense that it existed before there were human minds, and would exist if the human race vanished. I am not here concerned with realism as a view opposed to idealism, or realism in the Platonic sense of a view opposed to nominalism or conceptualism. As I shall use the word it is clear that even Berkeley and Royce were realists. The term of contrast is not "idealism" but "subjectivism."

(The phaneron, by the way, was Peirce's term for "the world of our experience—the totality of all we see, hear, taste, touch, feel, and smell.")

And how would you contrast it to this thing Charles Hartshorne says:

> It appears, then, that the idealistic interpretation of reality as essentially relative to or consisting of mind, experience, awareness, that is, psychicalistic idealism, is entirely compatible with a realistic view of the independence of the particular object and the dependence of the particular subject, in each subject-object situation. It may also be urged that we need the word "realism" to refer to the mere thesis that every act of knowledge must be derivative from a known which is not derivative from that act. Thus

the practice of contrasting "idealism" and "realism" as though they were contradictories, is of doubtful convenience. "Realistic idealism," or "realistic subjectivism," has a reasonable and consistent meaning.

29-05-02 *I Think She'll Know* (to N. D. Mermin)

Remember what the dormouse said; feed your head.

You can tell I'm pretty darned behind in my email. I'm in Australia now, finally with a little time to think. Kiki and the kids are in Munich. Caves, Schack, and I are down here for three weeks doing Bayesiany things.

Merminition 86: *I don't see what your teleportation example (pages 11, 12) adds to ordinary EPR. Aren't all the issues exactly the same if Alice "in her laboratory prepares" the single qubit in (1) that she possesses by an appropriate measurement (to be sure, she can't control which outcome she'll get, but that doesn't seem to be central to your point, or is it?) after which she and only she knows what the outcome of the corresponding yes-no measurement on Bob's qubit will be.*

Yeah, I'd agree that it doesn't add a heck of a lot to the old argument. Mostly I wanted to say something about teleportation: Namely that if Einstein had known about it, then he might have used it to the same devious purposes he did with his old argument.

But still, I guess there were a couple of features of this version that I thought made it a bit cleaner than the old Einstein thing. 1) With the supplementation of *only* two bits of physical action on the part of Bob (i.e., one of four possible unitaries), Alice can put Bob's system into any state she wishes. So, in essence (i.e. up to two bits), there's nothing even random and uncontrollable about the process. 2) In the case of teleportation, even examining the measurement device before and after the measurement will tell you nothing about the posterior state for Bob's system that Alice ends up with. Very literally, only Alice ends up doing some updating. If it's she and only she, why not call the state her knowledge?

29-05-02 *That Damned von Neumann* (to N. D. Mermin)

Now let me try to answer more adequately the question you asked after my talk in Ithaca. The point is simply this: Suppose I tell you that I've got a device that measures a standard observable H. How do you know that you should accept my claim? Let's say you do this: You simply give me a supply of a gazillion nonidentical states you've prepared (anyway you wish) in your laboratory. I'll perform my measurement on each of them and report the results I found. If you find that I'm giving you back outcomes with the (conditional) statistics you expect, then you'll have some warrant for believing I'm really performing the measurement I claim.

Now let me ask you this. Suppose you confirm my measurement to your satisfaction by that method. Do you now have warrant to say anything about the post-measurement state for each of the systems you gave me? The answer is "no" of course. The point is, you need to know more about the particulars of the device.

Now, von Neumann said that for an "ideal" device the post-measurement states for the systems will be eigenstates of the observable. But I claim that is an arbitrary notion of ideal, and Kraus's theory of "effects and operations" backs that up in a kind of technical way. The Kraus theory says that the state change can always be thought of as a collapse in the von Neumann sense PLUS a trace-preserving completely positive map.

Now, the standard quip that is made is that the CPM part of this is just extra noise that didn't need to be there. But again, I claim this statement is arbitrary. Here is a simple counter example. Suppose I perform the measurement H on half of an entangled pair. Then (via the entanglement), I can always think of this measurement as *really* a measurement of some sort on the other half. In fact I can think of it as simply the observable that is the transpose of the original one ... only performed on the second system rather than the first. What could be a more minimally disturbing way of measuring H^{T} on the second system than that? But does von Neumann's collapse postulate hold for this kind of measurement? Try it, and you'll find that it doesn't. The only state changes this kind of measurement can produce is of the pure-refinement kind.

Von Neumann brainwashed the generations in a needless way. Even something so simple and "ideal" as a standard photon detector does not follow the postulate. When there's a click, the photons are absorbed from the mode and the field is now in the $|0\rangle$ state.

I hope that helps make up for my lack of lucidity at your celebration.

29-05-02 *More Strict Coherence* (to B. Skyrms)

> Brian said: I think that the usual response to Shimony is just that strict coherence isn't a plausible requirement in the usual way of doing probability theory with continuous mathematics.

That's too bad. What I'm really looking for is anything that expresses a dissatisfaction with strict coherence with respect to discrete event spaces. It turns out that enforcing strict versus normal coherence can make a pretty drastic difference for some interpretive problems in (even finite dimensional) quantum mechanics. Unfortunately, with respect to the attractiveness of the conclusion, my coauthors and I have opposite opinions. Thus, I am inclined to require only normal coherence for the agents in our game; whereas they are inclined to require strict coherence.

The main point, even in the classical case, is that strict coherence requires that an agent ascribe probability 1 to an event if and only if he believes the event is a certainty. Whereas under the assumption of normal de Finettian coherence, a probability-1 assignment cannot be used to conclude a belief of certainty on the agent's part. He might be assigning probability 1, not because it reflects his true beliefs, but because it is advantageous for other purposes. That is, with respect to certainty, strict coherence compels an agent to never "tell a lie."

I think that goes too far as concerns a foundation for "rational behavior." Whereas normal coherence appears to me to strike a sweet spot.

In any case, what I am looking for is some confirmation of my troubles in the published literature ... to help me build a case for the inevitable battles I foresee with my coauthors.

By the way, you might be interested know that we have been putting a substantial effort into interpreting quantum probabilities as personalist probabilities. Let me recommend four of our papers to you (along with the web links to get them) in case you're interested. [`quant-ph/0205039`, `quant-ph/0204146`, `quant-ph/0104088`, `quant-ph/0106133`]

As you climb from bottom to top in this list, you'll find us moving closer and closer to a personalist position. Also, there is a lot of supporting information posted at my website, link below.

31-05-02 *Poor Young Duvenhage* (to H. M. Wiseman)

Thanks yourself for your long note in reply to my long note! You can see I'm working with a much larger lag time than you in my emails. Anyway I'm in Australia now, and slowly working off

the jetlag and becoming more productive as the days go by.

Let me say a few words on your last note.

Wisemanism 4 : *Freedom taken, thank you. And I think you will be stronger if you don't try to prop up your position using another paper which I think goes fundamentally against your views in a number of places.*

If you believe that, you misconstrue my purposes for citing Duvenhage. My citation was to give attention to and encourage this young researcher. Of course, I don't agree with a lot of the paper; but that is beside the point for me. There is one thing I certainly agree with and I think he said it particularly cleanly.

You can find where I said this outright to Duvenhage himself by looking at pages 167 and 168 of my web samizdat, "Quantum States: What the Hell Are They?" (It's in a 28 March 2002 letter; I hope you'll read it.) There you will find me expressing some of the same misgivings as you, though with less detail. As I told him, I would rather encourage the similarities at the moment than the contrasts.

Duvenhage's paper will appear in *Foundations of Physics*, and I think that is a good thing. Having the paper out in publication-land just encourages someone to write a comment on its deficiencies. Indeed, I would love for that to go further and get a discussion stirring. For, what might get more people to pay attention to the potential of a Bayesian approach to QM than a good stirring discussion? Moreover, if people start to pay attention to the potential of the approach, they might just get to work on filling its other (I would say, more real) deficiencies!

So, there. I can even imagine an eloquent writer to start the ball rolling ... (OK, a hint: His initials are HMW.)

Now, let me go back to the one thing I said "I certainly agree with."

Wisemanism 5 : *But here is the difference between classical and quantal.*

In the classical theory the belief p of the weatherman could be given a precise formulation in terms of a set of classical variables (presumably related to his [brain]), and that belief would actually evolve (for a "true h" etc.) in a deterministic way from $p(h)$ to $p(h|d)$ as he looks out the window. That is, the belief of the weatherman can be treated ON THE SAME BASIS as the objects of that belief (i.e. the physical world). That is not to say it MUST be treated on the same basis, but it CAN be, and there is no NEED to have Bayes thm in the foundations of the theory.

In the quantum theory, our belief cannot be treated on the same basis as the physical world. We use a state matrix to encapsulate our belief about the world: our expectation for the results of a fiducial measurement if you like. In the absence of information gathering, this state matrix evolves in some well-defined way. But if we try to treat our belief on the same basis, as a function of physical brain variables, we run into the QMP.

I think this pinpoints in a pretty terse way the root of our disagreement. To say what you said is to 1) accept a kind of reductionism that I no longer think is healthy, and 2) to posit a strong faith that something *can* be done that, in actual fact, has *never* been done.

Concerning 1) I apologize for using the phrase "those beliefs are presumably a property of his head," for it evokes an imagery I would rather not have in this discussion. Perhaps it would have been better for me to use the word "possession" rather than "property."

Here's a belief I presently possess: With probability greater than .9, there will be another suicide bombing in Israel in the next month. Try to put that into physical terms. What can the word "suicide" mean in classical mechanical terms (or even quantum mechanical terms)? Or to

make it look just a little more physical, here is another belief: With probability greater than .99, I will see at least one car today. But why didn't I say a lump of atoms with this characteristic, that characteristic, and the other characteristic? The point is: The world independent of man does not, and cannot, know that that lump of atoms signifies a car. My beliefs—at their starting points—are always about things denoted at such a level of *practical* existence.

Thus I would claim, concerning 2), that it is nothing but a religious faith to suppose that one can derive the form of Bayes' conditionalization rule from a mechanistic physics. It has never been done before, and I would venture to guess that it will never be done.

But still, let me suppose that it could be done after all—it's just that it hasn't been done yet. What would that do? In agreement with (a small part of) Duvenhage's paper, I would say we're left with one conclusion: We should call the difficulties so found the "classical measurement problem." For in classical physics we wouldn't know where the observer begins and the world ends ... just as we don't presently know it with quantum mechanics. Nor as I said above, would we know how to get a Bayesian collapse in the beliefs of the observer (even once he has been identified).

So, with regards to this particular aspect of things, I hold firm with my assessment of Duvenhage. If there is a distinction between a classical and quantum conception of nature, it is somewhere else than in the updating of one's beliefs. I claim the same difficulties are either absent from both conceptions, or present in both conceptions.

But ... maybe I say this all for nothing. For you also wrote this:

Wisemanism 6 : *Duvenhage is quite specific about what he is saying. He says "the Heisenberg cut is therefore no more problematic in quantum mechanics than in classical mechanics". This is true if you are a committed Bayesian, because then the cut is necessarily there from the beginning of your conception of the world.*

and that seems to express that my previous quotation of you was directed more toward Mr. D than me.

In any case,

Wisemanism 7 : *I accept that. This is your solution to the QMP. You say "there is no QMP", but isn't that the same as their saying "I've solved the QMP"? You don't have to answer that.*

I would say it doesn't solve anything; it just shifts the terms. For, where it dismisses old problems, it creates new problems. And it's the problems that keep us all young.

31-05-02 *Reality* (to M. J. Donald)

Thanks for the clarification on your form of idealism. I also had a nice time reading the FAQ on your webpage. I'm whiling away a little time in Australia at the moment, visiting Nielsen and trying to write some papers with Caves and Schack.

Donaldism 2 : *If the "current wavefunction" is just the best description of our knowledge of the system, then what are we made of?*

Something that is not the wavefunction, but for which, once we have accepted its existence, compels us to a structure of reasoning and belief revision whose form is identical to what we once thought of as "quantum mechanics." My best shots so far at saying this in a clear way can be found in Sections 4.2 and 6 of the new paper `quant-ph/0205039`. Or you can see the same thing in pictorial form by looking up the talk "Where's a Good Weatherman When You Need One?" at my webpage.

And so the sparring match goes on ...

01-06-02 *High Dispute* (to P. Grangier)

I hope you will forgive me for not replying to your letters for so long. The volume of email I have been getting lately has started to become something I am not equipped for. Sometimes I just crack under the pressure, and thus my silence. But at the moment, I've got a little time away from home: I am in Australia, visiting Nielsen and trying to write some papers with Caves and Schack. Beyond that, my wife, kids, and Bell Labs have all been left behind! So I am hoping for a productive three weeks.

Grangierisme 7: *If we speak about "objectivity", we have first to agree about what it is. To keep it simple, I stick with the "naive" view that if I do (or if a student does) a measurement in the lab, this is an objective process: this "did happen", and the fact that you don't know the result of the measurement will not change it.*

As far as I can tell, we do NOT disagree on this. So, it kind of annoys me that you keep bringing it up. Instead, I would say our disagreement lies right here: For some reason YOU think it is NECESSARY to uphold the idea that a pure quantum state is an objective property (of something in nature) in order for your sentence above to come about. Whereas I do not.

I know that I can function just fine with subjective quantum states, even with subjective pure quantum states. You will find no logical inconsistency in me; and I doubt I can find any in you. At the level of our squabble, it is to a large extent a matter of taste. However, you and I both know that matters of taste can lead to matters of fact with regards to the questions we will seek of nature. Your taste will lead you one way; my taste will lead me another. Only history will tell which of us will have had the more productive view. Only history will tell which one of us ended up asking the most interesting questions of nature. (Recently I tried to capture this in a little story to Charlie Bennett and John Smolin about the pleasures of broccoli. I will paste the story at the end of this note.)

Here is the way I put the whole point in a message to Matthew Donald just a minute ago. At the present time, I just do not know how to say it any more cleanly than this:

> If the "current wavefunction" is just the best description of our knowledge of the system, then what are we made of?
>
> Something that is not the wavefunction, but for which, once we have accepted its existence, compels us to a structure of reasoning and belief revision whose form is identical to what we once thought of as "quantum mechanics." My best shots so far at saying this in a clear way can be found in Sections 4.2 and 6 of the new paper `quant-ph/0205039`. Or you can see the same thing in pictorial form by looking up the talk "Where's a Good Weatherman When You Need One?" at my webpage.

The sections I recommended to him, in their focus on the Bureau International des Poids et Mesures, have direct implication on you. So I have a secret dream that you will read them ... and finally, finally something will click in your head, and you will say, "You know, Chris is not being so unreasonable after all."

Grangierisme 8: *it is quite enough that a complete (=self-defining) set of physical properties can be predicted with certainty. The crucial point is that there is no "ignorance" left (you must admit that each time you write that a pure state has zero entropy).*

It is fine to note that some observables can be predicted with certainty when a state is a pure state, but that does not delete the fact that there is plenty of ignorance left for other observables. One can even quantify that ignorance nicely. See equations (77) through (82) in my `quant-ph/0205039`. (But the result goes back to Wootters.) The von Neumann entropy—which is what you are thinking of in your statement—simply captures the BEST CASE predictability. So what?

Grangierisme 9 : *PS In your paper you strongly "recommend" the work by Lucien Hardy. I have two comments :*
it surprises me that you adhere with the "relative frequency" approach to probabilities that is used by Lucien (his first axiom). I certainly agree with it, but I thought you would not.
Lucien is trying to make QM look like a new probability theory.

If you look at my paper, you will note that I said:

> Beyond that, let me recommend four other articles. The first two are the most technically important for the enterprise I promote in my other contribution to this volume: Namely, to secure a transfer from our present abstract, axiomatic formulation of quantum mechanics to a more physically meaningful one. I think some elements in Lucien Hardy's papers *almost* carry us to the brink of that. In his work, I think the right emphasis is finally being placed on the right mathematical structures. . . .
>
> I should point out, however, that in all four of the above references, I think significant improvements could be made by adopting a sufficiently Bayesian stance toward the use and meaning of probability.

I said what I meant. (Just as the experimentalist should strive to perform his measurements accurately; the good theorist should strive to read his friends' papers carefully.) The interesting part of Hardy's papers, in my view, is the mathematics. Saying that constitutes an endorsement of neither 1) the relative-frequency interpretation of probability, nor of 2) Hardy's desire to generalize probability theory to a larger structure. Hardy and I, indeed, have had extensive discussions on this.

Grangierisme 10 : *Let us simply admit that local realism is dead, but that physical realism can do quite well without it. Why is that so difficult to accept?*

One more time: I do not deny realism. Moreover, just as you, I am happy with the death of local realism (if what one means by that is "hidden variable realism"). However, I simply do not find your proposed solution to the whole shebang of quantum interpretation problems to be as compelling as you find it.

Measurement clicks alone do not specify post-measurement quantum states. Rather, measurement clicks PLUS prior quantum states (for one's description of the measurement apparatus) do. Thus, where you think a measurement outcome prepares a unique quantum state, I say it has a subjective component. But you say, my measurement device is calibrated. And I say with respect to what? And on and on we could go ad infinitum. The Gordian knot of the state's subjectivity simply cannot be cut by your assuring me that I ought to think otherwise.

Grangierisme 11 : *admit that there is a "reality" attached to the pair of particles, but that there is no "reality" attached to each particle.*

This I see as an arbitrary move. Just as one man's unipartite system is half of another man's bipartite system, one man's bipartite system is half of another man's quadripartite system. And so it goes.

Instead, I would say ALL systems have a reality attached to them. It is simply that that reality has NOTHING TO DO with the quantum states one ascribes to those systems. A system has a Hilbert space, and a Hilbert space has a dimension that does not depend upon which state is "alive" within it. That dimension, I would offer you, can be treated as a reality for a quantum system. But there I stop, whereas you want to go further (by supposing a reality to some nonlocal quantum state).

Grangierisme 12 : *the "reality" attached to the pair makes no problem with Lorentz invariance, because it was created when the two particles interacted, and it simply follows them if they move very far away. The same conclusion apply to more fancy schemes like entanglement swapping, that requires classical communications to effectively prepare the remote entangled state.*

In the usual sense of what one means by the word "interacted," this—I would say—is just wrong. Suppose Alice and Bob possess two particles that interacted in the past so that they are now entangled (by your way of speaking). Further suppose Carol and Ted possess two particles that interacted in the past so that they are entangled. Maybe even make both states—the AB state and the CT state—to be singlets.

Suppose finally that Alice and Bob have never before seen Carol and Ted. A conclusion one can draw from this is that the A and C particles have never interacted in the past. And the B and T particles have never interacted in the past either.

Let now Alice and Carol meet by chance and perform a Bell measurement on their two particles. If Alice shares all her knowledge of the original AB interaction with Carol, and Carol shares all her knowledge of the original CT interaction with Alice, then they will both be warranted to update their assessments of the BT system. They each will immediately ascribe a pure Bell state (one of four possibilities to that system). With RESPECT to Alice and Carol, the BT system will now be in a Bell state, even though B and T have never interacted in the past.

And this has nothing to do with Alice and Carol sending the information about their measurement outcomes to Bob and Ted. I'll say it one last time: WITH RESPECT TO Alice and Carol, the BT system will be in a Bell state after the measurement.

You see, you get hung up because you want to think of entanglement as a real objective property of two systems, not merely a property of an observer's judgment about those systems. Beyond that, your desire for Lorentz invariance makes you want to think of entanglement as only a consequence of local interaction. But that just goes too far, as the above example shows. Entanglement, just like quantum states, can arise from measurements in the distance.

You can call that a "contextually objective" affair if you wish, i.e., that the entanglement between B and T arises only with respect to the context set by A and C. But then I say—as I've said before—why not just call the state ascribed to the BT system Alice and Carol's information about it? Moreover, by calling it information, you will find that you will stop forgetting the prior (subjective) information that was crucial for defining the posteriori states in the first place.

By the way, I have read your FAQ posted on `quant-ph`; I have not ignored it.

Grangierisme 13 : *Bell's inequalities do not hold !!!! ... if a measurement if performed on one side, ABSOLUTELY NOTHING happens on the other side.*

Well, at least we can agree on this. But look how many of your words I had to delete from the paragraph below to get us there:

Grangierisme 14 : *- since there is no "reality" attached to each particle, Bell's inequalities do not hold !!!! All the job is done by the fact that the individual particles have no quantum state, or no other property whatsoever that would decide on the result of the measurement. Then "action at a distance" simply vanishes: if a measurement if performed on one side, ABSOLUTELY NOTHING happens on the other side.*

And we most certainly agree on the following:

Grangierisme 15 : *What next ? QM is a fantastic theory that can only stimulate one question : why is it working so well ? Then we may notice that QM was invented 75 years ago in a somehow anarchic way, as an attempt to understand atomic spectra. But we may speculate the following : QM is actually the answer to a question that was never clearly formulated. We have the answer, what about finding the question ?*

Even if we do not see eye to eye in the secular world, we seem to dream of the same heaven.
PS. Don't forget the story below.

> King Broccoli
>
> "In any case, none of this matters for doing physics," you say. But I think it does in the long run. (Certainly, you've got to concede that there's something that fuels me—and it hurts to think that you might think it is nothing more than irrationality.) When Charlie sent me the picture of a skunk cabbage in reply to these very issues, I found myself thinking of King Broccoli. The story goes that one day, by divine providence, it came to King Broccoli that broccoli, the vegetable, his namesake, actually tastes good. Good in a way that hitherto only gods and angels had known. Every child who had ever said, "Yuck, I don't like broccoli; it tastes awful!" was simply wrong ... or at least that's what the king realized. King Broccoli, being the head of state, decided to do something about it. Henceforward, all gardens in the kingdom should have a patch devoted solely to broccoli. It really wasn't much of a burden on the national product (except, perhaps, for the psychiatrists who had to treat all the movie stars who had never felt fulfilled in their broccoli experience). But think of the diversity of vegetables the kingdom might have raised if its citizens hadn't been encumbered with the king's notion that broccoli had an objective, but never verifiable, taste?
>
> Moral? Maybe there is none. But it is a documented fact that the Kingdom of Broccoli eventually fell and was replaced by a liberal democracy (where the ideals set forth in the constitution, rather than the particulars laws of any given day, have an objective status).

01-06-02 *Postmodernity* (to N. D. Mermin)

Ever since you wrote me this,

Merminition 87 : *I liked the first half (the anti-Final Theory) part of your sermon to Preskill very much. You really could become the darling of the postmodernists if you put your mind to it.*

I've been wondering what the heck a postmodernist really is. Despite my occasional mention of Derrida and Baudrillard and the like, I've never really read any of their essays. The language barrier was always just too big.

In fact, I had never even read Sokal's parody. Well anyway, I've started the process of remedying that now. Yesterday, I finished the book *The Sokal Hoax: The Sham that Shook the Academy*. It is a collection of maybe 50 or 60 articles and news reports that arose in the aftermath of Sokal's paper. Here is the entry I put for Sokal's article in my upcoming Cerro Grande II, the compendium "The Activating Observer: Resource Material for a Paulian–Wheelerish Conception of Nature."

> A. D. Sokal, "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity," Social Text **Spring/Summer 1996**, 217–252 (1996). This parody article contains a wealth of references.

There appear to be some good references there, some of which I have read I *know* that I like!

Anyway, reading the book was an eye-opening experience. Now that I know the perils, it should be interesting to look back 20 years from now and see how successfully or unsuccessfully I managed to navigate the waters.

By the way, I just noticed a mystical coincidence: The big samizdat last year was assigned the number `quant-ph/0105039`; whereas my fire and brimstone "Quantum Mechanics as Quantum Information" from this year was assigned the number `quant-ph/0205039`. Pretty cool.

Did I tell you that I'm in Brisbane now? I've been here for a week, and will be here for two more. Kiki and the kids are in Munich until June 18.

03-06-02 *I Think She'll Know, 2* (to N. D. Mermin)

Merminition 88: *Kurt Gottfried got me back to thinking about the old "derivations of probability" dating back to Hartle in the 1960's and going through the Sidney Coleman application to many worlds. Turns out Jeffrey Goldstone did something on it and there's a nice paper by Gutmann (*`quant-ph/9506016`*). To remind you, in the modern version one's only probabilistic assumption is that if* $\mathrm{tr}(\rho E) = 1$ *then* E *must happen. Combining that with some highbrow analysis of the nonseparable hilbert space formed by infinitely many copies of a system with itself, one derives all the usual probabilistic rules.*

As I remember you were quite scornful of this approach, saying that they were sneaking probability into the story without admitting it. Was this because as a good Bayesian you regard probability 1 as no different from any other probability — merely the current best guess. (Our conversation about Coleman took place before we had our arguments about the difference between probability 1 and "has to happen".) Or did you have some other leakage of probability into the argument in mind?

Yeah, I've still got loads of issues with that approach. But maybe just let me mention the simplest one again: $\mathrm{tr}(\rho E) = 1$ certainly should not be taken to imply "must happen" in an infinite setting. Take the converse. Consider an infinite sequence of coin tosses. Each individual outcome string has probability strictly zero, yet one of them does happen.

Shifting the problem to sets of measure 1 doesn't help either. For lots of inequivalent sets have measure 1. What principle of nature sets one out as important?

It's all ad hockery that these guys are up to ... shined up with some high-powered mathematics so that it looks important.

12-06-02 *Receipt* (to M. J. Donald)

I got your long note; thanks. I'm going to mull over it for a while before replying. But I will reply.

Donaldism 3: *I also haven't commented much on the points I agree with in the paper, although there are a few of these!*

It would be nice to see what these are. In ways, I'm more interested in where we agree than in where we disagree. A good discussion needs some solid ground somewhere.

Beyond that, it would please me to no end to learn that you might have found a thought or two that you could use in the thing. But if you didn't, you didn't. It's a horrible feeling to think I might be writing nothing for nothing.

Each day, I tell my daughters that they can change the world. I tell them that they can change it to the core. But I never tell them that they can believe anything they want. There is a difference. And you don't see it.

The summary at the end of your note troubles me to no end.

Donaldism 4: *Your know-nothing ism, like de Finetti's irrationalism (Gillies, "Philosophical Theories of Probability", page 86), have the dangers of Bohr's writings on which I would agree with Beller (Physics Today, September 1998, pages 29 - 34). In particular, by leaving far too much in vagueness, incoherence, and pious hope, you give the religously-minded the official endorsement of the physics establishment that they may believe anything they want, instead of, by example, instructing them that they can believe anything they want as long as it is rational, coherent, tentative, revisable, and compatible with the evidence (and therefore contrary to naive expectations, because if quantum theory, or indeed science in general, tells us anything it is that the world is not how we would have imagined it before we investigated); and they accept that they may be completely wrong.*

I will certainly return.

24-06-02 *Points* (to H. M. Wiseman)

You see, I'm catching up on my email finally. Thanks for the Nash pointer and the Aussie WWII statistics.

Wisemanism 8: *4. re. the processing power limitations on quantum states. You could have a look at my paper "Bayesian feedback versus Markovian feedback in a two-level atom" (Phys Rev A 2002, or* `quant-ph`*) to see what language I actually did use (I can't remember). But the point is that the state an experimenter computes on the fly to do feedback control is not necessarily less pure than the "true" state which the experimenter does not know. But the experimenter still wants to know how much his state is likely to be "in error" from the "true state". I guess so the experimenter armed with this knowledge would have a better state estimate, by accounting for the error by making the state more mixed. But in any case, how can you call the "true state" a state of belief or whatever if there is no one who believes it? It is what the experimenter's state of knowledge should be, given what they know. That is, it is their state of knowledge, even if they are too dumb to know it. This is another reason you may use to avoid the phrase "state of knowledge", but the challenge is still how to describe this "true state" in that case, without circumlocuting intolerably.*

What you are asking for is the experimenter to compare his pragmatic gambling commitments under the real world constraints he is living under at a given moment, to the beliefs or commitments he would possess under more ideal conditions (for example, if he had limitless computing power). Just as there is no "true" quantum state, independent of the agent—i.e., for two agents, there might be two quantum states—the same may be the case for a single agent. I like your examples because they help draw that out. I can contemplate how I should bet believing what I believe now

and knowing that my feeble mind cannot analyze the full implications of those beliefs, or I can contemplate how I should bet believing what I believe now and imagining a supercomputer that draws out the proper conclusions from those beliefs.

In all cases, whatever the final products of all calculations and all approximations one writes down, those are one's gambling commitments. And *with respect to* that situation, that is one's "quantum state" for a system.

24-06-02 *The World is Under Construction* (to H. M. Wiseman)

Wisemanism 9: *Do you believe that events in the world really are random? Or do you believe they only appear to be random? In the first case, doesn't that mean that you have to believe in objective probabilities? . . .*

Or are you saying that the real world is unanalysable, unthinkable even? Everything we say should be couched in terms of gambling commitments. First, that seems to be a cop-out, giving up on any understanding of the Universe. Second, it can't explain anything in the Darwinian way you mentioned, except Dutch-book consistency. It can't explain why it is "bad" to hold a gambling commitment based on the idea that all world cup soccer balls contain bombs that have a 50% chance to blow up every time a goal is scored . . . You cannot say anything about animals that would have been likely to have gone extinct because of poor (but consistent) gambling commitments, because that is a statement using the concept of objective probabilities. You cannot explain *anything that is not strictly deterministic without using objective probabilities, it seems to me.*

I trust you understand my motives. I wouldn't bother discussing this with you if I didn't think your ideas were potentially revelational. What does not kill you makes you stronger.

For a couple of days I have been thinking about how to reply to the questions in your 6/14/02 email 'reality', but this morning I found myself significantly revising the response I had started to build. In particular, I decided to hardly reply to your questions at all! This may be a little bit annoying to you, but I think it will benefit our longer term discussion.

Of the three options you gave me for answering your questions (I only quoted the last one of the three above), I suppose if I were forced to choose one, I would align myself with the one you called a "cop-out." However, from my point of view, the language you use builds about the ugliest picture it can for where this effort is going. Indeed, you miss the very point, the very beauty, of the "cop-out." So, what I'd like to do is set that right—right here and right now—before we go much further. As advertised, in that way, I will not reply straightforwardly to your questions.

You see, the very starting point for most of my latest thoughts—the thing I think quantum mechanics gives us the deepest and most thorough hint of—is that there is no such thing as THE universe in any completed and waiting-to-be-discovered sense. The thought I am *testing out* is that the universe as a whole is still under construction. And when I say this, I am not thinking of just bits and pieces of it; I am thinking of the whole shebang, all the way to the roots. Nothing is completed. Not just the playhouse Kiki is building for Emma and Katie, or the evolutionary track of the human species, but even the "very laws" of physics. The idea is that they too are building up in precisely the way—and ever in the same danger of falling down as—individual organic species. That is to say, it's Darwinism all the way down.

So when you ask me if I am "saying that the real world is unanalysable, unthinkable even," the answer in a way is "yes." For it is blatantly impossible to analyze to the last detail the characteristics of a world that has not even been dreamt up (even in its own mind's eye).

But how can I impress this upon you, or even make it seem reasonable as a direction for research? That is a tough call. For, like with beer or single-malt Scotch, it is surely an acquired

taste that builds only slowly and with the right company. Of course, I could just send you back to my paper `quant-ph/0204146` and ask you to take it very seriously. But this morning it dawned on me to maybe spend a little time with my scanner to try to IV some thoughts straight into your bloodstream.

At the moment, I can think of no better introductions to the line of thought I'd like to expose you to than three articles by Richard Rorty: "A World without Substances or Essences," "Truth without Correspondence to Reality," and "Thomas Kuhn, Rocks, and the Laws of Physics."[16] (Read them in that order, if you read them.) All three papers can be found in his collection of essays, *Philosophy and Social Hope*. If you absorb these, I think you'll understand completely what I'm up to, and why I so dislike the negative connotations you associate with the radical-Bayesian way of viewing the quantum state. Of course, it may not turn your head the way it turns mine, but at least you'll know where I'm coming from, and from what pool of enthusiasm I derive my strength to eschew the "golden nuggets" of *mere* quantum cosmology, *mere* Bohmianism, and *mere* "dreams of a final theory." The world as I see it is a much bigger place than those stories can tell. And the interpretational issues at the core of quantum mechanics strike me as our first rigorous indication that there is something more to this idea than simply the hopes and desires of an enthusiast.

For now, let me give you a flavor of the thoughts in these papers, and then leave you on your own in the case that you would like to pursue this further. The following quotes come from "Truth without Correspondence to Reality."

> In this essay I shall focus on Whitman's phrase 'counts ... for her justification and success ... almost entirely upon the future'. As I see it, the link between Whitmanesque Americanism and pragmatist philosophy—both classical and 'neo-'—is a willingness to refer all questions of ultimate justification to the future, to the substance of things hoped for. If there is anything distinctive about pragmatism it is that it substitutes the notion of a better human future for the notions of 'reality', 'reason' and 'nature'. One may say of pragmatism what Novalis said of Romanticism, that it is 'the apotheosis of the future'.
>
> As I read Dewey, what he somewhat awkwardly called 'a new metaphysic of man's relation to nature', was a generalization of the moral of Darwinian biology. The only justification of a mutation, biological or cultural, is its contribution to the existence of a more complex and interesting species somewhere in the future. Justiflcation is always justification from the point of view of the survivors, the victors; there is no point of view more exalted than theirs to assume. This is the truth in the ideas that might makes right and that justice is the interest of the stronger. But these ideas are misleading when they are construed metaphysically, as an assertion that the present status quo, or the victorious side in some current war, stand in some privileged relation to the way things really are. So 'metaphysic' was an unfortunate word to use in describing this generalized Darwinism which is democracy. For that word is associated with an attempt to replace appearance by reality.
>
> Pragmatists—both classical and 'neo-'—do not believe that there is a way things really are. So they want to replace the appearance-reality distinction by that between

[16] WARNING: Just because I say I can think of no better introductions to these ideas, it does not mean I endorse every statement in these papers; I may not endorse even half of them. However, I think these papers go in the right direction, even if they go too far ... and even if their arguments are far too weak. But I choose the papers I do because they are easy reading, with beautiful writing, and I suspect these thoughts are so foreign to you that if you can find any sense in *some of them*, then it may be a good start for a dialogue. Moreover, I continue to stress that the best justification yet to pursue this direction of thought—and this is something Rorty does not know—is quantum mechanics itself. So, rather than being the final words on things, these are just the beginning words on things.

descriptions of the world and of ourselves which are less useful and those which are more useful. When the question 'useful for what?' is pressed, they have nothing to say except 'useful to create a better future'. When they are asked, 'Better by what criterion?', they have no detailed answer, any more than the first mammals could specify in what respects they were better than the dying dinosaurs. Pragmatists can only say something as vague as: Better in the sense of containing more of what we consider good and less of what we consider bad. When asked, 'And what exactly do you consider good?', pragmatists can only say, with Whitman, 'variety and freedom', or, with Dewey, 'growth'. 'Growth itself,' Dewey said, 'is the only moral end.'

They are limited to such fuzzy and unhelpful answers because what they hope is not that the future will conform to a plan, will fulfil an immanent teleology, but rather that the future will astonish and exhilarate. Just as fans of the avant garde go to art galleries wanting to be astonished rather than hoping to have any particular expectation fulfilled, so the finite and anthropomorphic deity celebrated by James, and later by A. N. Whitehead and Charles Hartshorne, hopes to be surprised and delighted by the latest product of evolution, both biological and cultural. Asking for pragmatism's blueprint of the future is like asking Whitman to sketch what lies at the end of that illimitable democratic vista. The vista, not the endpoint, matters.

So if Whitman and Dewey have anything interesting in common, it is their principled and deliberate fuzziness. For principled fuzziness is the American way of doing what Heidegger called 'getting beyond metaphysics'. As Heidegger uses it, 'metaphysics' is the search for something clear and distinct, something fully present. That means something that does not trail off into an indefinite future ...

and

So far I have been trying to give an overview of Dewey's place in the intellectual scheme of things by saying something about his relation to Emerson, Whitman, Kant, Hegel and Marx. Now I want to become a bit more technical, and to offer an interpretation of the most famous pragmatist doctrine—the pragmatist theory of truth. I want to show how this doctrine fits into a more general programme: that of replacing Greek and Kantian dualisms between permanent structure and transitory content with the distinction between the past and the future. I shall try to show how the things which James and Dewey said about truth were a way of replacing the task of justifying past custom and tradition by reference to unchanging structure with the task of replacing an unsatisfactory present with a more satisfactory future, thus replacing certainty with hope. This replacement would, they thought, amount to Americanizing philosophy. For they agreed with Whitman that America is the country which counts for its 'reason and justification' upon the future, and *only* upon the future.

Truth is what is supposed to distinguish knowledge from well-grounded opinion—from justified belief. But if the true is, as James said, 'the name of whatever proves itself to be good in the way of belief, and good, too, for definite, assignable, reasons', then it is not clear in what respects a true belief is supposed to differ from one which is merely justified. So pragmatists are often said to confuse truth, which is absolute and eternal, with justification, which is transitory because relative to an audience.

Pragmatists have responded to this criticism in two principal ways. Some, like Peirce, James and Putnam, have said that we can retain an absolute sense of 'true' by identifying it with 'justification in the ideal situation'—the situation which Peirce called

'the end of inquiry'. Others, like Dewey (and, I have argued, Davidson), have suggested that there is little to be said about truth, and that philosophers should explicitly and self-consciously *confine* themselves to justification, to what Dewey called 'warranted assertibility'.

I prefer the latter strategy. Despite the efforts of Putnam and Habermas to clarify the notion of 'ideal epistemic situation', that notion seems to me no more useful than that of 'correspondence to reality', or any of the other notions which philosophers have used to provide an interesting gloss on the word 'true'. Furthermore, I think that any 'absoluteness' which is supposedly ensured by appeal to such notions is equally well ensured if, with Davidson, we insist that human belief cannot swing free of the nonhuman environment and that, as Davidson insists, most of our beliefs (most of *anybody's* beliefs) must be true. For this insistence gives us everything we wanted to get from 'realism' without invoking the slogan that 'the real and the true are "independent of our beliefs" '—a slogan which, Davidson rightly says, it is futile either to accept or to reject.

Davidson's claim that a truth theory for a natural language is nothing more or less than an empirical explanation of the causal relations which hold between features of the environment and the holding true of sentences, seems to me all the guarantee we need that we are, always and everywhere, 'in touch with the world'. If we have such a guarantee, then we have all the insurance we need against 'relativism' and 'arbitrariness'. For Davidson tells us that we can never be more arbitrary than the world lets us be. So even if there is no Way the World Is, even if there is no such thing as 'the intrinsic nature of reality', there are still causal pressures. These pressures will be described in different ways at different times and for different purposes, but they are pressures none the less.

The claim that 'pragmatism is unable to account for the absoluteness of truth' confuses two demands: the demand that we explain the relation between the world and our claims to have true beliefs and the specifically epistemological demand either for present certainty or for a path guaranteed to lead to certainty, if only in the infinitely distant future. The first demand is traditionally met by saying that our beliefs are made true by the world, and that they correspond to the way things are. Davidson denies both claims. He and Dewey agree that we should give up the idea that knowledge is an attempt to *represent* reality. Rather, we should view inquiry as a way of using reality. So the relation between our truth claims and the rest of the world is causal rather than representational. It causes us to hold beliefs, and we continue to hold the beliefs which prove to be reliable guides to getting what we want. Goodman is right to say that there is no one Way the World Is, and so no one way it is to be accurately represented. But there are lots of ways to act so as to realize human hopes of happiness. The attainment of such happiness is not something distinct from the attainment of justified belief; rather, the latter is a special case of the former.

Pragmatists realize that this way of thinking about knowledge and truth makes certainty unlikely. But they think that the quest for certainty—even as a long-term goal—is an attempt to escape from the world. So they interpret the usual hostile reactions to their treatment of truth as an expression of resentment, resentment at being deprived of something which earlier philosophers had mistakenly promised. Dewey urges that the quest for certainty be replaced with the demand for imagination—that philosophy should stop trying to provide reassurance and instead encourage what Emerson called 'self-reliance'. To encourage self-reliance, in this sense, is to encourage the willingness

to turn one's back both on the past and on the attempt of 'the classical philosophy of Europe' to ground the past in the eternal. It is to attempt Emersonian self-creation on a communal scale. To say that one should replace knowledge by hope is to say much the same thing: that one should stop worrying about whether what one believes is well grounded and start worrying about whether one has been imaginative enough to think up interesting alternatives to one's present beliefs. As West says, 'For Emerson, the goal of activity is not simply domination, but also provocation; the telos of movement and flux is not solely mastery, but also stimulation.'

and

It may seem strange to say that there is no connection between justification and truth. This is because we are inclined to say that truth is the aim of inquiry. But I think we pragmatists must grasp the nettle and say that this claim is either empty or false. Inquiry and justification have lots of mutual aims, but they do not have an overarching aim called truth. Inquiry and justification are activities we language-users cannot help engaging in; we do not need a goal called 'truth' to help us do so, any more than our digestive organs need a goal called health to set them to work. Language-users can no more help justifying their beliefs and desires to one another than stomachs can help grinding up foodstuff. The agenda for our digestive organs is set by the particular foodstuffs being processed, and the agenda for our justifying activity is provided by the diverse beliefs and desires we encounter in our fellow language-users. There would only be a 'higher' aim of inquiry called 'truth' if there were such a thing as *ultimate* justification—justification before God, or before the tribunal of reason, as opposed to any merely finite human audience.

But, given a Darwinian picture of the world, there can be no such tribunal. For such a tribunal would have to envisage all the alternatives to a given belief, and know everything that was relevant to criticism of every such alternative. Such a tribunal would have to have what Putnam calls a 'God's eye view'—a view which took in not only every feature of the world as described in a given set of terms, but that feature under every other possible description as well. For if it did not, there would remain the possibility that it was as fallible as the tribunal which sat in judgment on Galileo, a tribunal which we condemn for having required justification of new beliefs in old terms. If Darwin is right, we can no more make sense of the idea of such a tribunal than we can make sense of the idea that biological evolution has an aim. Biological evolution produces ever new species, and cultural evolution produces ever new audiences, but there is no such thing as the species which evolution has in view, nor any such thing as the 'aim of inquiry'.

To sum up, my reply to the claim that pragmatists confuse truth and justification is to turn this charge against those who make it. They are the ones who are confused, because they think of truth as something towards which we are moving, something we get closer to the more justification we have. By contrast, pragmatists think that there are a lot of detailed things to be said about justification to any given audience, but nothing to be said about justification in general. That is why there is nothing general to be said about the nature or limits of human knowledge, nor anything to be said about a connection between justification and truth. There is nothing to be said on the latter subject not because truth is atemporal and justification temporal, but because *the* **only** *point in contrasting the true with the merely justified is to contrast a possible future with the actual present.*

I don't have to tell you that I find these ideas tremendously exciting. It is not that nature is hidden from us. It is that it is not all there yet and never will be; 'nature' is being hammered out as we speak. And just like with a good democracy, we all have a nonnegligible input into giving it shape. That is the idea I am *testing* for consistency and utility. On the chance that it will lead somewhere, it seems to me, worth the gamble.

26-06-02 *The One Boolean Algebra Approach* (to I. Pitowsky)

Let me make one further comment. I too used to think that the PBA approach was the way to go if one wanted to build up a theory along quantum logical lines. But now, I'm not so convinced of it. That is because I am starting to think that quantum mechanics is more analogous to the epistemological theory Richard Jeffrey calls "radical probabilism" than anything else. From that view, there are "probabilities all the way down" with one never getting hold of the truth values of *any* propositions. Rüdiger Schack and I just discovered a wealth of material on Jeffrey's webpage `http://www.princeton.edu/~bayesway/`.

In any case, I think what this leads to is that we ought to be focusing much more on characterizing quantum mechanics solely in terms of the "logic" of POVMs than anything else—these being the structures analogous to what crops up in Jeffrey's "probability kinematics." Thus, if one is looking to characterize PBAs, the best task might be to focus on the kinds of PBAs that POVMs generate, rather than the ones of Kochen and Specker based solely on standard measurements. (This may or may not have some connection to what people are calling "effect algebras" but I don't know.)

Beyond that, I am now of the mind that all one really ever needs for understanding quantum mechanics is a *single* Boolean algebra that is kept safely in the background (solely) for reference. The rest of the theory (and indeed all real-world measurement) is about probability kinematics with respect to that algebra. See Sections 4.2, 6, and 6.1 of my paper `quant-ph/0205039` for details.

27-06-02 *Compatibility Never Ends* (to D. Poulin)

I apologize for taking so long to get back to you. My email box has just been running over lately, and on top of that I've been traveling a lot (just back from Australia, actually).

Poulinism 1: *About your approach: as far as I can see, the main difference (aside from the "vocabulary") between your derivation of the BFM compatibility criterion and their's is that you are aware that it follows from* strong consistency*.*

No, I think the difference between us and them runs deeper than that. Let me try to express the point in a way that maybe I haven't used before. Concerning the BFM criterion: When BFM call it "necessary and sufficient" and when CFS call it "necessary and sufficient," we mean two very different things. For BFM the "necessary" part is enforced by the supposed existence of a super-observer, Zeno. That is, they mean that their criterion is "necessary" because they suppose a super-observer, Zeno, must exist. (See the final section of their paper.) What we (i.e., CFS) do instead is define a notion of "compatibility" based on the classical notion of "strong consistency" and show that the BFM condition is necessary and sufficient with respect to that. Beyond that, however, we leave the issue open.

That is, with regard to the question, "MUST the quantum states of two observers be compatible?," we make no statement in the paper. That is because, from our view—or at least mine

in particular—there is nothing in nature that enforces that states MUST BE compatible. This is quite important if one wants to get a consistent Bayesian view off the ground. (I try to say all this in a lengthier and maybe more complete way in my samizdat *Quantum States: What the Hell Are They?*, pages 116–118, in a note titled "The Spirit of Gandhi." Actually, I think I say it better there; so have a look at that.)

Now, if there is nothing to enforce compatibility in the BFM sense, what would happen if two observers are incompatible with respect to this criterion? That's a fair and decent question, and I think where all the excitement begins. What the W criterion shows is that the two observers can just shake off their incompatibility and go forward (if they are in a congenial mood). That is, they can come to future agreement, not simply by pooling their prior beliefs, but by making an active intervention upon the system their states are about (i.e., by making a measurement). I view this as an extremely interesting property of the quantum world: it allows possibilities for going forward that the classical world does not.

On the other hand, if two observers are not in a congenial mood (i.e., will not perform a measurement of the W variety), then a "crisis" can ensue. What the full implications of that are, we don't know yet. But it may be even more exciting still.

Poulinism 2 : *Maybe honesty can be related to strong consistency?*

Yes, you are right, and this is an important point. I discuss this at length in a note titled "The Commitments" on pages 133–138, and again on pages 192–193 in a note titled "More Balking." The upshot is, yes, strong consistency is deeply tied to honesty. The next question is, must one enforce honesty in a Bayesian approach to things? Your note implies that you tend to think, "yes." The pages I refer you to show that I tend to think, "no."

Poulinism 3 : *Finally, while you relate the BFM condition to your ES, PP, W, and W', it is fundamentally different in spirit. This distinction is related to what we refer to as "type of knowledge". In our vocabulary, the BFM criterion is appropriate to compare knowledge of the quantum while the other are suited for quantum knowledge (just like quantum fidelity is). To me, the most convincing argument is the one of two nonorthogonal pure states: the "measurement criteria" will grant that they are compatible while, as we expressed it "if two observers claim to have complete knowledge of a system, their descriptions had better agree completely." Comparing states of knowledge and measurement outcomes predicted from these states are two different things. We have discussed a similar subtlety by comparing knowledge described by a density matrix and one described by a preparation: these are two different questions which deserve different answers although physically, they cannot be distinguished.*

It's a question of perspective. If one goes the full Bayesian route (which I am starting to do), then it is much better to call a quantum state a "state of belief or judgment," and not a "state of knowledge." For then, one finds that one is never inclined to say "their descriptions had better agree completely." There is nothing to enforce that, except possibly Darwinism. (I'm serious about that statement.) That is to say, when the world rears its head, someone with a firm belief might be wiped out, but in a Bayesian approach (where probabilistic statements always depend upon SUBJECTIVE priors), there is nothing to enforce compatibility beforehand. A misjudgment can only be declared a misjudgment after the fact, not before the fact (which in the quantum setting is created by the process of measurement).

27-06-02 *Probabilism All the Way Up* (to H. M. Wiseman)

Wisemanism 10: *Second, my wife, Nadine, wants to know why Kiki was building a playhouse while you indulge yourself in philosophy.*

I'd like to think we both do what we do best. But I suspect there's no philosopher out there who would say I'm doing good philosophy. (Kiki's artistry on the other hand is always a good hit.)

Wisemanism 11: *Now, more seriously, you say that my language "builds about the ugliest picture it can for where this effort is going". As I keep saying, I mean to be provocative. I hope it drives you to new heights in building a beautiful picture in response. Honestly I do see the beauty in your program. And I think the more extreme it becomes, the more beautiful it becomes. I am very interested to see where it ends up.*

Thanks for the compliment. And, indeed, your correspondence does drive me to new heights (of something). But now I worry that I offended you with my phrase "ugliest picture." It probably came off that way, but it wasn't meant to be an emotional statement or a point about you personally. If some emotion did slip into it, it most likely refers to a conversation I had with Harvey Brown, circa September 11 of last year. Harvey kept saying that I wanted to "doom" nature to being "ineffable." But that language carries such a negative connotation. It carries the idea that there is something there that we can never, or should never, attempt to speak of. So, when you said something similar in print, it gave me the opportunity to try to reply in print. (As you know, I try to have my thoughts recorded so I can refer people to them. One of the original ideas was that it would save me time that way; so far, that aspect of it hasn't worked out.) Anyway, as I made clear, I want to combat that with all my strength. In particular, the way that I am thinking about it, it is not a bad thing that there are some things beyond description in nature. Instead, it is just a statement that there are more things to come; it is a way of leaving room for something new.

Wisemanism 12: *As it happens, I don't have much of a taste for beer or single-malt Scotch. Also as it happens, I was reading a critique of Richard Rorty the very morning before I got your letter. Otherwise I never would have heard of him. It was a 1997 article by Alex Callinicos "Post-modernism: a critical diagnosis". The most interesting criticism in there was to say that Rorty "presumes what he needs to establish, namely that science and philosophy can be assimilated into literature. ... It is ... very hard in practice when trying to explain why one theory can be said to be more useful than another to avoid at least tacitly appealing to the idea that it captures how things are better than its rival does."*

Perhaps this is one aspect of Rorty you disagree with. But I wonder about your saying that quantum mechanics is the best justification for Rorty's philosophy, as if quantum mechanics is something you accept to be real, an "intrinsic nature of reality", the very idea of which Rorty explicitly rejects.

First, just a technical point. The philosophies I am most attracted to at present are those of James and Dewey and what James says about F.C.S. Schiller (but I haven't read Schiller himself yet). Rorty has donned himself to be the spokesman of those guys—and I don't mind that because he writes so nicely—but his writings also have a good admixture of the postmodernist ideas (of Foucault, Derrida, etc.) thrown into them to boot. This business about science not being more trustworthy or real than literary criticism presently strikes me as going too far.

But to Callinicos' point (give me the reference, by the way, and I'll read it)—"It is ... very hard in practice when trying to explain why one theory can be said to be more useful than another

to avoid at least tacitly appealing to the idea that it captures how things are better than its rival does."—I would just reply, "Darwinism." And then, if that didn't sink in, I'd say, "Darwinism." The point is, from this conception, there is very little to say beyond that. Were elephants written into the blueprints of the universe? From the Darwinistic conception, they were not. Yet, the species fills a niche and has had a stability of at least a few million years worth. There is a sense in which an elephant, like a theory, is a "true" component in a description of the world. But that "trueness" only has a finite lifetime, and is largely a result of a conspiracy of things beyond its command (selection pressures). To put it another way, in contrast to Callinicos, the elephant doesn't "capture how things are better than its rival does" in any absolute sense—only in a transitory sense—but that doesn't take away from the functional value of the elephant today. So too, I am trying to imagine with theories.

Henry Folse, by the way, wrote me that there is something of a tradition with this evolutionary idea (beyond Rorty). So I've got a big reading list ahead of me: He tells me Toulmin, Kuhn, Kitcher, and van Fraassen.

Now, to quantum mechanics. You find something contradictory about my liking both quantum mechanics and Rorty. Here is the way I would put it. Presently at least, I am not inclined to accept quantum mechanics "to be real, an 'intrinsic nature of reality'," except insofar as, or to the extent that, it is a "law of thought," much like simple (Bayesian) probability theory. Instead, I view quantum mechanics to be the first *rigorous* hint we have that there might actually be something to James's vision.

I've already told you the history of this, haven't I? I gave a talk in 1999 at Cambridge on the quantum de Finetti theorem, after which Matthew Donald came up to me and bellowed, "You're an American pragmatist!" I didn't know what that meant really, but I kept the thought in the back of my head; I figured one day, I'd figure out what he meant. As it goes, that happened on July 21 of last year. I came across this book of Martin Gardner's of which one of the chapters was titled, "Why I Am Not a Pragmatist." (Part of the story is recorded on page 15 of my little samizdat in a note titled "The Reality of Wives." You might read it for a little laugh.) As I read it, it was like a flash of enlightenment. For every reason Gardner gave for not being a pragmatist, I thought about quantum mechanics and realized that indeed I was one. Donald was right after all; I am an American pragmatist. And my further study of pragmatism has borne that out to a T.

My point of departure, unlike James's, was not abstract philosophy. It was simply trying to make sense of quantum mechanics, where I think the most reasonable and simplest conclusion one can draw from the Kochen-Specker results and the Bell inequality violations is, as Asher Peres says, "unperformed measurements have no outcomes." The measurement provokes the "truth value" into existence; it doesn't exist beforehand. Now, go off and read about James's and Dewey's theory of truth and you'll find almost exactly the same idea (just without the rigor of quantum mechanics). And similarly with lots of other pieces of the philosophy.

So, I view quantum mechanics as the hint of something much deeper. But the full story is not yet told. That is, quantum mechanics strikes me as being to our community what the Galapagos Islands were to Darwin—just a hint of something bigger.

Wisemanism 13: *You and Rorty I guess would agree that "dreams of a final theory" will never be more than dreams. I guess that idea does not worry me as much as it would some physicists, but it does seem like a defeat. But perhaps that just says something of my personality. How much of a role does personality play in one's preferred philosophy?*

Your question is a good one, and one I worry about a lot. Where your knee-jerk reaction is defeat, mine is one of unlimited possibilities and newfound freedom. On a similar issue, James put it like this:

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

But I think the disparity between our views is in better shape than that. I think you're only seeing the program "physics is the ability to win a bet" as a defeat because—even if you don't know it—you're working within a kind of Kantian mindset. That the universe is already formed and there; that there is an "a priori." Anything that can't be said about the universe is then most surely a loss or limitation. But, I think once you see that what the pragmatist is trying to get at is not that, maybe your heart will change. Physics as the ability to win a bet will strike you as something immensely positive. Physics is like that because reality is still forming, and the Darwinistic component (along with the "non-detachedness" of the observer in quantum mechanics) indicates that it may be somewhat malleable. From that point of view, to have "dreams of a final theory" is almost like admitting defeat.

But given what you've said, maybe you're already starting to feel some of this. And that tickles me immensely.

Wisemanism 14: *To conclude, I can (or rather could) accept a lot, or even all, of what you and Rorty are exploring. But I am still not emotionally or intellectually compelled to do so. And I am really not sure whether I want to be compelled in one direction, or whether I want to be able to contain conflicting philosophies. I have this idea that there is an incoherence at the heart of things. Irreconcilable levels of description. Profound truths being the opposite of other profound truths. Incompleteness theorems. That sort of stuff.*

Understood. I know that there's nothing worse than an evangelist knocking at your door on a Saturday morning. Feel free to not reply to this note at all. In the mean time, I'll try to do my best to do what I really ought to be doing: proving theorems, simplifying the quantum axioms, trying to find real-world physics problems for which this view is the most powerful way to tackle it, etc., etc. One thing physicists never deny is a better method for solving a problem.

28-06-02 *To Believe to Know* (to D. Poulin)

Poulinism 4: *Otherwise, you get incompatible statements with which Bayesian theory cannot deal (like computing $p(x|y)$ when y is assigned probability 0).*

It's starting to sound like we mean two different things by Bayesian theory. See, `http://www.princeton.edu/~bayesway/KC.tex.pdf`, slide #12 (on page 13 actually). More seriously, see: `http://cepa.newschool.edu/het/essays/uncert/subjective.htm`, especially the parts on Ramsey and de Finetti.

Poulinism 5: *I think that most of the discussion is based around this distinction: "state of knowledge" and "state of belief". I would say that states of knowledge must be (BFM) compatible while states of belief can be incompatible. How are these two things defined? Well I would say that a state of knowledge is built with the help of an initial state on which everybody agrees, the postulates of quantum mechanics, and "public actions".*

I will agree to your definition of "state of knowledge." But, backtracking from that, an initial state upon which everyone agrees? If one is taking a subjectivist approach (or what I had been calling a "Bayesian approach") to interpreting the quantum state, there is nothing in nature to enforce an initial prior agreement. God does not come down from on high and say to all the agents (i.e., all the observers), "Your starting point shall be the quantum state Ψ." Everyone is left to fend for himself.

That is to say, in the language of the second webpage I sent you to above, in a world where the quantum state is not presupposed to be objective—as had been Mermin's goal when he started this exercise—there is nothing to enforce the "Harsanyi doctrine."

28-06-02 *The Harsanyi Doctrine* (to C. M. Caves & R. Schack)

I just happened to run across the following discussion:

> Finally we should mention that one aspect of Keynes's (1921) propositions has re-emerged in modern economics via the so-called "Harsanyi Doctrine"—also known as the "common prior" assumption (e.g. Harsanyi, 1968). Effectively, this states that if agents all have the same knowledge, then they ought to have the same subjective probability assignments. This assertion, of course, is nowhere implied in subjective probability theory of either the Ramsey-de Finetti or intuitionist camps. The Harsanyi doctrine is largely an outcome of information theory and lies in the background of rational expectations theory—both of which have a rather ambiguous relationship with uncertainty theory anyway. For obvious reasons, information theory cannot embrace subjective probability too closely: its entire purpose is, after all, to set out [an] objective, deterministic relationship between "information" or "knowledge" and agents' choices. This makes it necessary to filter out the personal peculiarities which are permitted in subjective probability theory.

28-06-02 *Who Would Have Thunk?* (to N. D. Mermin)

Who would have guessed that all we were debating in our ill-fated correspondence on the BFM criterion was simply the validity (or maybe the jurisdiction) of the "Harsanyi Doctrine"? Not me! But that's what I found out this morning quite by accident.

If you want to read the story leading up to this, download my mini-samizdat "Quantum States: What the Hell Are They?" and look at the notes to Poulin on pages 215 and 219. I now find this quite interesting actually. Harsanyi won the Nobel prize with Nash, and there seems to be a bit of literature on his doctrine.

By the way, you never responded to (the main content of) my December 10 note "The Spirit of Gandhi." Here's your chance before I close the doors on this chapter of my life. (I.e., I'm going to bind up and close this mini-samizdat by the end of the day. The next one is going to be titled, "Darwinism All the Way Down.")

29-06-02 *Incompatible Beginnings* (to D. Poulin)

Poulinism 6: *Maybe requiring a single description on which everyone agrees is a bit too strong, but something has to be imposed.*

The whole point of the research program I am building up, and the point of most of the 220 pages in *Quantum States: What the Hell Are They?*, and much of the point of my paper `quant-ph/0205039`, is that I don't buy that. Remember the line in the Pink Floyd song, "Mother's going to put all her fears into you."? The point is, none of us are born with "rational" views. We start out in life with whatever our community pumps into us. The mark of "rationality" instead is how we change our views in the light of evidence, and how we gamble based on what we believe. That is what Dutch-book coherence is about, for instance.

So, when I say "agents are left to fend for themselves," I mean it. Every now and then agents start out compatible in one sense or another and then they stay that way, indeed ... perhaps with ever more similarities between their distributions as data flows in. But that is not the norm. Instead most of the time, there is some incompatibility in our world views lurking in the background. We can live with that as long as nothing comes to light to challenge our views. But when the opportunity arises, there will be a "crisis," as Rüdiger Schack likes to say.

At that point, as you say, Bayesian theory can't handle things. But the view I am shooting for is that Darwinism can. That is, if you have a firm belief (i.e., a probability 1 ascription) for something, by Dutch-book rules you are willing to bet your whole bank account on the event. If the event doesn't occur, then you have lost your whole bank account, and in a way your life.

The program, as I see it, is to see how rigorous and fruitful on can make that line of thought.

Chapter

Darwinism All the Way Down (Probabilism All the Way Back Up)

05-07-02 *The Physics of Floyd* (to M. J. Donald)

I said I would be back; I apologize for the delay.

Donaldism 5 : *Your know-nothing ism, like de Finetti's irrationalism (Gillies, "Philosophical Theories of Probability", page 86), have the dangers of Bohr's writings on which I would agree with Beller (Physics Today, September 1998, pages 29 - 34). In particular, by leaving far too much in vagueness, incoherence, and pious hope, you give the religously-minded the official endorsement of the physics establishment that they may believe anything they want, instead of, by example, instructing them that they can believe anything they want as long as it is rational, coherent, tentative, revisable, and compatible with the evidence (and therefore contrary to naive expectations, because if quantum theory, or indeed science in general, tells us anything it is that the world is not how we would have imagined it before we investigated); and they accept that they may be completely wrong.*

In saying this, you demonstrate that you have read Gillies page 86. I'm glad I know that. But, as I tried to convey before, I think you show a deep misconstrual of de Finetti's program (or radical subjectivism, or radical probabilism, or whatever you want to call it, even irrationalism). I never tell my girls that they can believe anything they want. I always try to be as impartial as possible, but in the end, I'm sure all I really teach them is to believe what *I* believe and what the community around me believes. That's where they take their start in life.

And so it is with all of us. Pink Floyd recorded a great lesson with the words, "Mother's going to put all her fears into you." De Finetti's theory of probability is simply a stark recognition of this fact and a stark recognition that there is no getting around it. The point is, none of us are born with "rational" views, and a particular opinion at any one moment is NEITHER rational or irrational. The mark of "rationality" is instead in how we change our views in the light of evidence, and how we gamble based on what we believe. This is what "coherence" and Bayesian updating are about. When Mother has put such a bad opinion into us that rationality cannot realign it enough for us to survive, then we won't survive, and that opinion will not propagate.

It is not "anything goes" here any more than it is for the species in the animal kingdom (Darwin). If you see in my writings a hint that anything goes, then it seems to me you are fighting a battle with someone else and hoping I will fill his shoes. The mathematical side of "radical subjectivism" is an attempt to quantify the ideas above and to show their consistency and utility. It is what the

586 pages of Bernardo and Smith's book "Bayesian Theory" is about; example after example that rationality is not in the prior, but in the conditionalization and in the coherence.

Where you think you need a law-driven reality to explain the success of our scientific activity, I think all I need is (something along the lines of) de Finetti and Darwinism. The most important point of distinction between our views is in how we are willing to mark the "badness" of an opinion/belief/judgment. I say it is only possible to mark a judgment as a misjudgment—in any objective way—after the fact, after the event to which it was directed has either occurred or not occurred. Whereas you want to think there is something that defines the badness or goodness of a judgment before the event.

This is the great gulf between us. I say, "unperformed measurements have no outcomes." "The universe is in the making." You say, if I'd just step back far enough—until I'm outside of the universe, actually—I'd see that the universe "is." It's already there. It's already complete and waiting for me to draw a precise picture of its smooth surface. And in the dimples of that smooth surface, I would see which of my actions are right and which of my actions are wrong long before they take place. Indeed, in that timeless ground state, I would see that everything I'm talking about is folly.

There's no denying that there is something about the former picture that piques the lascivious side of my character. But the problem is deeper and more openly communicable than that. A couple of times in your notes, you have said something to the effect,

Donaldism 6 : *I do hold as an article of faith that there is a true view, although I accept that we may not ever know it, let alone ever know that we know it.*

If we may never know it, or even know that we know it, then I cannot see that it plays any normative role whatever in helping us get along in life. Its explanation of the phenomena about us is little more than a hollow promise. The argument is a minor modification of J. S. Mill's argument against substance:

> If there be such a *substratum*, suppose it at this instant miraculously annihilated, and let the sensations continue to occur in the same order, and how would the *substratum* be missed? By what signs should we be able to discover that its existence had terminated? Should we not have as much reason to believe that it still existed as we now have? And if we should not then be warranted in believing it, how can we be so now?

Just change "substratum" to "immutable law."

There, I've said my piece. And I've probably said too much in too condescending a way: For that I apologize (almost). But I wanted to make a point and make it forcefully. My purpose is not to give fuel to the religious fire-burners. It is to change our conception of how the law-like stability we see around us arises, in a way that does justice to what I view as the great lesson of quantum mechanics. Quantum mechanics, that is, as viewed from inside the world, where we actually live, instead of from some Everettic fantasyland (where 1957 stands out as the most atypical year ever, the year man first saw the universe in its entirety and confirmed what the rationalists had suspected for so long: its image is so bleak as to fit within the mind of a single man). The mark of pious hope is in the belief of miracles: I don't believe in miracles or miraculous years.

Is all that I've said vague? Of course it is. But that is why this is a research program rather than a completed product. There is so immensely much to do. Carl Caves instilled in me long ago that the good physicist is the one who poses a problem he can hope to just solve in the span of his lifetime. That's the way I see this program. With sustained determination, some creativity, and

the help of a lot of friends, I see it as actually going through (and carrying us to a vista that is almost unimaginable now).

Some of the best help we could probably get would be the mind of Matthew Donald. But that might require a midlife crisis! (And I would hate to think of what that might do to your wife and children.)

Now let me address some of your other comments.

Donaldism 7 : *Actually, it was great fun writing: I've always wanted to be allowed to say "Nope", but I've never got away with it before.*

Are you saying that you find it easier to let your hair down around me than with your Cambridgean/Oxfordian contemporaries? If so, then I will reinstate the image of the romantic and mustachioed revolutionary just for you. What the whole movement needs is a little letting down of the hair; glad to be of service.

Donaldism 8 : *page 9: "the final state itself for B cannot be viewed as more than a reflection of some tricky combination of one's initial information and the knowledge gained through the measurement." To what extent then is the "final state for B" actually "for B" and to what extent is it "for" the observer?*

similarly, on page 12, in the last paragraph of section 3, can we say that the system "has" the state $|\psi\rangle$, or should we say that for Alice the system has the state $|\psi\rangle$?

A later point in your note indicates that you know I would say "for the observer" here. Indeed that is better language. Sometimes it's just hard to make the right rhetorical judgment; sorry if I caused you confusion. My intention instead was to put one foot into the enemy camp, and try to fight on their terms. That is to say, my intention was to draw an absurdity from associating a quantum state with a system rather than with an observer's judgment.

Donaldism 9 : *footnote 12: which view gives the most coherent, consistent, plausible explanation of the most phenomena? which view is true?????*

I exasperate you, don't I? The point is, I have my reasons for refraining from that word, and I tried to explain some of them above.

Donaldism 10 : *page 22: I'm not sure what to conclude from section 4.2. This is all that comes to mind:*

Somewhere in the classical division of the BIPM is a die testing machine. It is used to throw a die sufficiently many times to provide a reasonable estimate of the probability of each face. It shares with its quantum analogue the features that in general the probability of any face is never zero, and that many trials are needed to provide useful statistics.

If it were a good classical die testing machine—that is, if it really did the same thing each and every time—then the toss would always come out exactly the same. That is to say, there are no good classical die testing machines.

Donaldism 11 : *page 24: Are POVM's really "the structure of our potential interventions"? All of our potential interventions? Our most important interventions? The structure of a two-year old child's interventions? Or are they just one particular "very carefully contrived way by which we can sometimes manage to see fairly directly into part of [reality's] deep structure".*

The way I would say it now is this. Our interventions are whatever they are. Even a two-year old's. The concept of our interventions having a structure is something we lay on top of them. To that extent, the use of POVMs as a formal descriptive device is a normative one. I would say the same thing for our general use of probability theory. That is to say, we don't use the probability calculus in making all our decisions, but we ought to. Bernardo and Smith put it this way:

> What is the nature and scope of Bayesian Statistics within this spectrum of activity?
> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way.

Similarly I would say of POVMs. If you want to do your best reasoning concerning the consequences of what you've just done (or what you are about to do) then you should use the calculus of quantum mechanics. And that says lay a template down over what you've done (or what you are about to do) that happens to have the shape of a POVM.

Donaldism 12: *page 34: "Quantum measurement is nothing more, and nothing less, than a refinement and a readjustment of one's initial state of belief." But it is something more. It is also the result of a physical process.*

No, I would say, the result of the physical process is something else. It is a creation of sorts. The structure of quantum mechanics, I would say, has something to do with what I can say about that creation ... thus my refinement and readjustment. But concerning the real stuff of the world, the theory hardly goes there. (And that's what I try to say, for better or for worse, in my `quant-ph/0204146`.)

Donaldism 13: *The idea in many-worlds interpretations is to drop von Neumann's process 1 (collapse of the wavefunction). In section 6, it looks as if you're intending to drop process 2 (unitary time propagation). But, the A_{di} in (63), or the Π_d in (65), should be derived from the physics of the measurement; no doubt using environmental decoherence at some point.*

Yes, I do intend to drop process 2 as being something more fundamental than process 1. I keep looking for ways to do that that I find completely convincing. From my view, process 2 is just a special case of process 1, where the POVM is a one-element set. The word "should," you should realize, is little more than a cultural statement ... from a cult that I do not buy into. The phrase "no doubt," on the other hand is a statement of faith. The reasons for your use of that phrase I will leave as an exercise in self-reflection.

Donaldism 14: *Probability theory is only a law of thought in as far as it describes how one should deal with new information. (vN process 1). But physics should also be able to describe how one acquires new information. (vN process 2).*

Ditto.

Donaldism 15 : *It is the physical laws and initial conditions which make an agent's beliefs about the forms and the probabilities of possible future experiences either right or wrong given his past experiences.*

I told you that was the gulf between us.

Donaldism 16 : *The rational assignment of a POVM to a given experimental device is not a matter of free choice.*

I claim it is; at least initially so. The rationality lies purely in coherence and updating, not in particular assignments. But clearly it's going to take a lot of inculcation into my culture to convince you of that. I know it won't help, but I'll throw this out for evidence's sake that I take your skepticism seriously: Just as there is a de Finetti theorem to make sense of "unknown states," there is a de Finetti theorem to make sense of "unknown measurements." (This is a result Schack, Scudo, and I will be posting soon.)

Donaldism 17 : *The strict quantum Bayesian of the Fuchsian persuasion, however, faces the quagmire that physical laws are as subjective as probabilities, so that there is nowhere to start (not even with nothing!).*

Darwinism. Darwinism. Darwinism. Darwinism. It's presently the best nothing I've ever known.

Donaldism 18 : *page 51: Hilbert space dimension, of the small integer type you are referring to, is itself only an effective concept, which depends on not looking too closely at the systems studied. For example, D=2 arises when you consider a photon which may hit one of two detectors, and ignore the full (infinite-dimensional) photon-detector space. Therefore D also is state and agent and context dependent and subjective.*

Absolutely. But it is of the harmless type that fixing a classical phase space is: and that's the point. Any classical (real-world) pendulum has more than one-degree of freedom, but once the context is set, once the approximation is made, the human interests that set that context and made that approximation can be safely forgotten. This is what I envision for Hilbert-space dimension (but not the quantum state).

———-

Let me thank you also for the other uplifting note you sent me. Despite my own combative reactions (as witnessed above), I do find my conversations with you valuable. And in ways, you are partially responsible for the creation of this monster. (In fact, I recently set to paper the story about your involvement. In case you're interested, I'll narrow the search for you: it's somewhere between page 202 and 221 of the mini-samizdat "Quantum States: What the Hell are They?" posted on my webpage. If you wade through that, you'll get the full impact of the story. By the way, I'll be shutting the doors to that samizdat soon. I'll equip it with an index, etc., like I did with the last one, but I won't post it on `quant-ph` this time, only my webpage. After that, I intend to start constructing a new one titled "Darwinism All the Way Down." The present letter to you will be my first entry.)

Finally, let me end with a long quote that I pulled from Richard Jeffrey's paper, "Reading Probabilismo." You inspired me to copy it into my machine.

There is a most instructive contrast between de Finetti's "irrationalist" probabilism and Carnap's rationalist positivism.

For Carnap, probabilism was a fallback position from what one might call dogmatic rationalism, i.e., the view that scientific forecasts and universal hypotheses ought to be logically deducible from *Protokollsätze*. Probability theory was to replace deductive logic; dogmatic rationalism would give way to probabilistic rationalism. Rationalism itself was seen as essential to empiricism. The probability $c(h, e)$ of a scientific forecast or hypothesis h relative to a sentence e that reports the totality of one's empirical evidence needed to be independent of who one might be, provided only that one were ideally rational: qua scientist, an ideally rational being would be individuated only by e, the report of that being's total individual experience to date. If c as well as e could vary from scientist to scientist then $c(h, e)$ would represent a subjective judgment. The only scientific basis on which the $c(h, e)$ values might vary must appear in the second argument-place, not in the function c itself, i.e., such a basis must be empirical. Logical empiricism was wedded to that sort of rationalism. *That* sort of rationalism—not the rationalism of the bogeyman who thinks he can predict the future by pure reason, or can prove that space must be euclidian, a priori.

De Finetti's probabilism was "irrationalist" in denying the possibility of an intelligible split between reason, represented by the conditional probability function c, and experience, represented by the total observation-report e. "Anti-rationalist" would have been a less provocative term. But if de Finetti's probabilism was anti-rationalist it was anti-empiricist as well on the same showing, and "anti-empiricist" has the same ring of madness that "irrationalist" has. The suggestion is that an anti-empiricist opposes observation and experiment, just as an irrationalist is against being reasonable and thinking things through. What's rejected, though, is neither experience nor reason, but the dichotomy underlying logical empiricism, according to which the two can be cleanly separated—say, into e and c. de Finetti rejects as untenable the basic dogma of logical empiricism: the dualism between reason and experience.

05-08-02 *The Spirit that Breathes Life* (to G. L. Comer)

"The divinity that breathes life into nature cannot be represented."

Thanks for your note of August 1. It gave me food for thought; free will saved another life. Indeed, I think the divinity that breathes the whole of life into nature is just that: free will, the essence of making a choice.

I don't think I ever told you this, but 132 years ago, the realization that free will is key saved the life of another great man. It was William James, and the realization is attributed not only to saving his life—he had been in deep depression for some time, with the accounts saying he was on the brink of suicide—but in turning his philosophy and all his thought around. The full road to recovery took several years, but that moment was the starting point.

Let me paste in a passage from his diary for you.

> April 30, 1870
>
> I think that yesterday was a crisis in my life. I finished the first part of Renouvier's second "Essais" and see no reason why his definition of Free Will—"the sustaining of a thought because I choose to when I might have other thoughts"—need be the definition of an illusion. At any rate, I will assume for the present—until next year—that it is no

> illusion. My first act of free will shall be to believe in free will. For the remainder of the year, I will abstain from the mere speculation and contemplative *Grüblei* in which my nature takes most delight, and voluntarily cultivate the feeling of moral freedom, by reading books favorable to it, as well as by acting. After the first of January, my callow skin being somewhat fledged, I may perhaps return to metaphysical study and skepticism without danger to my powers of action. For the present then remember: care little for speculation; much for the form of my action; recollect that only when habits of order are formed can we advance to really interesting fields of action—and consequently accumulate grain on grain of willful choice like a very miser; never forgetting how one link dropped undoes an indefinite number. *Principiis obsta*—Today has furnished the exceptionally passionate initiative which Bain posits as needful for the acquisition of habits. I will see to the sequel. Not in maxims, not in *Anschauungen*, but in accumulated acts of thought lies salvation. *Passer outre.* Hitherto, when I have felt like taking a free initiative, like daring to act originally, without carefully waiting for contemplation of the external world to determine all for me, suicide seemed the most manly form to put my daring into; now, I will go a step further with my will, not only act with it, but believe as well; believe in my individual reality and creative power. My belief, to be sure, can't be optimistic—but I will posit life (the real, the good) in the self-governing resistance of the ego to the world. Life shall [be built in] doing and suffering and creating.

07-08-02 *Trouble Man* (to N. D. Mermin)

Merminition 89 : *I completely forgot about this and have no idea what I was thinking of when I sent you that title. Better count me out. I have too many other things I'm supposed to do in the next two months.*

Is there none of that college spirit left in you? No loyalty to the editorial board (of which you are a member)?

Honestly, is there really nothing you would find worthy to write up? I remember loving the last line in this paragraph from your "teaching" paper:

> There are nevertheless some who believe that all the amplitudes α_x have acquired the status of objective physical quantities, inaccessible though those quantities may be. Such people then wonder how that vast number of high-precision calculations (10^{30} different amplitudes if you have 100 Qbits) could all have been physically implemented. Those who ask such questions like to provide sensational but fundamentally silly answers involving vast numbers of parallel universes, invoking a point of view known as the *many worlds* interpretation of quantum mechanics. My own opinion is that, imaginative as this vision may appear, it is symptomatic of a lack of a much more subtle kind of imagination, which can grasp the exquisite distinction between quantum states and objective physical properties that quantum physics has forced upon us.

Surely, that sentence could be turned into a whole paper, at least under my pen.

I remember the first time I met you—in Montreal—I heard you say, "Quantum computation is the biggest sham in the industry. The government gives you all this money to crack codes, and what you're really doing is quantum foundations." Couldn't you find some way to flesh that out and say it in print? It would benefit you and it would benefit the community.

30-08-02 *Transformations of This and That* (to G. L. Comer)

Thanks for the enjoyable note.

Comerism 2: *Why did this passage interest me? Because of the problem of the dictionary: How can one "understand" language? It seems to me that even when we native English speakers speak English, all we are doing is using rules of translation. Into what? I don't know. Interestingly enough, when I think of relativity, I see a big similarity. For me, relativity is not only about gravity as a manifestation of the curvature of spacetime, but also about how two observers can communicate the results of their experiments in such a way that they can agree that they have witnessed the "same" event. As an example, in order to determine the energy of the cosmic microwave background, one observer with unit four-velocity* u_1^μ *will say the energy is* $u_1^\mu p_\mu$*, where* p_μ *is the momentum of the radiation, but another observer with four-velocity* u_2^μ *will say the energy is* $u_2^\mu p_\mu$*. Even if the observers are at the same place, at the same time, when they make their measurement, they will in general not record the same energy because their motions will in general be different. So how can they ever agree on anything? Relativity tells them how to "translate" one measurement into the other so that a consistent description results. What is objective about the energy? Nothing. It's value depends on the motion of the observer. Where is the objectivity? Only in the rules of translation. The only objective thing to me seems to be the how one object is to be compared with another.*

Did you ever read my "Anti-Vaxjo Interpretation of Quantum Mechanics" paper? (If not, you can download it from my webpage; link below. It should be super-easy reading; no equations at all.) If I'm not mistaken I play up a similar point there, in the context of noncommuting observables.

But I kind of like something about your twist. Spacetime is the dictionary. And, like the dictionary, the "divinity that breathes life" into its connections is its user, the active agent. Without the agent it is a snake that bites its tail.

I'm in Los Alamos at the moment, being forced by the scenery to remember some of my own transformations. It has been two years since I've been here; the first time I've been back since the fire. The mountainside is still full of these toothpicks that used to be trees; I guess it'll be like that for at least 20 more years or so, fading slowly. Yesterday I drove to the street where our house (and all my material possessions) used to be. A new house is finally being constructed in its place. Funny, I found myself thinking mostly about the ground underneath it; I guess like a burial place.

05-09-02 *Everett Quote* (to N. D. Mermin)

I just want to make sure I have this quote archived. An easy way for me to do that is to send it to you. It comes from a footnote in Hugh Everett's original relative-state paper.

> Note added in proof. — In reply to a preprint of this article some correspondents have raised the question of the "transition from possible to actual," arguing that in "reality" there is—as our experience testifies—no such splitting of observer states, so that only one branch can ever actually exist. Since this point may occur to other readers the following is offered in explanation.
>
> The whole issue of the transition from "possible" to "actual" is taken care of in the theory in a very simple way—there is no such transition, nor is such a transition necessary for the theory to be in accord with our experience. From the viewpoint of the theory *all* elements of a superposition (all "branches") are "actual," none any

more "real" than the rest. It is unnecessary to suppose that all but one are somehow destroyed, since all the separate elements of a superposition individually obey the wave equation with complete indifference to the presence or absence ("actuality" or not) of any other elements. This total lack of effect of one branch on another also implies that no observer will ever be aware of any "splitting" process.

19-09-02 *Unsurprising Fact* (to N. D. Mermin)

Merminition 90 : *Did you know that Bob Griffiths believes that within each and every one of his frameworks there is a "history that actually occurs"?*

Yes, I did. He says so quite explicitly in the *Scientific American* article (or was it *Physics Today*?) he wrote with Omnes. I talked with Omnes at length about it at the NATO meeting in Greece you sent me to a couple of years ago, picking particularly on this point.

I'm not sure whether I've told you this in the past, but I am convinced that the consistent historians do nothing beyond what (some interpretations) of Bohr already do. Focus for the moment on a SINGLE standard observable. If one considers that observable in isolation—i.e., without consideration of all other possible observables—then there is absolutely nothing to stop one from acting AS IF one of the values of the observable obtains and all the others don't. Where one runs into trouble (via Kochen-Specker, etc.) is if one tries to hold that view for all observables simultaneously. Now, what do the consistent historians do? Instead of playing the game above for a single standard observable they do it for a single so-called "consistent set of histories." But from my point of view, all a consistent set of histories is, is an arbitrarily singled-out kind of multi-indexed POVM. The point being, woop-ti-do! You give me *any* POVM, and there is nothing to stop me from acting *as if* one of its values obtain and all the others don't. I would only run into trouble (through KS, etc.) if I were to try to play this game for various noncommuting POVMs simultaneously. When Griffiths and company command that one cannot consider distinct sets of consistent histories simultaneously, all they are doing is what I could already have done with *any* POVM. There is nothing deep there.

25-09-02 *Ulfbeck and Bohr* (to N. D. Mermin)

Merminition 91 : *While I don't think they're [Ulfbeck and Bohr] foolish by any means, I was not terribly excited by what they had to say. If you were, by all means invite them.*

I'm finally getting a chance to clean up my mailbox. Sorry for the long absence.

After writing you the note referred to above, I got a chance to actually read the Ulfbeck/Bohr paper. Before that, I had only had discussions with Ulfbeck. Here's what really surprised me. In talking to him, I got the impression that he was giving me something of a précis of their views, and that I would find the heart and the details in their paper. But I didn't! In fact, as far as I could tell, I didn't find anything more in their paper that I didn't already find in our short conversation ... only said six times over in paper form. "The older view did not adequately account for the genuine fortuitousness of the measurement click."

I know I've complained about Father Bohr's lack of detail when asserting the origin of the quantum formalism, but I think they force my complaints to a new level.

There is, however, one idea in the paper that I am inclined to keep or, at least to me, seems worth trying to develop. I say this predominantly because of its William Jamesian feel. Here it is, deleting the words of theirs that I don't like or don't agree with,

> The click with its onset is seen to be an event entirely beyond law. ... [I]t is a unique event that never repeats ... The uniqueness of the click, as an integral part of genuine fortuitousness, refers to the click in its entirety, with all the complexity required for a break-through onto the spacetime scene. ... [T]he very occurrence of laws governing the clicks is contingent on a lowered resolution.

You see, from the Jamesian viewpoint of "radical pluralism," every piece of the universe, every crumb of its existence, is a unique entity unto itself. Here's a little quote in that direction from his essay "Abstractionism and 'Relativismus'":

> Let me give the name of 'vicious abstractionism' to a way of using concepts which may be thus described: We conceive a concrete situation by singling out some salient or important feature in it, and classing it under that; then, instead of adding to its previous characters all the positive consequences which the new way of conceiving it may bring, we proceed to use our concept privatively; reducing the originally rich phenomenon to the naked suggestions of that name abstractly taken, treating it as a case of 'nothing but' that, concept, and acting as if all the other characters from out of which the concept is abstracted were expunged. Abstraction, functioning in this way, becomes a means of arrest far more than a means of advance in thought. It mutilates things; it creates difficulties and finds impossibilities; and more than half the trouble that metaphysicians and logicians give themselves over the paradoxes and dialectic puzzles of the universe may, I am convinced, be traced to this relatively simple source. *The viciously privative employment of abstract characters and class names* is, I am persuaded, one of the great original sins of the rationalistic mind.

I wish I could find a better quote than that—I have memories of reading the idea expressed in much greater detail and so much more eloquently—but this morning, try as I might, I can't find it.

So I'll end this little note with another note I wrote a few months ago—it carries the sentiment, if not the eloquence. [See note to Greg Comer titled "Music in the Musician," dated 23 April 2002.] It's pasted below. Maybe I should have titled the present article, "A Click is but a Click Not: it is so much more." For the same holds with "clicks" as with "atoms."

27-10-02 *A Crowded Placetime* (to G. L. Comer)

Thanks for sending your poem about the solitary place. I especially liked the penultimate paragraph.

> My choices are radioactive remedies
> That rot the roots, spin down the spirals,
> Snip the snares, and dampen the dark.
> Something much larger than the universe,
> With all that space and time and matter.

You make me want to quote William James again. This one comes from a letter in his collection (of letters):

> All I can tell you is the thought that with me outlasts all others, and onto which, like a rock, I find myself washed up when the waves of doubt are weltering over all the rest of the world; and that is the thought of my having a will, and of my belonging to a

brotherhood of men possessed of a capacity for pleasure and pains of different kinds. ... And if we have to give up all hope of seeing into the purposes of God, or to give up theoretically the idea of final causes, ... we can, by our will, make the enjoyment of our brothers stand us in the stead of a final cause ...

28-10-02 *Blather, Lather, and Rinse* (to J. Bub)

The following is a note I started to construct for you several weeks ago. Unfortunately, I never got a chance to finish it, but it seems appropriate to send you the note as it stands nevertheless ... if for nothing else as a starting point for this week's discussions. The note was to be titled "Blather, Lather, and Rinse." I had wanted to polish it better and add some more detail, but maybe this will get the ball rolling.

> Pilate was probably not the first to ask what truth is, and he was by no means the last. Those who ask it seek something deeper than disquotation, which was the valid residue of the correspondence theory of truth.
>
> — W. V. Quine

I apologize for taking so long to reply to your several letters, but I wanted to read Quine's little book *Pursuit of Truth* (revised edition) before doing so.

I want to defend my position that there is some good stuff in Marcus's manuscript. At the very least, there are things that I needed to read about and be exposed to. One man's blather is another man's rinse, I suppose. But I think it is more than that. There were three things that caught my attention.

a) The strategy of taking the Bohmian and Everettista's strong desire to find a "designatable reality term" within quantum theory to take as its anchor and turning that against them. The point he was trying to make could be more polished and intensified, but it was one that I found to be a keeper to be developed.

b) I found the discussion of the "primitive theories" that underlie our developed theories quite well done, and I think it is an important point that has yet to sink in to most of our mentalities. I just loved some of his lines in that section. Here was one that particularly struck me:

> My second point was that the function of a formal physical theory is to extend the primitive theory. And, indeed, modern physical theories extend it to a degree which may seem almost miraculous (which, to a Palaeolithic hunter, and perhaps even to an Elizabethan savant, might really have seemed miraculous). But, however striking the advance, we are never in a position actually to dispense with the primitive theory. No matter how sophisticated the fighter aircraft, the pilot still controls it in the time-honoured Palaeolithic manner, using hands and feet. Similarly here: if you want to confirm the theory, or to apply the theory, then you must have recourse to your Palaeolithic sensory organs, and your Palaeolithic sensory cortex. No matter how marvellous the theory, if you want to relate it to the world it is supposed to concern, you have to go through these Palaeolithic channels. Which means the theory can have no commerce

with the world it is about except through the mediacy—the good offices, as it were—of the primitive theory. There is no escaping it: it's the way we're wired.

I think this is the proper way to understand (or at the very least build upon) what Bohr was talking about when he kept referring to the necessity of a classical description for making sense of quantum phenomena. You can see this mimicked (but in much less developed form) in my Samizdat: p. 260, "Foods for Thought [sic]," p. 464, "Always One Theory Behind," and in footnote 18 of my `quant-ph/0205039`.

In particular I found Marcus's discussion of this point extremely relevant to an ongoing debate I've been having with Mermin, Caves and Schack for some time. From where comes the judgment that a beamsplitter is a beamsplitter (and not some other kind of device)? Those fellows want to think that it somehow—in a way yet to be explained!—comes out of quantum theory itself. But I don't buy it. The path I'm travelling indicates that that judgment will never come out of quantum theory. The judgment that a beamsplitter is a beamsplitter is, in proper Bayesian terms, the assumption of a "prior". And where an ultimate prior comes from, Bayesian theory is silent. One must seek an answer from somewhere else, if from anywhere at all. In fact, I would say this is the ultimate meaning of my slogan "Quantum Mechanics is a Law of Thought" (at those times when I hold to it).

c) Finally, I rather liked the discussion of "reality as a logical requirement" at the very end of his draft. I mimic some loosely related thoughts at the beginning of Section 4 of my `quant-ph/0205039`, and flesh it out a little more fully in Section 5 of my `quant-ph/0204146`. The reason to draw a distinction between the "state space" and the "sample space" is not because the "sample space" captures the idea that there is a representative that is "really there" and we are just ignorant of it, but because it captures the idea of a spur on which we revise our beliefs.

The things in Marcus's draft that seemed to capture your attention were those (small) things that fall within the frame of usual (nonpragmatic) philosophical debate. I'm thinking here of your remark on disquotation. But I think he and I both have bigger bones to pick. The deeper issue is to do away altogether with a correspondence notion of truth, and to put into its place something akin to a pragmatic account (where "truth" is made by the process of "measurement intervention"). The old pragmatists, as I see it, had a glimpse of something that quantum mechanics gives us a much stronger reason to take seriously. [I was going to write much more here, but now no time.]

04-11-02 *Got It!* (to R. Schack, C. M. Caves & N. D. Mermin)

WARNING! WARNING! WARNING! WARNING! WARNING!

Subsections 2 and 3 of the present note are UTTER RUBBISH, and I entirely withdraw the ideas put forth there. (I still agree with subsections 1 and 4, however.) See the notes "Utter Rubbish and Internal Consistency, Parts I and II" of 27 and 28 June 2003 for an explanation. I nevertheless leave the ill-fated sections in this collection because, despite their embarrassment for me, I deem them crucial for understanding the surer path we all explored later.

WARNING! WARNING! WARNING! WARNING! WARNING!

It finally sank in completely and whammed me over the head! It's beautiful; it's compelling; it was implicit the whole time. It's trivial even, and I am so excited! There is absolutely no mystery in the Penrose example, and there is absolutely no mystery in the Einstein-Podolsky-Rosen example once one has accepted that POVM ascriptions have the same non-ontic character as the state vector itself. Since my first note to you on my "Identity Crisis" in August of last year—("Quantum States:

WHAT?", page 35)—the solution has been staring us in the face. In fact, the examples above now appear as hardly worthy of ever having been called conundrums or paradoxes in the first place.

I guess this is what Rüdiger was trying to get across to me (and to David?) on Friday before he left. Let me put the story in my own words.

I'll break the note below into four sections for clarity:

1. A general exegesis on how to think about POVMs
2. The Penrose example
3. The EPR example
4. Forward-looking thoughts on POVMs and a pluralistic universe

1) The POVM as a function from raw data to meaning

We generally write a POVM as an indexed set of operators, E_d. Here is how I would denote the referents of those symbols. The index d should be taken to stand for the raw data that can enter our attention when a quantum measurement is performed. The whole object E_d should be construed as the "meaning" we propose to ascribe to that piece of data when/if it comes to our attention. It is important here to recognize the logical distinction between these two roles. The symbol d stands for something beyond our control, something that enters into us from the world outside our head. The ascription of a particular value d is not up to us, by definition. The *function* E_d, however, is of a completely different flavor. It is set by our history, by our education, by whatever incidental factors that have led us to believe whatever it is that we believe when we walk into the laboratory to elicit some data. That is to say, E_d has much the character of a subjective probability assignment. It is a judgment.

I have tried to say this in various ways before. Maybe the first place in "Quantum States: WHAT?" is in the note "Note on Terminology," pages 49–50, or in more detail in "Replies to a Conglomeration," page 92. Maybe there are still better shots at it, but I didn't look further. (I guess I also give another variation on the matter on page 42 of `quant-ph/0205039`). You can have a look at those if you think it'll help, but I think the paragraph above says it as well as anything. And I know that at least Rüdiger is on board with all this. Carl probably is too, but I'm not as sure.

2) The Penrose example

Here's one of the passages I recorded [from Penrose's book] in the Samizdat, pages 402–404,

> One of the most powerful reasons for rejecting such a subjective viewpoint concerning the reality of $|\psi\rangle$ comes from the fact that whatever $|\psi\rangle$ might be, there is always—in principle, at least—a *primitive measurement* whose **YES** space consists of the Hilbert-space ray determined by $|\psi\rangle$. The point is that the physical state $|\psi\rangle$ (determined by the ray of complex multiples of $|\psi\rangle$) is *uniquely* determined by the fact that the outcome **YES**, for this state, is *certain*. No other physical state has this property. For any other state, there would merely be some probability, short of certainty, that the outcome will be **YES**, and an outcome of **NO** might occur. Thus, although there is no measurement which will tell us what $|\psi\rangle$ actually *is*, the physical state $|\psi\rangle$ is uniquely determined by what it asserts must be the result of a measurement that *might* be performed on it.

Let's think about this from our perspective. From our point of view, both the state $|\psi\rangle$ and the measurement Penrose speaks of are subjective judgments. They both count as priors. In principle, they are distinct subjective judgments, but in this case they happen to coincide in a meaningful sense. Here's the meaningful sense. When accepting quantum mechanics as a theory for reasoning, we are, among other things, accepting the consequences of Gleason's theorem. And with that comes the coordinated states and measurements Penrose speaks of. WHEN the state, THEN the given measurement outcome with probability one.

But what does that mean? It means little more than that, no matter what the objective character of the raw data we find, we *ascribe* to it a meaning appropriately associated with the given state. That is, the **YES** boils down to essentially a convention. The meaning we ascribe to the raw data has no choice but to be labelled **YES**.

And that, I think, is the whole story. I'll add some more metaphysics to this in a minute, but first let me move on to the EPR situation before going further.

3) The EPR example

To illustrate this one, consider a bipartite set of qubits in a maximally entangled state and suppose one measures the same von Neumann measurement on each. In this case, we can't predict the outcome of either measurement, but we can predict that the two will match identically. Is there anything mysterious about this? Nicolas Gisin tells me there is. For he says, "I'll grant you that at least one of the results is a sort of free creation or birth—i.e., it does not arise because of a local hidden-variable theory—but then how does the other creation in the pair know how to go the same way?" "That simply can't happen unless there is some kind of superluminal communication going on or, even more radically, spacetime itself is meaningless."

Well, what do we have here but little more than an extension of the situation above? Now, there are simply three priors rather than two. And those priors command us to interpret the two pieces of raw data (coming from the two separated parts of the experiment) as having the same meaning. That is to say, in analogy to our solution to the Penrose conundrum, it is simply a convention that the clicks are the same in the two wings of the experiment.

And that too is the end of the story. It is just a triviality that the measurements come out perfectly correlated. They came out that way because WE labelled them that way with the choice of our priors.

4) POVMs and radical pluralism

Now let me go into a bit of the metaphysics of this. Here's a point of view that I'm finding myself more and more attracted to lately.

I think it is safe to say that the following idea is pretty commonplace in quantum mechanical practice. Suppose I measure a single POVM twice—maybe on the same system or two different systems, I don't care—and just happen to get the same outcome in both cases. Namely, a single operator E_d. The common idea, and one I've held onto for years, is that there is an objective sense in which those two events are identical copies of each other. They are like identical atoms ... or something like the spacetime equivalent of atoms. But now I think we have no warrant to think that. Rather, I would say the two outcomes are identical only because we have (subjectively) chosen to ignore almost all of their structure.

That is to say, I now count myself not so far from the opinion of Ulfbeck and Bohr, when they write:

> The click ... is seen to be an event entirely beyond law. ... [I]t is a unique event that never repeats ... The uniqueness of the click, as an integral part of genuine fortuitousness, refers to the click in its entirety .... [T]he very occurrence of laws governing the clicks is contingent on a lowered resolution.

For though I have made a logical distinction between the role of the d's and the E_d's above, one should not forget the very theory-ladenness of the set of possible d's. What I think is going on here is that it takes (a lot of) theory to get us to even recognize the raw data, much less ascribe it some meaning. In Marcus Appleby's terms, all that stuff resides in the "primitive theory" (or perhaps some extension of it), which is a level well below quantum mechanics. What quantum mechanics is about is a little froth on the top of a much deeper sea. Once that deeper sea is set, then it makes sense to make a distinction between the inside and the outside of the agent—i.e., the subjective and the objective—as we did above. For even in this froth on the top of a deeper sea, we still find things we cannot control once our basic beliefs—i.e., our theory—are set.

Without the potential d's we could not even speak of the possibility of experiment. Yet like the cardinality of the set of colors in the rainbow—Newton said seven, Aristotle said three or four—a subjective judgment had to be made (within the wide community) before we could get to that level. If this is so, then it should not strike us as so strange that the raw data d in our quantum mechanical experience will ultimately be ascribed with a meaning E_d that is subjectively given. (I expressed some of this a little better in a note I wrote to David last month; I'll place it below as a supplement.) More particularly, with respect to the EPR example above, it should not strike us as odd that the phenomenon comes about solely because of an interpretive convention we set: All quantum measurement outcomes are unique and incomparable at the ontic level. At least that's the idea I'm toying with.

I think that's enough for tonight. I now intend to sleep for at least 14 hours! (It was a long three weeks.)

Mermin's Reply

> Thanks cc'ing me the latest epiphany. I've only had a chance to glance at it and am still torn between whether my response is "that was how I understood your position to be all week" or "what utter rubbish" so I will take a little time before responding. (I have to go to Germany for a week in a week so it may be a couple of weeks.)

Schack's Reply

> I am happy with your section 1) on the POVM as a function from raw data to meaning. But I don't get the point you are making in section 2). I would say that the sentence "a σ_x measurement of a particle in the state $|+\rangle$ gives the outcome YES with certainty" is saying exactly the following: If our states of belief about the POVM and the state are expressed by σ_x and $|+\rangle$, then consistency requires us to assign probability 1 to the outcome YES. It's a statement about consistent application of the formalism. To say about a particular experiment that it performs a σ_x measurement of a particle in the state $|+\rangle$ is a subjective judgment. So far we are in agreement.
>
> But then you write
>
> > But what does that mean? It means little more than that, no matter what the objective character of the raw data we find, we *ascribe* to it a meaning appropriately associated with the given state. That is, the **YES** boils down

> to essentially a convention. The meaning we ascribe to the raw data has no choice but to be labelled **YES**.

I probably just misunderstand this paragraph. It seems to me that once we have ascribed the state (the pair σ_x, $|+\rangle$), we are making a very strong statement. We have identified two possible outcomes, YES and NO, and we make a commitment to accepting bets on YES with arbitrarily unfavorable odds. It is logically possible that the outcome will be NO. If the outcome is NO, there will be a crisis. Ruin. Your paragraph suggests that in this case we just relabel the raw data. If that's what you mean, then I agree with David's "what utter rubbish". But I probably misunderstand you here. (In a year's time, you'll write "I never said it better than in my note headed 'Got It!' from 3 November 2002" :-)

I agree that for the purpose of this discussion, the EPR scenario does not add much. But you get me worried again in section 3):

> That is to say, in analogy to our solution to the Penrose conundrum, it is simply a convention that the clicks are the same in the two wings of the experiment.

Either I don't get it, or it's utter rubbish.

Section 4), I like. It highlights the chasm that exists between our approach and the many-worlders and decoherence people. We all agree that a click is not an elementary phenomenon. They want to reduce it to something more fundamental. We say it's irreducible (which does not mean that we cannot analyze a measurement apparatus or decoherence of a quantum register in as much depth and detail as anybody else). Your metaphysical bit is nice in that it makes clear that "irreducible" is not the same thing as "elementary". As you say it,

> All quantum measurement outcomes are unique and incomparable at the ontic level.

I hope you got your 14 hours of sleep. My best night so far has been 5 hours long.

06-12-02 *Enjoyed* (to D. M. Greenberger)

I enjoyed listening to you talk about an evolutionary approach to quantum mechanics the other day. When we get together next time please tell me more! You've got my ear on this subject.

Below I'll place some notes I've written on things to do with Darwinism and quantum mechanics ... and their connections. The notes come from my samizdat, *Quantum States: What the Hell Are They?* ...

Greenberger's Reply, "Tiny Answer to Large Questions"

Your emails, like your papers, are quite prolix, lots to read (not meant as a criticism, only as an excuse for having not had time to digest it yet), and so I can only tell you where I am at on a few issues. I will have to reread most of the comments in the email and ruminate upon them.

But I strongly believe in Darwinism as the mechanism that drives science. In fact I define science by saying that man is always reacting to the environment presented to him, in order to better adapt to it, and science is his conscious effort to maximize this adaptation. So I strongly believe that we frame the theories that we do because we

perceive the world the way we do. Our senses and conceptualizations are partly built in and are partly a response to what is out there, and we build theories and instruments to help us better interact with it. But if our senses and brain structure were different, we would have evolved a different response.

For example, if like a snake we could sense infrared, our whole world view would be different. The snake sticks out his forked tongue and says, "ah, a mouse passed this way a while ago, and it is hotter to the left than to the right, so I can track him to his lair." The mouse and his infra-red image aren't very separable.

Now we see objects in visible light, of short wavelength, and so we see sharp images. So we develop rules that give things sharp boundaries, and we say two things can't occupy the same place at the same time. This in turn leads us to think that the integers are important, and that enumerating things is important. Our whole counting system and mathematics is based on it. If we perceived things like a snake, nothing would be sharp, and I suspect the integers would be way down on our list of what's important in mathematics. We would think a continuum approach would be much more intuitive.

So you see, I don't even think mathematics is god-given. I think it is part of our response to our environment, based on our senses. And when people tell me, as almost all mathematicians do, that mathematics is objective, and corresponds to a Platonic something "out there", my answer is that "The reason you believe that two plus two equals four, is because those of your ancestors who also believed that, ate the ones who believed that two plus two equals five." There was survival value in it for us as we are. But that might not have been the case if we were different. It's not necessary, it's Darwinism.

And so science evolves in a manner reminiscent of the man who looks for things under the lamppost, on a dark night, because the light there is better. We do the experiments we can do, with the equipment we have, because otherwise we couldn't do anything. But we end up with a very skewed view of the world. One of the things Darwinism gave us was an insatiable curiosity, and so it is good to dream of final theories, but the dream is rationally a silly one. Every time we open a new window on nature we see all sorts of things we never dreamed of. We are under a lamppost somewhere in the middle of nowhere, and most of creation is totally unknown to us. Topics like consciousness and ESP, etc, we ban from science, because we can't begin to get a handle on them. But that doesn't make them unimportant. They are actually the essence of things for us. Our lamppost just doesn't throw any light in those directions.

About a real theory of everything, I always tell people that I wouldn't want to live in a universe where I could understand everything. Or even the important things. And I don't think there's any danger of that.

So you see, I think the search for knowledge is a wonderful dream, but it's a romantic, even Quixotic one. I think we'll learn a lot about controlling the small part of nature that we see. But truly understanding anything deep, when we aren't even aware of most of what's out there? Not very likely.

I'm afraid I can't even take science seriously at some level, although I thrill to its beauty, and am willing to dedicate my life to the search. But it's hubris to expect too much, because any reality we can perceive now, can only pertain to where we are now on the evolutionary ladder. I think that when we die, and go to heaven, and ask God how things really work, he will look at us and smile, and say, "first you need a brain transplant."

As for my views on quantum theory, I'll tell you sometime when we're drunk. Anyway, thanks for coming, I enjoyed it very much. And I very much would like to come down and see you soon.

19-12-02 *Which One Really?* (to G. L. Comer)

Great new little poem. Here, the lines that took me were: "The freedom to choose! The source of chance." Often someone will say to me, as Howard Barnum did,

Barnumism 1: *Here's a caricature, so feel free to object: Bell's worry about the foundations of QM has been: that we have "measurement" as an "unanalyzed primitive" of the theory. Everett shows us how to get around that. You don't like Everett's resolution because you* want *to have an unanalyzed primitive around so it can be the locus of free will.*

And I say it is not that. The universe has within its categories two species, one is chance, and one is free will. Free will does not rely on chance as its source. Instead, it's only through the intercourse of the two that we get a real birth.

Gotta run to a big division meeting. Such things are always scary to me.

20-12-02 *Oh Haight Ashbury* (to N. D. Mermin)

Merminition 92: *Footnote 13 repeats without acknowledgment a point that you, Carl, and Rüdiger love to make. More disturbingly, it promulgates a joke that arose (for me) in the course of an email exchange with you that I can no longer find. For the life of me I can't remember whether I made the joke or you did. Googling on +"go ask Alice" +"entangled state" produces nothing. Feel free to delete footnote 13 if you think I'm stealing your line.*

Funny you can't remember the origin of the "Go Ask Alice" line. We had a conversation about it the moment I arrived at your MerminFest. Since it didn't sink in then, I'll tell you the story again. At 6:56 PM that morning, I sent off a note to my old friend Greg Comer titled "Go Ask Alice"—I'll place the first part of it below. After the part I quote here, however, the letter becomes quite personal.

So, you can imagine the shock when at 12:15 PM I receive a note to you titled "Go Ask Alice"!!! I have always had a fear of getting a note written and then, by accident, sending it to the wrong person! I thought, "Oh my God, I finally did it." However, reading your note, I quickly realized that there was no connection at all . . . (other than a Jungian synchronicity).

Merminition 93: *I don't see what your teleportation example (pages 11, 12) adds to ordinary EPR. Aren't all the issues exactly the same if Alice "in her laboratory prepares" the single qubit in (1) that she possesses by an appropriate measurement (to be sure, she can't control which outcome she'll get, but that doesn't seem to be central to your point, or is it?) after which she and only she knows what the outcome of the corresponding yes-no measurement on Bob's qubit will be.*

But this, of course, has been debated in the EPR context for generations, and I don't see the force of your argument that the would-be informationist should not be weak in the knees. Everybody agrees that Alice is the only one who can do the trick. There's no problem if she only does it once. Bob says the YES was random and only Alice knows that it had to be YES. Bob can think she has delusions of grandeur. But if she does it right 10,000 times, then on run 10,001 Bob would be a fool if a certain confidence that Alice can call it every time should start to enter his mind too.

So to find out what the qubit will do you do indeed have to go ask Alice. (Isn't that an old Jefferson Airplane (pre-starship) song?) You ask her; you don't ask the qubit (as you guys like to say). But by run 10,001 everybody who has been paying attention (except confirmed Humeans) will be fairly sure that "the system [is] prepared to reveal" whatever answer Alice has sent over in a lock-box.

I'm not saying I agree with Penrose on this. Just that I don't see that you've helped very much in relieving the queasiness one is left with when one denies the psi-ness.

On 5/29/02 I finally replied at length in a note titled "I Think She'll Know." The opening lines of the note were, "Remember what the dormouse said; feed your head."

But back to your question: I think beyond a doubt, for the present context, you were the inventor of the phrase "Go Ask Alice." It's a nice way to put it. (But only people of our generation are gonna get it.)

But the importance of the point in our little group, as far as I can remember, was first brought out clearly by Carl. It's always been Carl's favorite "argument" for the subjectivity of the wave function. This piqued my own curiosity to see where we first put it in print. I could find a trace of the idea in Footnote 44 of our paper `quant-ph/9601025`. Also I could see it shining through just after equation 4.105 on page 120 of my `quant-ph/9601020`.

If you think `quant-ph/9601025` gets sufficiently close to the mark, it might be nice if you'd cite it.

24-12-02 *Give Us a Pluriverse* (to G. L. Comer)

Comerism 3: *I've been fiddling around with the idea, though, that chance is a result of the intercourse between free-willed entities.*

Yes, I think I like that better. I had played with still a different turn for a while. One that I might sloganize like this: Chance is what you call "it;; when viewed from the outside; free will is what you call "it" when viewed from the inside. What I wrote you in the last note, was a small attempt to get away from the monism of that slogan. But what you said above, I think, might now appeal to me even more.

Merry Christmas.

02-01-03 *Glory Days* (to A. Peres)

Asherism 5: *Yesterday the Dept Chairman informed me that it was impossible to extend by one more year my professorial position, because of the dismal financial situation of Technion (well, the whole country is bankrupt). . . .*

I am not unhappy to "retire" this fall. I won't have to teach or bother with administrative duties, and I'll have more time for research.

Congratulations on your new career move! You should think of yourself as being a postdoc again, and what glorious days you will have!

Here: I'll give you first postdoctoral research project! I learned predominantly from you that the proper analog to the quantum state in classical physics is the Liouville distribution. However, I learned from Wigner that the only time evolutions for quantum states that are overlap preserving are (up to phase equivalence) the unitary evolutions:

- V. Bargmann, "Note on Wigner's Theorem on Symmetry Operations," J. Math. Phys. **5**, 862–868 (1964).
- C. S. Sharma and D. F. Almeida, "A Direct Proof of Wigner's Theorem on Maps Which Preserve Transition Probabilities between Pure States of Quantum Systems," Ann. Phys. **197**, 300-309 (1990).
- U. Uhlhorn, "Representation of Symmetry Transformations in Quantum Mechanics," Arkiv För Fysik **23**, 307–340 (1963).

My question is, what are the complete set of time evolutions on a classical phase space that are overlap preserving for Liouville distributions. As far as I can tell, this question has never been tackled in the literature.

06-01-03 *Pedagogy* (to N. D. Mermin)

Now I'd like to use your patient ear as an excuse to think out loud. This is what I was referring to when I wrote you yesterday, "Much more soon."

Today, I've got to spend part of the day preparing a talk to give at Bell Labs tomorrow. Here's the title and abstract I sent in for it:

> Title: Representing Quantum Mechanics on the Probability Simplex
> Abstract: Classical information theory is about input probability distributions, output probability distributions, and the transition functions that connect them. Quantum mechanics and so far quantum information theory, on the other hand, have been traditionally formulated in terms of linear operators on a complex vector space and the linear superoperators that connect them. To automate a comparison between the two theories, a means for expressing the newer theory in a way that leans toward the older, more established one ought to be sought. It can be done. This talk is about a small part of that project and a couple of mathematical questions it poses.

In substance, the talk will focus predominantly on the stuff I presented to you (privately) in Montreal, but will give a little more emphasis to the stuff about symmetric POVMs that Gabe looked at this summer.

However, I'd like to give the beginning of the talk a little different slant than I had previously. Here it is. (Here's the part where I'm thinking out loud.)

Everybody has their favorite speculation about what powers quantum information and computing. Some say it is the superposition principle, some say it is the parallel computation of many worlds, some say it is the mysteries of quantum entanglement, some say it is the exponential growth of computational space due to the tensor product. For my own part though, my favorite speculation is that it is Newton's Third Law: For every action, there is an equal and opposite reaction. Indeed I sometimes wonder if the very essence of quantum mechanics isn't just this principle, only carried through far more consistently than Newton could have envisioned. That is to say, absolutely NOTHING is exempt from it.

What do I mean by this? What might have been exempt from the principle in the first place? To give an answer, let me note an equivalent formulation of old Newton. For every REACTION, there is an equal and opposite ACTION. Strange sounding, but there's nothing wrong with it, and more importantly, this formulation allows for the possibility of an immediate connection to information theory. In particular, we should not forget how information gathering is represented in the Shannon

theory. An agent has gathered information—by the very definition of the process—when something in his environment has caused him to REACT by way of revising a prior expectation $p(h)$ (for some phenomenon) to a posterior expectation $p(h|d)$ (for the same phenomenon).

When information is gathered, it is because we are reacting to the stimulation of something external to us. The great lesson of quantum mechanics may just be that information gathering is physical. Even something so seemingly unimportant to the rest of the universe as the reactions that cause the revisions of our expectations are not exempt from Newton's Third Law. When we react to the world's stimulations upon us, it too must react to our stimulations upon it.

The question is, how might we envision a world with this property—i.e., with such a serious accounting of Newton's law—but in a way that does not make a priori use of the information gathering agent himself? If the question can be answered at all, the task of finding an answer will be some tall order. For never before in science have we encountered a situation where the theorizing scientist is so inextricably bound up with what he is trying to theorize about in the first place.

It's almost a paradoxical situation. On the one hand we'd like to step outside the world and get a clear view of what it looks like without the scientist necessarily in the picture. But on the other hand, to even pose the question we have to imagine an information gathering agent set in the middle of it all. You see, neither Shannon nor any of modern information theory has given us a way to talk about the concept of information gain without first introducing the agent-centered concept of an expectation $p(h)$.

So, how to make progress? What we do know is that we actually are in the middle of the world thinking about it. Maybe our strategy ought to be to use that very vantage point to get as close as we can to the goal. That is, though we may not know what the world looks like without the information gathering agent in it, we certainly do know something about what it looks like with him in: We know, for instance, that he ought to use the formal structure of quantum mechanics when thinking about physical systems. Beyond that, we know of an imaginary world where Newton's Third Law was never taken so seriously: It is the standard world of classical physics and Bayesian probability.

Thus, maybe the thing to do first is to look inward, before looking outward. About ourselves, at the very least, we can ask how has the formal structure of our *behavior* changed since moving from what we thought to be a classical Bayesian world to what we now believe to be a quantum world? In that DIFFERENTIAL—the speculation is—we may just find the cleanest statement yet of what the quantum world is all about. For it is in that differential, that the world without us surely rears its head.

To do this, we must first express quantum mechanics in a way that it can be directly compared to classical Bayesian theory, where the information-gathering agent was detached from the world. That is what this lecture is about ...

As I say, just thinking out loud. Thanks for the imaginary ear.

08-01-03 *Your Note to Grangier* (to N. D. Mermin)

I haven't read the Grangier paper outside of the sentence he quoted from me, but I did read your letter. I think you do me pretty good justice, and I rather liked your explanation.

Merminition 94: *At the beginning of section II of* `quant-ph/0301001`*, I believe you miss Chris Fuchs's point. He is not talking about Bell's theorem; he is talking about something rather like pre-Bell EPR. After you have made a measurement on subsystem A, the information you acquire permits you to assign a quantum state (as defined by your second paragraph [in boldface type]) to subsystem B. Before the measurement on subsystem A, subsystem B had no quantum state.*

Fuchs uses this to argue that the quantum state of a system (or subsystem) — when it has one — cannot be an objective property of that system since statehood can (under EPR conditions) be conferred on a system from afar.

You want to have it both ways — denying action at a distance, yet maintaining the objectivity of the quantum state. I believe you can do it, but only if you acknowledge that the objective state of subsystem B after subsystem A has been measured (so B does indeed have a state) is not a local property of subsystem B.

I would say that the state of subsystem B is a compact way of summarizing (1) the preparation of the A-B system that resulted in the original EPR state, (2) the fact that nothing further was done to B, (3) the fact that a measurement was performed on A, and (4) the outcome of that measurement. Because (1)-(4) are all objective facts (Fuchs would disagree about this) the state of B can be said to be objective. But it would be dangerously misleading to call it an objective property of B, because this suggests something residing in B, whereas (1)-(4) are statements about both A and B and their earlier history.

True enough. But then here's my challenge to you. It's something I should have challenged to you too long ago. Once you have that a state can be said to be objective, and once you have that a measurement specification can said to be objective, then through the Born rule you have that the probabilities generated by the measurement can be said to be objective too. If so, then you must be able to give me a definition of what it means to be a particular probability value q in a way that 1) is not circular, and 2) makes no necessary use of a gambler. (For after all, invoking a gambler is just another way of invoking the agent/observer/experimentalist once again.)

10-01-03 *Filth Under the Rug* (to N. D. Mermin)

Merminition 95 : *What's wrong with the old frequentist definition? If N different sets of qubits are subject to those same objective conditions the fraction of final measurements giving the outcome x gets very close $p(x)$ when N is large.*

Granted you need more probabilistic statements to say what you mean by "gets very close" (is that what you meant by circular?) but surely that's an issue for any non-Bayesian view of statistics and not peculiar to the interpretation of QM.

Now it is MY turn to be shocked by the triviality of YOUR reply. I almost feel like it is 1996 again, and we haven't made a bit of progress in our discussions. That bugs the hell out of me. What have I been wasting my time on all these years? You might as well still be writing papers that say, "If all quantum puzzles can indeed be reduced to the single puzzle of interpreting objective probabilities, I would count that as progress."

If what you're still shooting for is to sweep the issues of quantum mechanics under THAT rug—and there's every indication you are—you're just going to find more filth and dirt there. What a shame really. I thought you had been slowly absorbing the Bayesian point all this time. I guess I had just not expected you to fail my challenge in this facile way ... and I'm taken aback.

I'm fairly confident I understand (from your notes to Philippe and Rüdiger and also your newest paper) your latest view of what the quantum state is about. And it might as well be the same view as expressed in Asher's paper "What is a State Vector?" [AJP, 1984, p. 644]. What he calls a "procedure" or an "instruction set" you call a "history," but that's essentially where the difference ends. And just as that paper led Asher to no deeper or more convincing insight for 15 years, so too it will be with you if you continue down this ... I almost said "path," but maybe I should say "dead end."

Let me pick up on your last sentence above:

Merminition 96 : *Granted you need more probabilistic statements to say what you mean by "gets very close" (is that what you meant by circular?) but surely that's an issue for any non-Bayesian view of statistics and not peculiar to the interpretation of QM.*

You get this completely backwards. The great insight Ed Jaynes had, and that Carl and Rüdiger and I have slowly been reckoning with, is that because quantum mechanics is so intimately tied up with probability, one cannot hope to disentangle the troubles of quantum mechanics without FIRST clearing up what the formal structure of probability theory is actually about. And on that first count, we think the Bayesians are the winners.

Once that is accepted—clearly you haven't accepted it yet, but that is no matter for the argument I want to make—then the task is to ask, what are the IMPLICATIONS of that acceptance for our understanding of quantum mechanics? What my debates with you, and to a lesser extent Carl, and to a still lesser extent Rüdiger, have been about since the beginning of your BFM murmurs is just this: What are the implications?

My starting point has been the unbending acceptance that probabilities are of the (de Finettian) subjective caste. What are the IMPLICATIONS of this? Well, the first thing one gets is that the quantum state is of the same subjective caste. But then—and I don't know why it was so hard to stumble across this, except possibly for sheer prejudice—the next thing one gets is that at least some quantum operations are also of the same subjective caste. For beauty's sake, I then go further than Carl and Rüdiger are presently willing to go, and say, "If so be it for SOME quantum operations, then so be it for ALL quantum operations." But the main point is that the first three steps of this paragraph are pure implication.

You called it poetry at the Montreal meeting—yes, it did hurt a little—but it is logic just as clean as you can get it. And it's of the most elementary sort. (That's what made your remark hurt.) The only thing that makes it appear to be poetry to you is some deep resistance and, I suspect, fear of where it leads.

So, I have implications that run FROM interpretation of probability TO quantum mechanics. So what? If it just stays at you saying objective every time I say subjective, then this is a worthless exercise and a waste of time. There had better be more to it. And I claim there is.

For, I would say the implications above lead me down a mathematical path. Whereas your hope to retain the word objective for these structures leads you nowhere.

1. My point of view COMPELS me to ask whether there is a way to think of a quantum state as a (single) probability distribution, plain and simple. With a little toil, I find there is.

2. My point of view COMPELS me to seek out the analogies between Bayes' rule and quantum collapse. With a little toil, I find an analogy that's never been found before.

3. My point of view COMPELS me to ask why the notion of quantum measurement is anything other than the refinement of one's belief, i.e., exactly what classical (Bayesian) measurement is about. With a little toil, I find that it is precisely that after all ... just that in the quantum case there is an extra little kick given to my final state of belief.

4. Here, I think this is the most important one: My point of view COMPELS me to ask, if a quantum operation is as subjective as a quantum state, then why are the two mathematical structures not formally identical? And we are led back to Jamiołkowski's and Choi's old insight: A quantum operation IS a density operator. With a little reflection, one sees that

that had to be ... in the same way that *prior* probabilities $p(h)$ and *conditional* probabilities $p(h|d)$ are both probabilities nevertheless.

I would dare say that your point of view—where probability theory is, at very best, secondary to, or at very worst, absolutely detached from the deeper issues of quantum mechanics—would leave all of these things as little more than coincidences. "There is a way to map quantum operations and unitary operators to density operators? Who cares? It's just as mysterious as the structure of quantum mechanics to begin with."

But as long as there are coincidences in the structure of the theory, that structure will always be a mystery. What I think Bayesian probability theory does for us is COMPEL us to view as natural the connections we see within the axioms of quantum theory, rather than as miracles plain and simple.

So you see, you have depressed me. If I can't make any headway with my best and most sympathetic friends—you're one of them—I don't see how I'm going to make any headway in the wider world. Even YOU had not realized that all this talk about Bayesian stuff was meant to LEAD us, and not just be an afterthought tacked on for NOTHING BUT philosophical reasons.

Have you read Carl's document "Resource material for promoting the Bayesian view of everything" posted at his website `http://info.phys.unm.edu/ caves/`? It would do you some good. It's about time you took a course in Bayesian Ideas 101. "What's wrong with the old frequentist definition?"—that about knocked me over!!

If after reading this note, you don't think it is too offensive, I may forward it to Carl and Rüdiger. I'll bet they too will be shocked—though much more polite in reaction than me—by this dangerous frequentist tendency you're starting to reveal.

I thought I was going to write a little report on your Copenhagen Computation today, but I knew I couldn't touch it until I got this off my chest. Sorry about that. I won't be able to write you a report until Sunday now. (I'll be in NYC tomorrow.)

In friendship, disappointment, and enduring hope, ...

13-01-03 *Ouch* (to N. D. Mermin)

I told Kiki the other night, "Well, I probably lost a friend today." In an ominous voice she replied, "What'd you do?" I said, "I wrote David Mermin a scathing note about some stuff in quantum mechanics." In a scolding tone, she said, "Why do you always do that?" I said, "I just couldn't take it. This guy hasn't hardly absorbed a thing in our six years of discussion!" She said, "Why do you get so upset? You know you're work's never going to be done: Churches never go out of business, do they?"

Ouch.

13-01-03 *Der Kopenhagener Geist* (to N. D. Mermin)

I actually don't have much to say in the capacity of a referee.

First, a couple of typos: [...]

Finally, a little technical point: [...]

Now, let me tell you the thoughts you provoked as I was reading the paper.

Merminition 97: *The state of n Qbits has no meaning going beyond the abstract state vector itself, together with the rules for how it can be constructed and the computational uses to which*

it can be put. We return to this below, merely noting for now that although we shall speak often, as everybody does, of "the state of n Qbits" the terminology is potentially misleading. It must not be taken to imply that the state characterizes a property possessed by and directly inferable from those Qbits, as it does for Cbits. A better, but clumsier usage, would be always to say "the state associated with n Qbits".

For my own part, I would say, "the state ascribed to n Qbits." "Ascribed to" is not a hell of a lot clumsier than "of," and it has the advantage that it makes clear and serves as a constant reminder that the origin of the "state" is not in the system itself, but in a system external to it—namely, the agent.

Merminition 98 : *The fact that the generic multi-Qbit state is incompatible with associating states with the individual Qbits is already an indication that Qbit states have a much more abstract character than the states of Cbits, which are always products of one-Cbit states.*

Nice sentence. I like it. That's the way everyone ought to view the issue.

Merminition 99 : *The state associated with the Qbits is merely an extremely convenient way of recording the potential consequences of the past actions of the computer on those Qbits. The consequences of those past actions can become accessible in only one way, and this way is the only way to extract information from n Qbits: one can measure them.*

There's something about this that I don't quite like, but I'm having trouble putting my finger on it. Probably has something to do with your using the word "consequences" in a way that I wouldn't endorse. See glossary on page 49 of my *Quantum States: W.H.A.T.?*

Merminition 100 : *The Born rule contains, as a special case, a quantum imitation of the unproblematic (and therefore usually unremarked upon) process of extracting information from Cbits. . . . The statistical, state-altering character of the outcome of a measurement of n Qbits in a general state becomes the deterministic, state-preserving, unproblematic classical extraction of information when the state is one of the 2^n classical-basis states. (page 7)*

and

Merminition 101 : *The view of quantum mechanics I gave my computer scientists relies on a primitive notion of measurement, without which the computation has neither a beginning nor an end. A measurement gate is a black box whose interaction with the n Qbits results in an unambiguous output on a display, whose reading is as unproblematic as reading the display of an ordinary classical computer. Measurement is where the quantum-computational process starts, by permitting the association of an initial state with the Qbits, and finishes, by producing an unambiguous digital output. Quantum computer science delves no more deeply into how information is actually extracted from a measurement than does classical computer science, where the preparation of the initial state of the Cbits and the reading of their final state are steps too trivial to warrant explicit theoretical attention, though they are certainly of concern to the engineers who design the computer. (page 9)*

I like these lines. But ask a philosopher if this is unproblematic. The reason the physicist finds it unproblematic is the same reason he finds the independence or nonindependence of the continuum hypothesis unproblematic in transfinite set theory: He never thinks about it. But philosophical lives have come and gone on the question. (Probably, the question first came to life for me in reading William James's little book, *Pragmatism*.)

Anyway, I view your lines above as the greatest contribution of the present paper. That is, because they sort of soften up the western front for a (one-day-in-the-future) full-fledged assault. To the extent that you can find yourself willing to do that, I'm happy.

The way I would emphasize the issue, though, is to say that quantum measurement is either as *problematic* or as *unproblematic* as classical measurement, take your pick. The main point is that it is not MORE problematic.

Here's the way I put it in my `quant-ph/0205039`:

> As far as Bayesian probability theory is concerned, a "classical measurement" is simply any *I-know-not-what* that induces an application of Bayes' rule. It is not the task of probability theory (nor is it solvable within probability theory) to explain how the transition Bayes' rule signifies comes about within the mind of the agent.

And here's the way Rocco Duvenhage put it in another paper in `quant-ph`:

> In classical mechanics a measurement is nothing strange. It is merely an event where the observer obtains information about some physical system. A measurement therefore changes the observer's information regarding the system. One can then ask: What does the change in the observer's information mean? What causes it? And so on. These questions correspond to the questions above, but now they seem tautological rather than mysterious, since our intuitive idea of information tells us that the change in the observer's information simply means that he has received new information, and that the change is caused by the reception of the new information. We will see that the quantum case is no different.

The reason I say you are softening up the western front is because, though you seem to admit this for a single, particular quantum measurement—the computational basis—you haven't yet had the heart to admit it for ALL quantum measurements.

The only things, it seems to me, that set the quantum case of measurement apart from the classical case is A) what we do with the information we gather, and B) what we concede the information is about. In the classical case, we enact "Bayes rule full stop" with the information we've just gathered. In the quantum case, we generally do something more (unless we are confident that the system we are talking about was not touched physically by our measuring device). Concerning B), we had gotten in the habit classically of thinking that the information we've just gathered is about "what is" or "what was." Quantum mechanics instead teaches us to look to the future. The information is about "what will be." (I use the measurement device LOCKED AWAY in the bureau of standards to make it dramatic.)

So, the action, the excitement, of quantum mechanics is not in the measurement, but in what it is that we're presupposing about the world that causes us to process our data differently than we would have classically.

Merminition 102: *A state can be associated with the Qbits only if their prior history is of a certain special form. The state can then be constructed out of the particular features of that history: the outcome of the initial measurement and the particular sequence of unitary gates subsequently applied to the Qbits prior to the moment at which one associates the state with the Qbits. But there is no way to determine that state if one is simply presented with the Qbits; only those who know this history know what state to assign to the Qbits. The state does not reside on the Qbits; it is a concise encapsulation of those features of their history, back to the initial measurement, that are relevant to the outcome statistics of a subsequent measurement.*

and

Merminition 103: *But to describe this as a collapse of the state of the Qbits and to regard it as a second kind of time dependence that Qbits can have in addition to their unitary evolution, is to ignore the whole point of the state vector and its unitary evolution, and, of course, to confuse the Qbits themselves with the state vector that compactly summarizes the statistical implications of their past history for future measurement outcomes.*

Beside these two passages, I wrote in the margin, "Asher's 'What Is a State Vector?'." It's interesting that you (in your note Friday evening) called this accusation to be that of a "regress." This is because I think an infinite regress is all this view is gonna get you. That is to say, ultimately you're going to have to admit that the quantum state "compactly summarizes the statistical implications of [the] past history" of the entire universe.

Look at the discussion around Eq. (14) in my `quant-ph/0205039`. What you would call a measurement is always determined by a further quantum state for the apparatus. And off to infinity (or the boundaries of the universe) it goes.

Merminition 104: *Indeed, the generalized Born rule demonstrates that their state cannot be regarded as a property carried by the Qbits, since it provides an indirect method for associating a state with n Qbits that share an entangled state with an additional Qbit, and therefore cannot initially be associated with any state of their own. By measuring only the additional Qbit, one disentangles the $(n+1)$-Qbit state and is able to assign a state to the n Qbits, even though nothing interacts with them during or after the one-Qbit measurement. This is only possible because the association of all $n+1$ Qbits with an entangled state prior to the measurement requires the additional Qbit to interact with the n of interest at some time before it is measured. If one knows enough about that past interaction to determine the original $(n+1)$-Qbit entangled state assignment, then it is not surprising that the outcome of the measurement on the single Qbit can provide enough additional information to permit the assignment of a state to the n Qbits. This newly assigned state cannot be a property inherent to the n Qbits, because nothing interacts with them during the process that takes them from stateless and externally entangled to having a state of their own.*

Tell this to Philippe Grangier a thousand times and tell me whether you've made any more progress than I have. I think it is just silly to say "a state is a property" or "a state is a reality," and then say "some systems have no properties" (when they are entangled). The realization he should have is rather, systems have whatever properties they have, it is just that "the" quantum state is not one of them.

Finally,

Merminition 105: *[I]n our description of nature the purpose is not to disclose the real essence of the phenomena but only to track down, so far as it is possible, relations between the manifold aspects of our experience. — Bohr (1934)*

I don't like this quote, precisely because it is going to cause people to accuse you of what they always (unjustly) accuse me of: BANNING a set of questions that every physicist in his right mind ought to be asking. What is the real essence of the phenomena?

I have a pretty good reply to that now. I'll attach it (ForSlusher.pdf) for your midnight reading.

15-01-03 *The Ability to Write* (to N. D. Mermin)

I got a critique of one of my email expositions from a friend this morning, and he said:

> . . . the tone is probably appropriate. [But] it's about twice as long as (I thought) it needs to be. Is everyone in quantum foundations as verbose as you?

He's got a grain of truth there. But I find it so hard to rope myself in sometimes. I guess the best writer says what needs to be said and little more. I've got to work on the demons inside me.

15-01-03 *Memory Lane* (to O. Cohen)

[**NOTE**: The following is an unfinished note, composed on 12-13 December 2002. It wasn't sent until the present date, however.]

It seems I have email contact with you only about every three years. I hope things are going well. Where are you now, and what are you doing professionally? The last I remember, I recommended you seek a postdoc position with Carl Caves in New Mexico; I guess that never materialized.

Anyway, I'm writing you because I've been reading your paper "Classical Teleportation of Quantum States" this week. It's a nice paper, and I very much like the simplicity of your scheme and the point you make with it. I am in complete agreement.

In fact it took me a little down memory lane. You see, Asher Peres and I had used teleportation as an example in our March 2000 *Physics Today* article, "Quantum Theory Needs No 'Interpretation'," precisely to illustrate the sensibility of the conception of a quantum state as a "state of knowledge, rather than a state of nature". When the paragraph peaked in clarity (i.e., before the editor's knife), it went like this:

> The peculiar nature of a quantum state as representing information is strikingly illustrated by the quantum teleportation process. In order to teleport a quantum state from one photon to another, the sender (Alice) and the receiver (Bob) need a pair of photons in a standard entangled state. The experiment starts when Alice receives another photon whose polarization state is unknown to her, though known to some preparer in the background. She performs a measurement on her two photons, and then sends Bob a classical message of only two bits, instructing him how to reproduce the unknown state on his photon. This economy of transmission appears remarkable because to completely specify the state of a photon, namely one point in the Poincaré sphere, we need an infinity of bits. However, the disparity is merely apparent. The two bits of classical information serve only to transfer the preparer's information, i.e., his *state*, to be from describing the original photon to describing the one in Bob's possession. This can happen precisely because of the previously established correlation between Alice and Bob.

At the time I was basing my opinion predominantly on the result of Cerf, Gisin, and Massar (`quant-ph/9906105`, "Classical Teleportation of a Quantum Bit") along with the long heartfelt conviction that the idea of a quantum state as a state of knowledge gave the most sensible and constructive point of view about quantum mechanics. Sometime after that though, Steven van Enk and I—to the best of my recollection—worked out a scheme pretty similar to your own at the chalkboard at Bell Labs. We never wrote it down however.

But that doesn't take away from your discovery. It's a very clean example, isn't it? The conclusion you draw, I think, is particularly important: the phenomenon of quantum teleportation only looks surprising and remarkable if one takes an ontic view of the quantum state. In fact, in the past, I have accused some of my friends (some of whom were authors on the original teleportation paper) of sticking with an ontic interpretation of the quantum state precisely because it is the only way to keep the phenomenon surprising and newsworthy. You might enjoy reading some of

my correspondence with them on the subject: Have a look at pages 175-176 and 184-189 of my samizdat *Quantum States: What the Hell Are They?* The said accusation comes on page 189. That correspondence occurred, by the way, as they were debating about how a dictionary definition of "quantum teleportation" should be written. (To get a copy of this samizdat, you can download the pdf file for it at my webpage; there's a link to the webpage below in my 'signature'.)

If you are interested in seeing the struggle Asher and I had in constructing the paragraph above (and the reasoning and cues behind it at the time), have a look at the discussions in my other samizdat, *Notes on a Paulian Idea*, `quant-ph/0105039` (or you can download a better indexed version of it from my webpage). The pages to look at are 312, 316, 319-320, 322, 326, 327-330. I have pages 326, 328, and 329 marked as the most interesting in my notebook (can't quite remember exactly in what way though). Maybe the main lesson in those discussions is how difficult it is to give up an objectivist language when using quantum states, even for a recalcitrant positivist like Asher, and even in an example intended to be illustrative of why quantum states should be viewed as states of knowledge, rather than states of nature. (The philosophy being: if quantum mechanics looks too very mysterious, then you're probably being wrong-headed about it. Case in point: if teleportation looks mysterious, then you're probably being wrong-headed about it too.)

The sense I get from your paper is that you are much more neutral about the lesson than I am. You say simply: "[O]ur classical version of teleportation is just as impressive as the original protocol, if we think of quantum states as representing states of knowledge. ... If, on the other hand, we think of a quantum state as having ontological content, ..., then our classical version of teleportation is not equivalent to the quantum case," and leave it at that. However, there is a spate of evidence starting to come out that a significant fraction of some of the most 'remarkable' phenomena in quantum information theory can be mocked up with classical toy models just as your own. The only requirement for seeing it is that one must focus on the epistemic states (i.e., the states of knowledge) in such models rather than the ontic states (like the actual H or T in your own model). For instance, Rob Spekkens has a toy model which he has presented in several conferences and which he is writing up presently as a paper, "In Defense of the Epistemic View of Quantum States: A Toy Theory," in which he can reproduce the following quantum mechanical and quantum information-theoretic type phenomena in a pretty NONremarkable way: the noncommutativity of measurements, interference, a no-cloning theorem, a no information-gain-without-disturbance principle, the multiplicity of pure state decompositions of a mixed state, the distinction between two-way and intrinsic three-way entanglement, the monogamy of entanglement, superdense coding, mutually unbiased bases, locally immeasurable product bases (i.e., what we originally called 'nonlocality without entanglement'), unextendible product bases, the possibility of secure key distribution, the impossibility of bit commitment, and many others. (In particular, he gets teleportation too, just like you do.) As Rob puts it in his abstract:

> Because the theory is, by construction, local and non-contextual, it does not reproduce quantum theory. Nonetheless, a wide variety of quantum phenomena have analogues within the toy theory that admit simple and intuitive explanations. ... The diversity and quality of these analogies provides compelling evidence for the view that quantum states are states of knowledge rather than states of reality, and that maximal knowledge is incomplete knowledge. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

So, given that your paper is an independent and particularly notable link in that, and as opposed

to his paper, your result is not buried within over 70 pages (and counting) of text, I very much endorse it. I think the lesson is this: A good lot of quantum information theory is simply regular probability theory and information theory applied in ways that had not been deemed interesting before. What is interesting and unique to the quantum itself, thus, must be something else.

In my paper `quant-ph/0205039`, "Quantum Mechanics as Quantum Information (and only a little more)," I tried to give the community to a call to arms by saying this:

> This, I see as the line of attack we should pursue with relentless consistency: The quantum system represents something real and independent of us; the quantum state represents a collection of subjective degrees of belief about *something* to do with that system (even if only in connection with our experimental kicks to it). The structure called quantum mechanics is about the interplay of these two things—the subjective and the objective. The task before us is to separate the wheat from the chaff. If the quantum state represents subjective information, then how much of its mathematical support structure might be of that same character? Some of it, maybe most of it, but surely not all of it.
>
> Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as subjective information will we be in a position to make real progress in quantum foundations. The raw distillate left behind—minuscule though it may be with respect to the full-blown theory—will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.

What your work and Spekkens' work does, from my perspective, is give the best illumination yet of what I was hoping for when I was speaking of "combinations of terms" in that passage. Teleportation—being a certain combination of uses of the axioms of quantum mechanics—is nevertheless a purely probabilistic or information-theoretic effect. As such, it tells us very little about the ontology behind quantum mechanics.

My own view—and the thrust of my research program presently—is that these examples help us to realize that what is unique in quantum mechanics is not the probabilities (i.e., the quantum states) but what the probabilities are applied to. There, I think, lies the essence of quantum mechanics: It is localized in the Kochen-Specker theorem. "Unperformed measurements have no outcomes," as Asher Peres likes to say. That is to say, where quantum mechanics gets its uniqueness is from breaking with the old idea that a probability (as a subjective state of knowledge) must be knowledge about a pre-existent reality. Instead, probabilities can just as fruitfully be applied to capturing one's knowledge of "what will come about due to one's actions." The predominant issue becomes how to formalize the difference between probability theory as applied to pre-existent facts and probability theory as applied to "creatables" (for want of a better word).

There are some lines for tackling this idea buried within Sections 6.0 and 6.1 of my `quant-ph/0205039` ...

21-04-03 *A Question of Condensation and Time* (to J. I. Rosado)

Thank you for your interest in my paper. Have we met before? Perhaps in Oviedo? Anyway, I apologize for taking so long to reply to you, but for over a month now I have been scrambling with the details of relocating my family to Dublin. Finally that task is coming to an end, and I hope to be able to turn my attention back to science soon.

Sánchezism 1: *In section 3 you give an argument, EPR+teleportation, to support the idea that a quantum state is a state of beliefs, so that they have nothing to do with real properties of the real physical state. At the same time, I think, your argument also implies that the outcome of a measurement doesn't reveal a preexisting property of the physical system, inclusive for pure states, inclusive in the case of a measurement of an observable that has that pure state as eigenstate. Do you think that this conclusion is true? If true why don't you have stressed it? I think it is very important to explicit this conclusion because in the vast majority of the quantum literature we can find phrases like: "quantum measurement, with very few exceptions, cannot be claimed to reveal a property of the individual quantum system existing before the measurement is performed." so that the vast majority of the scientific community thinks that if we can predict with certainity the outcome of a measurement then this is so because we know a real property of the individual quantum system.*

Yes, that is true. I have tried to stress it very significantly to many of my friends, but unfortunately I may not have yet carried the point through as clearly as possible in my papers. It is a question of time, finding the right phrases, and biting the bullet for a big job. What I mean by the latter is condensing the notes in my document "Quantum States: What the Hell Are They?" into a proper paper. If you are not familiar with that document, I would encourage you to try to follow some of the arguments there, starting around page 35. The content of the samizdat follows the format of email messages, but often they are self-contained. (You can download the samizdat from my webpage; there is a link for it below.) I have it as a goal to try to write a real paper on the subject before the end of the year, but we shall have to see if that really materializes.

Sánchezism 2: *My conclusion, after I have read your paper, is that if we know with certainty that a measurement will obtain a particular outcome we cannot conclude that this implies a preexisting property of the individual quantum system, only that if we perform that measurement in that physical system we will obtain that outcome with certainty.*

Yes, and I think this realization has the potential to lead us down a profound path. (But that is only a dream at the moment; it is not science yet.) Anyway, it has in part led to my newfound attraction to the philosophy of William James, which you can also read about in the same document. Maybe the best notes are the ones to Wiseman, starting on page 210.

There is much to do to put some technical flesh on these little observations and to follow their implications unflinchingly wherever they lead. I am flattered that you have taken an interest in these ideas, and I hope you'll help carry the torch in your own work.

06-02-03 *Looking at the List Again* (to N. D. Mermin)

Merminition 106:

> [CAF wrote:] I'm particularly keen to see the reaction Spekkens gets. (Did he give you a private version of his talk in Montréal as I had asked him to?)

He may have talked to me in Montréal. In my usual irresponsible muddle-headed way, I can't remember who he is or who I talked with.

He has a talk on about 27 reasons why you ought to think that the quantum state is epistemic in nature:

1. You can't clone a quantum state. Guess what? You can't clone a Liouville distribution either.

2. You can change a quantum state from afar by gathering information nearby. Guess what? You can do the same with classical joint probability distributions.

3. ... and so the list goes.

Moral: You can believe the quantum state to be ontic in character if you want. But then you've got to create an ad hoc reason for justifying each and every one of the effects above. Wouldn't it be so much simpler and more natural to accept the epistemic hypothesis and follow out its consequences?

20-02-03 *Intro Draft* (to J. Bub)

The more times I read over your draft for the intro, the more I like it. I'll send you a very mildly revised version this afternoon.

Possibly the only delicate point will be how I might de-emphasize the role of entanglement enough to suit me, while still pleasing you. A lot of quantum information does not depend upon entanglement at all (most quantum key distribution schemes, for instance, but also quantum non-locality without entanglement, incompleteable product bases, etc.) Or look at the present debate on the power of unentangled states for quantum computation (cf Gilles' talk at the commune). Finally, you ought to know by now that my own opinion is that entanglement is likely to be a red herring in the deeper vision of things: I see it as subordinate to the structure of measurements. Derived and secondary.

20-02-03 *New Draft, Only a Drop More of Poetry* (to J. Bub)

Attached are my modifications to your draft.

I debated for a long time toning down the theme on entanglement, but in the end gave up. I know that I compromise my beliefs somewhat with some of the phrasings, but, very probably, only *I* know that ... and, in the end, I may be able to make an easier connection to the entanglement stampede this way. (My role has always been that of a mole.)

Just some notes for your reference, so that you'll know what I did and what I thought: [...]

2) I inserted "they thought" between "which" and "spelled" in "could exist in certain states which spelled trouble for the Copenhagen interpretation" (second sentence, first paragraph). I did this to leave a window open for the possibility that EPR were on the wrong track with their argument. From my view, the only thing they demonstrated ultimately is that the quantum state is epistemic in character. If Bohr had known that word—epistemic—I think he would have agreed with them up to that point. He would have only disagreed that a more complete epistemic characterization could be given.

3) Second paragraph, fourth sentence: "That is, depending on what measurement Alice chooses to perform ... and the outcome of the measurement, Bob's system will be left in one of the states of some mixture ..." I'll just note this as a sentence that I would not normally write anymore, even though I left it intact. The trouble with it is that it conveys the image that the "state" is something possessed and inherent in the system—i.e., that it is a "property" of the system. Instead, I view the state in question as a property of Alice's head—i.e., her information about the system—and would normally use language appropriate to convey that idea. Since, however, we are contrasting things on Schrödinger's understanding, I can be OK with usage in this instance. [...]

10) Last paragraph. Added one small drop of poetry.

16-06-03 *Exhaustion, Depression, Stagnation, Integration* (to G. L. Comer)

The world is a big world. And our lives are ever such a small part of it. And ever such a big part of it. But regardless, there are limits. I feel I'm not prepared for this. I tell Emma all the time that she is destined for greatness; she will do important things.

23-06-03 *Bayesianism, Yes, but then Something More* (to N. D. Mermin)

Merminition 107 : *Alice measures a Qbit, gets 0, and then sends it to Bob. When Bob measures it he always gets 0. How can this be if the instruction to give 0 is not carried by ("resides in") the Qbit Alice sent Bob?*

Schackcosm 69 : *I believe that this formulation is part of the problem. "He always gets 0" is a loaded phrase. In my view, it can mean one of two things. It can mean (i) that Bob makes N measurements, and each time he happens to get 0. Or it can mean (ii) that for a single measurement, it is certain that he will get 0. The second meaning is what I try to deal with in my previous email, where I claim that certainty always refers to somebody's state of belief. "It is certain" is then always short for "it is my state of belief that it is certain".*

As for meaning (i), if Bob happens to get 0 every time in N measurements, he can use this result to update his belief about future measurements. Or he can update his beliefs about where the qubits came from. But I don't see why he would have to conclude that each qubit carried the instruction "0" before the measurement.

Merminition 108 : *"Have to conclude" is too strong, but wouldn't it be natural for him then to wonder why these two 0 results were so nicely correlated and entertain as an explanation the notion that Alice's measuring 0 imposed the instruction "0" on the qubit? What is it in his Bayesian training that prevents this thought from entering his head? Why should he be content with correlations without correlata?*

There is nothing in his Bayesian training to prevent the thought from entering his head. Instead, it is his training in quantum mechanics that bears the burden: Bell inequality violations and Kochen-Specker. It is a question of looking at the whole package.

Quantum mechanics, as I see it (and maybe Rüdiger too), is a layer added to the top of pure Bayesian reasoning. Bayesianism by itself does not care about the precise character of the left-hand argument in a probability function $P(h|b)$. The h could be a pre-existent fact living in the cold, hard world, or it could be something yet to come— something in existential character completely dependent upon the catalyzing intervention of the agent himself.

All we ask of the community is that it recognize the category distinction between the function $P(h|b)$ and its argument h. That is the first step in clarification. Once one can accept that into one's heart, then—the hope is—great progress will follow.

In particular, I would say, look long and hard at Figures 1 and 2 in my paper `quant-ph/0205039`. That region and the extra "rotation" within it signifies, symbolically at least[1], the extra layer on

[1] I said "symbolically at least" above because I don't want to commit myself to the particular representation of quantum mechanics discussed in `quant-ph/0205039` as in any sense the key to all mysteries. Instead I view it as a kind of scaffold for sharpening the issue—i.e. as a way of giving direct comparison between quantum mechanics and general Bayesianism. But like any scaffold it has to be taken away ultimately ... and maybe there is a way to shortcut the process without ever introducing the scaffold in the first place.

top of Bayesianism that quantum mechanics is asking us to contemplate. Within the shape of that region (or how its volume scales with system size, or some other relevant feature of it) and the style of the "rotations" that we add after pure Bayesian conditionalization lays hidden that which we are all really seeking: A precise statement about the (existential) character of the h's.

I bank on the idea that we will find that they are not correlata or relata at all, but rather creatia. (Simply trying to ontologize or reify the correlations while giving up on the correlata, as you try with your slogan, won't do.) The world is in constant birth. And to the extent that we focus our actions on anything and ask ourselves, "What will come of them?"—i.e., make note of quantum phenomena—we too are involved in that birth.

But I draw the discussion outward, while you want to bring it inward. I'll trust that Rüdiger will do all the hard work of bringing it inward for the present. You should know, though, that you yourself have the whole answer already; you said it completely: "So of course there is no mystery if you don't feel the need for an explanation of the first feature." The best I can do (and the best I will do, when I follow through with my promise) is fluff that sentence into a whole paper. Why give up on quantum measurement outcomes as the revelations of pre-existent facts? It is all and only a case of once bitten twice shy: the other route has been tried and tried too many times.

But to spin the result this way,

Merminition 109 : *Why should he be content with correlations without correlata?*

is to make the project look so very negative. "Be content with"—I hate that. It seems to forget that there is a much better world in return for this piddling little loss. To give you more detail, I'll attach the best shot I've had so far to make that convincing. (I hear Rüdiger chuckling: "I've never said it better than in my note to . . . ".) The file is titled ForSlusher.pdf. If for some reason you can't open it—you often seem to have troubles—it comes from notes to Wiseman on pages 210 and 217 of my samizdat *Quantum States: What the Hell Are They?* on my webpage.

Still beaming from Rüdiger's notes.

27-06-03 *Utter Rubbish and Internal Consistency, Part I* (to N. D. Mermin)

What's the good of Mercator's North Poles and Equators,
Tropics, Zones, and Meridian Lines?"
So the Bellman would cry: and the crew would reply
"They are merely conventional signs!

It's time for me to pay the piper. On November 4 of last year, I had a mystical experience during my flight home from the Montreal meeting: I thought for the first time in my life I had seen with complete clarity why there was no mystery, either to EPR correlations or to the Penrose argument calling for the objectivity of the quantum state. The whole strange flight from Montreal to Chicago to Newark (i.e., American Airlines Advantage Platinum Number D7E5856) I typed away. To the result, David replied:

Merminition 110 : *Thanks for cc'ing me the latest epiphany. I've only had a chance to glance at it and am still torn between whether my response is "that was how I understood your position to be all week" or "what utter rubbish" so I will take a little time before responding.*

David's second instinct was the best: IT WAS UTTER RUBBISH. The great discussions I had with Rüdiger the last two days a) brought me back to the subject, which I had shamelessly not thought about again since writing the original note (a sign that there must have been some inner skepticism or reluctance on my part, but I didn't have the intellectual integrity to confront it), and b) showed me the gross errors of my ways. This note represents both a heartfelt thanks to Rüdiger and an attempt to tell the Quantum Bayesian tale in a way that does it better justice.

Let me start out by describing what I was thinking previously, both to set the tone and because I think it adds a little clarity to the recent discussions Rüdiger and I have been having with David. What my own trouble boiled down to previously was a moment of weakness—a moment when even I lost faith in our Bayesian dream (and sadly did not realize it as such).

Bell inequalities and EPR correlations, what are they good for? I think one thing only: the conjunction of the two concepts provides us with the most dramatic evidence yet that quantum systems do not themselves carry "instruction sets" for specifying the outcomes of "measurements" we can make on them. As Rüdiger said it two days ago, never was there a better exposition of this point than in some of David's writings. Thus, it is a point David should appreciate and take to heart. I know that I did, and it is this that led me astray in other directions.

The question is, how to live with this result? It's one thing to say there is a formal demonstration that there can be no instruction sets inherent within a system. But it is another thing to feel good about it. In particular, why doesn't it get absolutely under our skins that there can be situations where quantum mechanics specifies that we have certainty about the outcome of a measurement? The million dollar question. You see, if there's no instruction set, where could the certainty possibly come from? Or again: If there are no instruction sets, measurement outcomes must come out of the thin air, determined by nothing else. However, if they do truly come out of thin air, how could we ever be certain which outcome WILL occur before it actually does?

The world does what it does, but yet I have certainty? Where did that come from?!? It couldn't come from the world (as demonstrated through the force of Mr. Mermin's writings); it must come from me. Or—to be absolutely clear—that was the path of thought I found myself travelling last November.

IF THE CERTAINTY CAN'T COME FROM THE WORLD, IT MUST COME FROM ME. The interpretation we ascribe our clicks must be a convention. (As Rüdiger pointed out, it all starts to sound too much like Umberto Eco, and I agree now, but I was blinded at the time.) How could that be?

The mystical insight—the false prophet!—was that the trail had just been blazed! It all had to do intimately with our newfound realization that at least some quantum operations must have the same subjective status as quantum states. (I put the "at least some" solely so that Carl will not stop reading at this point; if it were a letter to Rüdiger alone, I would leave out the qualification completely.) A quantum operation is a density operator in disguise. That was somehow to be the key.

In rough terms, I wanted to say that *ascribing* a pure state P to a system and *ascribing* a description $\{P, I - P\}$ to an associated measurement device was just the convention I needed: No matter what the outcome of the "true" physical intervention (measurement), the result would be interpreted as P. But what utter rubbish! And it's hard to see now what I could have been thinking, or even how I could have found it pleasing. It goes against everything I have ever viewed as the great insight of the Paulian idea:

> Like an ultimate fact without any cause, the individual outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case ...

> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.
>
> Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.

The last part of the idea is absolutely crucial. The observer must not be able to influence the results of a measurement, even by hiding it in a convention. I had lost my grip on reality during that flight, and something deep inside me must have sensed it.

Let me try to make this absolutely stark by running down the Rüdigerian path. Consider two agents, a quantum system, and a quantum measuring device. Suppose both agents agree that the relevant feature of the measuring device is that it has two possible clicks, and furthermore suppose they both agree that the clicks are to be interpreted as a measurement of the POVM $\{P, I-P\}$. In our language, these are both subjective ascriptions (more on the word subjective later), but they just so happen to agree. However suppose in contrast to this single belief about the measuring device, they are in wild disagreement about what they believe of the quantum system soon to be intruded upon: One says the quantum system's quantum state is P, the other says it is $I - P$. From our standpoint, there is in principle nothing wrong with this. A quantum state is a subjective ascription (more on the word subjective later), period. It is not determined by the world external to the agent; it is his personally. So what happens to the measurement device when the quantum system is dropped into it? There are two agents, two absolutely incompatible beliefs. Does the measurement device explode?

No. A click occurs, and one agent is *made* wrong. If that agent had bet his life on his utterly extreme belief, he will now be dead. Darwinian evolution will have stepped in to see that his extreme belief is not propagated. That is no convention. The world will smite one of the agents.

Now, let us join the Merminian discussion presently in progress. Let us conceptually erase one of the agents. What can possibly change for the other? Nothing. Once again a firm belief exists and once again there is nothing to keep the world from smiting the believer.

OK, take a restroom break, get some popcorn, and I'll be back with the second half of this note after lunch.

28-06-03 *Utter Rubbish and Internal Consistency, Part II* (to N. D. Mermin)

It was a long lunch. Let me pick up where I left off:

> A click occurs, and one agent is *made* wrong. If that agent had bet his life on his utterly extreme belief, he will now be dead. Darwinian evolution will have stepped in to see that his extreme belief is not propagated. That is no convention. The world will smite one of the agents.
>
> Now, let us join the Merminian discussion presently in progress. Let us conceptually erase one of the agents. What can possibly change for the other? Nothing. Once again a firm belief exists and once again there is nothing to keep the world from smiting the believer.

The ultimate issue is, is there really any difficulty with the idea of an utterly certain belief about an admittedly contingent fact? And, I'll add for later discussion, is that something uniquely quantum mechanical?

I'll tackle the first question by repeating something Rüdiger said yesterday, but in a windier way: I'll paste in two sections from de Finetti's article Probabilismo. (That, by the way, was the reason for my long lunch: I ended up reading Probabilismo from beginning to end again—I think my fourth time since 1996. If I would have only absorbed the darned thing the first time! Each time I read it, I am struck that it is the best thing on probability I have ever read. Yet each time, I miss something really important. In fact I end up feeling bad about myself, for it shows me just what an amateur I am. The article's *got* to be made standard reading!)

18.

> "A gambler wants to make a bet; he asks my advice. If I gave it to him I would rely on the probability calculus, but I could not guarantee success. That is what I would call *subjective probability.* But I suppose that an observer is there, who notes the outcomes over a long period; when he reviews the record he will see that the outcomes fall out in conformity with the probability calculus. That is what I would call objective probability, and it is this phenomenon that we must explicate." [A quote from Poincaré.]
>
> That is a difficulty that leads many into error: how can one not be persuaded—one would ask—that the value of probability is not simply subjective?
>
> In all these cases, in all similar arguments, what is impressive is only one fact: that a practically certain event actually comes about, or it is foreseeable that it will come about. But should we be impressed? When I say that an event is practically certain I mean that I should be amazed if it didn't occur: am I then entitled to be amazed at having guessed, to be amazed that an extraordinary fact whose occurrence would have amazed me did not in fact occur?
>
> Poincaré says that those who are present at the game "will see that the outcomes fall out in accordance with the probability calculus". First of all they would be able to see that *some remarkable and practically certain circumstances* occurred, relative, e.g., to the frequencies, while it is impossible that *all* the practically certain facts have occurred. It suffices to think that it was practically impossible that the particular sequence of outcomes that has taken place would have taken place. Then we must limit attention to just one or a few remarkable and practically certain circumstances. Poincaré says that they will happen. But why does he say it? Because he is certain of it, not absolutely, but practically. And didn't we already have to assume that he was practically certain of it? When I evaluate a probability as very close to 1, I express this sensation: that, almost without doubt, the event will occur. Do I add anything more when I repeat that, almost without doubt, it will occur? Do I have the right to think: first I evaluate a probability, and then I ask myself if I can actually anticipate the event with the corresponding state of mind? This is what many do, and, when they can answer affirmatively, they say that probability has an objective value.
>
> But, when I evaluate a probability, I only express my state of mind, and what does it mean to ask whether I can or cannot have a state of mind which corresponds with the state of mind which is actually mine? If such a doubt corresponds to something which is not meaningless and is actually mine, it was already a part of my state of mind, and I will already have used it in my evaluation of the probability. But once I have evaluated it (and as long as I suppose that my state of mind will not change: if it changes, then certainly I can modify my earlier evaluation!) it is meaningless to think

that my evaluation is wrong, because it is meaningless apart from me, it has no other function than to express my state of mind.

Why, when an event appears to me as practically certain (i.e., when I evaluate its probability as close to 1) have I the right to be practically certain that it will occur? Because when I say that an event is practically certain (when I evaluate its probability as close to 1) I do not say nor can I want to say more or less than this: that I am practically certain it will occur.

If it is true that "*opium facit dormire*", can we think it true that "*opium habet virtutem dormitivam*"? This is no less difficult and no less deep a philosophical problem! I leave it to the reader's acumen to see whether the comparison is apt.

19.

It seems strange that from a subjective concept there follow rules of action that fit practice. And Poincaré keeps explaining why the subjective explanation seems insufficient to him, mentioning practical applications in the field of insurance. "There are many insurance companies that apply the rules of the probability calculus, and they distribute to their shareholders dividends whose objective reality is incontestable."

Basically, this is only the preceding case, simplified by the fact that here it is very clear what the "remarkable circumstance" is that one must consider, and it has a very concrete importance: the dividend. We make a budget in such a way that it is practically certain that the gains will be such-and-such. Naturally, it is meaningless to say "practically certain" if I don't say *for whom* they are so; in this case it will be the managers, the actuaries, the shareholders. When an enterprise is sound and has little risk, it is easy to reach a universal or almost universal consensus on this opinion, and there is nothing to be surprised about, since it is exactly because of this that the enterprise is said to he sound and have little risk.

But this is not sufficient: it is not just a feeling of the managers, actuaries, and shareholders, someone will say. You will see that the dividend will prove them right. What does this mean? I mean that this someone shares the feeling, the persuasion, the faith, which the managers, the actuaries, and the shareholders already have. At the end of the year the dividend is regularly distributed. See that, one will say, that certainty was not just my feeling, it must have had an objective ground. But why? If he—even on the basis of a totally groundless conviction—thought it unlikely that there would be no dividend, he would have to find it very natural that there is the dividend, and it would be pointless, unnecessary, and useless to look for an explanation. Least of all for a purely verbal and abstract explanation, like the one that consists of inventing "chance" and "the laws of chance".

But let us look into the function of the probability calculus in the field of insurance.

Whatever enterprise one wants to undertake, whatever firm one wants to manage, one always proceeds by consciously or unconsciously making a budget, in which we equalize the hope of profits and the fear of losses, the hope and the fear that the profits and the losses will be more or less great. We can love risk more or less, we can be prudent or speculative, and our preferences will be different. We could be guided by the hope of a risky gain and risk everything, or we might prefer the modest tranquility of those who feel safe from the tricks of fortune. We are perfectly free with regard to this choice; everyone can do as he wishes. The probability calculus cannot say we are right

> or wrong: it is true, the concept of mathematical expectation is known, and it is very important, but its task is not (as some seem to think) that of constraining our freedom of choice in this case. The notion of moral expectation has also been introduced, which, besides not solving the problem, is also an artificial and unimportant notion. In any case, we must consider all the alternatives together with their probabilities and their consequences, and then act as we see fit.
>
> In the case of an enterprise that must be secure and have little risk we must act so that, as in the case of insurance, our profits may not be fantastic, but they should be sufficient and practically safe. That's all that non-speculative firms do, without using the probability calculus, and nevertheless this certainty is not too often belied by the facts. And there is nothing strange in this, for an obvious reason: if these forecasts were always belied, we would not make them, and we would act in some other way, and it would be this other way that would inspire us to have greater or less confidence in the various alternatives.
>
> That a fact *is* or *is not* practically certain is an *opinion*, not a *fact*; that *I judge it practically certain* is a *fact*, not an *opinion*. That I should act according to this opinion is only apparently a corollary, because this opinion only exists in that I think I must govern my action in accordance with it.

What of any of this loses force when we come to quantum mechanics, or when we come to Mermin's 1985 *Physics Today* article? I think almost nothing, except possibly de Finetti's rhetorical question:

> If it is true that "*opium facit dormire*", can we think it true that "*opium habet virtutem dormitivam*"? This is no less difficult and no less deep a philosophical problem! I leave it to the reader's acumen to see whether the comparison is apt.

But even that's up to debate, depending upon which part of the quantum confusion one wants to place alongside the analogy. Thus, if you will allow me to drop those three sentences from the passage, I will boldly declare that nothing whatsoever of de Finetti's point changes when we come to quantum mechanics. In particular nothing changes even when we come to EPR/Bell phenomena.

The issue is just one of mindset. I think Rüdiger said it very nicely yesterday:

Schackcosm 70 : *David approaches him and asks "But aren't you dying to find out how the whole thing can possibly work?" Bob is puzzled. He doesn't understand the question. When he finally thinks he understands, he is even more puzzled. "Why would I be more confident about my predictions if the photons carried instruction sets?", he asks. Clearly, Bob and David speak at cross-purposes.*

Apparently, for David the only acceptable way to understand "how it can possibly work" would be a mechanistic, clock-work type model for it. Bob, Bayesian to the bone, thinks that David meant to ask: "How can you possibly be (almost) certain that tomorrow's outcome will be 0?" And Bob's answer is "What difference would the existence of an instruction set make to my beliefs about tomorrow's outcome?"

So, it is not certainty that is a mystery ever. Certainty is just the expression of someone's state of mind, whether the world will bear it out or not. Certainty is, at best, about internal consistency. But, we would have no right to draw from this that there is no deep lesson to be learned from EPR/Bell phenomena. On the contrary, EPR/Bell is just as important as it ever was—it's only been the conclusion that has been misplaced. David drew from it a mystery that has nagged him

(and us) for at least 20 years. Instead, its lesson should be accepted at face value, regardless of the feelings of mystery that motivated Bell's initial analysis: There are no instruction sets. That's the lesson. It is telling us something about (what we believe of) the world.

Wow! What more could one ask for than a precise, well-formulated statement of what one actually believes. Sometimes I have a precise statement of the implicit beliefs that motivate my actions, but not very often. Here's a case where I can actually nail one down.

So, the whole business of understanding quantum mechanics starts to feel even better. It is about understanding one's priors! (I wish I could boom that out in a Ben Schumacheresque fashion! I hope you can hear it.) In accepting quantum mechanics, one is making an implicit statement about one's priors. It is the structure of those priors and not their precise values that is telling us something about what we believe of the world independent of our particular experiences.

In summary, let me just say this. Letting go of the mystery in EPR/Bell is not defeatism. Instead, through it we accumulate a fact for our Bayesian understanding of quantum mechanics, i.e., as a statement about the structure of our priors. There's far more work to be done in that regard. And if one wants metaphysics (or at least a post-positivism, i.e., something more than raw experience), that's where it is to be found.

Thus, let me end Part II. There'll still be a Part III, but now I've got to go to dinner.

30-06-03 *Probabilistic Dialogue* (to R. W. Spekkens)

Spekkensism 1: *I found myself thinking about probability theory last night. I realized I need to really go deeply into these issues. If radical Bayesianism is the answer, then presumably the only thing preventing me from adopting this view is my ignorance of the problems with the alternative.*

So, what should I read to learn about the arguments for and against both the logical and subjective interpretations of probability? If you could tell me the classic texts for both camps, as well as any good articles providing a modern perspective on the debate, that would be ideal. (I'm not interested in the propensity interpretation.)

I'm glad you're taking a deeper interest in the foundational issues of probability now. I think, as your own toy model shows to some extent, getting the idea of probability straight is a large part of the task of getting quantum mechanics straight. In any case, it is a part of the problem that cannot be ignored.

I think the most devastating critique of the classical and/or logical interpretations is the problem of defining the reference class for any given event. What is the set of possibilities that an event *must* be considered an element of? In the real world of statistical practice and decision, there is never a unique answer. Of course, in fundamental physics, one might think that one is in a holier position: One can always declare a reference class by fiat. But then the onus is on the declarer to show why that reference class and a uniform (or even any nonuniform but otherwise fixed) probability measure over it is actually relevant to statistical practice.

I was hoping to pin down some really good quotes over the weekend to accompany this note. You see, for instance, Richard Jeffrey, the inventor of the idea of "radical probabilism" and a wholehearted supporter of de Finettian ideas, started off in the school of Carnap, where the hope was that logical probability would someday be the new messiah. As I understand it, it was years of watching the project fail that ultimately led Jeffrey in the other direction. But I couldn't find anything quite forceful enough for my tastes.

In any case, here are the things I think you ought to read (in the following order):

1. Leonard J. Savage, *The Foundations of Statistics*. Read all of Chapters 1 and 4, especially Section 4.5.

2. Richard Jeffrey, *Subjective Probability (The Real Thing)*. Read pages 8 to 11. The book can be found at: `www.princeton.edu/ bayesway/`

3. José M. Bernardo and Adrian F. M. Smith, *Bayesian Theory*. Read all of Chapter 1 and pages 99 to 102. And finally, the very best thing that anyone can read is

4. Bruno de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989). (I have never gotten so much out of any of my reading on probability as I have from this article. But I should warn you that I can already guess you will find Sections 1, 2, and 3 of it unpalatable, at least on first reading. Therefore I hope you will be a little lenient on the man until you get past them. After that (and excepting the very last section on fascism) I think there is so much that is absolutely firm in the article, I don't see how anyone could disagree with it.)

Let me also add three attachments to the present note that I think are directly relevant to you. One is a little thing Joe Renes wrote recently. Despite the free-formness of the note, I think it hits the points related to your present query quite well. The other two are notes that I wrote recently: They have to do with the "mystery" (or lack thereof) of EPR from the Bayesian standpoint, and lean heavily on the concept of "certainty" in the Bayesian sense. I do hope that they'll somehow make something click in you. The best answers are going to be the most trivial, not the most contrived.

I hope you'll let me know what you think of all these things. In particular, it'd be nice if we could start up a dialogue. Can you articulate what troubles you most about the de Finettian version of probability? What looks less than scientific about it to you?

I suspect it all boils down to the fact that you feel that Bayesianism is somehow too arbitrary: "For God's sake, they won't declare *any* probability assignments right or wrong! Science is not just 'anything goes."' ... but I don't want to put words into your mouth. If however that is roughly the case, I hope you'll think hard about a point Marcus Appleby likes to make: "It's really hard to believe something you don't actually believe." I.e., Bayesians, from my point of view, think anything but 'anything goes.' A probability assignment, when it is made, is one's best shot at articulating what one believes; and what one believes is not up to one's whim.

I would have thought that the de Finetti version of Bayesianism should have been quite attractive from the point of view of the cut you want to introduce into physics: the ontic and the epistemic. Try your best to say what the world does or is; that's what the ontic is about. Fine. I'm OK with that. But then why try to make our fallible beliefs rigidly connected to the ontic world? If you're going to try to make probability assignments adhere to the concepts of right and wrong (just like those ontic states of yours), why not make them part of the ontic world too? Another way to put it: What's so blasted wrong about allowing a perfectly rational agent to be 'wrong' about the actual state of the world? You want a) that there is a world (with its one true state), and b) that any agent worthy of being called rational or ideal, must by right about it to within some tolerance. But why?

Or maybe it is just the fear of having an agent in the background as the anchor for a probability assignment that bothers you. Didn't Copernicus teach us that our place in the universe is not the center of the universe? Aha! Maybe that's it. So, if we could just find a sound notion of objective probability, we could imagine the calculus of quantum mechanics hanging around even when there's no one about in the quad to make use of it. Is that the deeper of the issues for you?

OK, enough blabbering. I'll shut up for a little while until you give me some guidance. In the meantime I'll leave you with some of my favorite Bernardo and Smith quotes. They're pasted below. [See *Quantum States: W.H.A.T.?*, pp. 19–20.]

30-06-03 *Fear of the Anchorman* (to N. D. Mermin & R. Schack)

Did you notice the ending lines I put in the note to Spekkens?

> I suspect it all boils down to the fact that you feel that Bayesianism is somehow too arbitrary: "For God's sake, they won't declare *any* probability assignments right or wrong! Science is not just 'anything goes.'" ... but I don't want to put words into your mouth. If however that is roughly the case, I hope you'll think hard about a point Marcus Appleby likes to make: "It's really hard to believe something you don't actually believe." I.e., Bayesians, from my point of view, think anything but 'anything goes.' A probability assignment, when it is made, is one's best shot at articulating what one believes; and what one believes is not up to one's whim.
>
> I would have thought that the de Finetti version of Bayesianism should have been quite attractive from the point of view of the cut you want to introduce into physics: the ontic and the epistemic. Try your best to say what the world does or is; that's what the ontic is about. Fine. I'm OK with that. But then why try to make our fallible beliefs rigidly connected to the ontic world? If you're going to try to make probability assignments adhere to the concepts of right and wrong (just like those ontic states of yours), why not make them part of the ontic world too? Another way to put it: What's so blasted wrong about allowing a perfectly rational agent to be 'wrong' about the actual state of the world? You want a) that there is a world (with its one true state), and b) that any agent worthy of being called rational or ideal, must by right about it to within some tolerance. But why?
>
> Or maybe it is just the fear of having an agent in the background as the anchor for a probability assignment that bothers you. Didn't Copernicus teach us that our place in the universe is not the center of the universe? Aha! Maybe that's it. So, if we could just find a sound notion of objective probability, we could imagine the calculus of quantum mechanics hanging around even when there's no one about in the quad to make use of it. Is that the deeper of the issues for you?

Now that I've written that, I wonder how much of a problem exactly the latter might be for the larger community. Take, for instance, David Mermin's Desideratum 1 in his original Ithaca interpretation paper: "The theory should describe an objective reality independent of observers and their knowledge." How would he ever fulfill Desideratum 1 if probability must be interpreted in the Bayesian way? Maybe it's that that worries people so much about the Bayesian creed when it comes to quantum mechanics ... even (and particularly) Bayesians!

01-07-03 *Objective Chance* (to W. C. Myrvold)

Myrvoldism 1: *It's several projects down the queue, and we may not get to it this summer, but Bill Harper and I plan to write an article on why Bayesians should believe in objective chance. I'll send you a draft when one exists.*

I look forward to it. It's about the biggest thing on my mind at the moment. Particularly, I'm fairly of the opinion that trying to force objective chance (rather than objective indeterminism) onto the world is going to be counterproductive for a good understanding of quantum mechanics. But I'll give you a chance to convince me! (I made the distinction above, by the way, because I rather like indeterminism of a sort, but I would be reluctant to try to ascribe a numerical measure to it.)

In the mean time, anyway, can you give me some pointers to any literature that tries to argue the same point as you'd like. I'll bank on your paper with Harper being a better version of it all, but still I'd like to see some its predecessors.

01-07-03 *Gasp or Shudder?* (to J. W. Nicholson)

Actually was it gasp or shudder? My memory is getting fuzzy on the whole affair. I know that I need to write this story down before is completely gone. Could I interest you in sending me some notes on what you remember? Of course, when I write it up, I'll probably embellish things a little to try build atmosphere, but I would like to be decently accurate to what actually took place (and how things were said).

01-07-03 *Samizdat and Potentia* (to A. Shimony)

Shimonyism 1: *About two weeks ago I sent you a note thanking you for your book, but I fear that the address was wrong. If you have already received my note of thanks, no harm is done. I certainly appreciated receiving your remarkable book. You gave good excerpts from emails sent to you, and these together with your replies constituted interesting dialogues. As you know, there are serious differences of opinion between us on the ontology of the wave function, but your arguments always seem to me thoughtful. I shall consult your book often and find stimulation in it.*

Thank you for the kind words.

I also reread the lovely letter you sent me on May 27 of last year. I know that there are differences of opinion between us, but I hope that in the end they will not be so serious. In particular it would be nice if physical theory itself would lead the way to minimizing those differences: I think there is a chance, with a suitable rewriting of quantum mechanics into more Bayesian-like terms. At the very least it'll be a new way to see things, and that may sharpen our real (rather than apparent) points of contention.

You write, "[I]n my opinion the greatest philosophical innovation of QM is the discovery and exploration of a new modality of reality – something in between actuality and logical possibility – which Heisenberg named 'potentiality' in his book 'Physics and Philosophy."' But I would like to think that I am quite attracted to the same idea. Throughout *Notes on a Paulian Idea* you'll find image after image of the quantum measurement process as a creative process, something that brings about a transition from the possible to the actual. Indeed it is captured by the Paulian idea itself (the conjunction of the two quotes at the beginning). Here's a slightly longer version of the same:

> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.
>
> Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.

> Like an ultimate fact without any cause, the individual outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case ...

Where we part company, I think, is only in a) my resistance to summing up this new category with a numerical measure (i.e., objective probability or objective chance), and b) my desire to make it clear that the quantum formalism is a calculus for manipulating agent-centered probabilities. Sometimes I put the latter point this way: Can a dog collapse a wave function? Dogs don't use wave functions. I myself didn't collapse a wave function until I was 23. But that doesn't mean that the quantum world will disappear without the agent! It only means that (judgmental, personalistic, subjective) quantum probabilities disappear without the agent.

Here's the way I put it (again) to David Mermin the other day:

> I bank on the idea that we will find that they are not correlata or relata at all, but rather creatia. (Simply trying to ontologize or reify the correlations while giving up on the correlata, as you try with your slogan, won't do.) The world is in constant birth. And to the extent that we focus our actions on anything and ask ourselves, "What will come of them?"—i.e., make note of quantum phenomena—we too are involved in that birth.

Thus, the transition from possible to actual I am toying with is of a sort of William Jamesian flavor (now that I know a little bit about James's philosophy).

Does that leave us with an even more gaping trench between us? Or is there some room for finding a common ground?

Shimony's Reply

> There are some bridges between your Weltanschauung and mine, and there are also some chasms. Among the first are these:
>
> (1) I too am a Bayesian, but in my view of scientific methodology. There are, however, many different versions of Bayesianism – logical probability theory, subjective probability theory, personalist probability theory (similar to subjectivism except for the constraint of consistency), and communalist probability theory. Mine is none of these. The probability theorist whom I most admire is Harold Jeffrey[s], who announces himself as a logical probabilist but modifies that position with what he calls "the simplicity postulate", which is really a strategy. Maybe I should call my rewriting of Jeffreys "strategic Bayesianism." The position is presented at excessive length in "Scientific inference", reprinted in vol. 1 of my *Search for a Naturalistic World View*. An improved and shorter version is in the paper that follows it, called "Reconsiderations on Inductive inference."
>
> (2) I maintain that Bayesian inductive methodology can be employed at the level of generality of metaphysics. It seems to me that Pauli thinks the same, but the metaphysical principles which he derives don't convince me, because of his emphasis on the subject – a residue of Kant. My best expositions of my theses are in the same volume, one called "Search for a world view that can accommodate our knowledge of microphysics," and the other "Reality, causality, and closing the circle."
>
> Another bridge is our common admiration of the pragmatic tradition in American philosophy. You love William James, and I have respect for James but reverence for Charles S. Peirce. If you look at their names in the indices of both volumes of "Search..." you will see my reasons for these attitudes. Something that gave me great

satisfaction when I was Wigner's student was pointing out to him an affinity between some of his philosophical papers and those of Peirce, whose name he had never heard of. He then read some Peirce and was excited: "this man has brains and imagination!". And later he cited Peirce several times. It could happen to you, too. I am not a missionary, but I like to share my enthusiasms with my friends [Maybe that's another way of describing a missionary!].

(3) Although Bayesian probability theory does not, in my opinion, suffice to characterize the probabilities implicit in quantum states, it can be a valuable adjunct. I am now in the middle of writing a Bayesian treatment of the problem of probability in ensembles that are both pre- and post- selected – a problem opened in a famous paper by Aharonov, Bergmann, and Lebowitz, Phys. Rev. **134B**, 1410 (1964), anthologized by Wheeler and Zurek, and treated in many papers by Aharonov and his school. There are, in my opinion, systematic errors in their work, easily resolved by carefully using Bayes's theorem. I'd like to present this work if we have the conference on Bayes and QM that we talked about, but in any case I shall send you the paper when it is done. I've written on the topic before, but am dissatisfied with earlier expositions.

This is a beginning. I hope there will be sequels.

Did I tell you how impressed I was by the beautiful photo of you with your daughter? No philosophy can pretend to depth without the message of that picture!

01-07-03 *Endorsing Probabilismo* (to N. D. Mermin)

I do hope by the way, when you come up for air from the wedding (is it one of your children's?) you'll take a shot at reading de Finetti's article "Probabilism." Here are the coordinates:

Bruno de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989).

The article is immediately followed by one titled "Reading Probabilismo" by Richard Jeffrey. It'd be a good idea to take a look at that one too.

I thought I would send you two things to help prod you into taking this homework assignment seriously. One is an endorsement of the paper by Rüdiger that I noticed he hadn't cc'd to you. The other is a quote from within the paper that sounds ever so much like 'correlation without correlata'. (I know that nothing else so whets your appetite.)

First the quote:

> Concerning Aliotta, I think it necessary to report the following passage, to avoid what might be an easy misunderstanding.
>
> "It is necessary to distinguish relativism from relativism. There is one of its forms (the one commonly pointed to when relativism is accused [of skepticism]) that relegates our knowledge to the realm of relativity, opposing to it an absolute reality that will always elude knowledge. In this form relativism has a skeptical and agnostic flavor and often goes together with mysticism. In the blinding light of the absolute our relative world devaluates, degenerating into a vain apparent shadow. We are the dream, the absolute is reality. And life becomes the painful chase of those shadows, vainly trying to become light.
>
> "But there is another form of relativism (and this is mine), in which what is relative is itself the reality and leaves nothing outside itself. What we know is not the shadow, but the light, not a copy, but the true and concrete original" (*Relativismo e Idealismo*, Naples, 1922, p. 92).

This is exactly my opinion, and I wish to note, for more complete rigor, that the sentence "what is relative *leaves nothing outside itself*" must not be understood as saying that the sentence "there exists something outside what is relative" is FALSE, but that it is meaningless, so that it is impossible even to pose the question as to its truth and falsity. This is, after all, the interpretation that conforms to Aliotta's thought, as appears clearly further along in the text, where "*the being in itself and outside any relation of things*" is seen as "one of the many verbal statements to which there correspond no ideas, and which have become true and proper puzzles of philosophy" (ibid.).

02-07-03 *Photographs and Memories* (to A. Shimony)

Thank you again. I will read your note many times over (as is my habit), and I do hope this is only the beginning.

Shimonyism 2: *Did I tell you how impressed I was by the beautiful photo of you with your daughter? No philosophy can pretend to depth without the message of that picture!*

You flatter me ... but you might also enjoy two letters in the Charlie Bennett chapter, titled "Emma Jane." They start on pages 52 and 53. The second of the two was certainly meant to be a philosophical statement.

03-07-03 *The Dangers of Analogy* (to R. Schack)

Schackcosm 71: *I just discovered that von Mises was right after all! Proof by analogy:*

Einstein: Gravitational mass and inertial mass do not just have the same numerical value—they are the same thing.
Von Mises: Probability and limiting frequency do not just have the same numerical value—they are the same thing.

True enough. But here's one I'm letting guide me ever more often:

$$\text{Hilbert space dimension} \quad \longleftrightarrow \quad \text{gravitational mass}$$

(If the Bekenstein bound is right ... or at least contains a grain of truth ... perhaps it should be mass $\times$ area.)

04-07-03 *Solid Ground, Maybe?* (to G. L. Comer)

Comerism 4: *P.S. When we're together, I want to press you somewhat on how spacetime concepts enter, appear, etc either implicitly or explicitly in your information theoretic program.*

Yep, this is what the note is about. Once upon a time I promised to write you something about the information theoretic roots or NONroots of the principle of equivalence if I ever had any thoughts on it. I think I had one thought. Let me try to get it onto paper.

I go up and I go down when it comes to speaking the words gravity and quantum in the same sentence. At times I find myself thinking that general relativity and quantum mechanics express

two absolutely incompatible worldviews. The general relativistic universe is a "block universe" in William James's sense: It's just there. One can talk about foliations and dynamics, etc., WITHIN the 4-manifold, but in the largest view—the view from nowhere—the world and all its history is just there. It is a universe without life (in the creative sense). In contrast, the quantum world strikes me as a malleable world—one that is still in formation, and in particular, one for which it is impossible to get such a "view from nowhere" (as Nagel would call it).

At other times, I find myself feeling more lenient: Perhaps the two worldviews are incompatible, but that does not mean we cannot gain insight about one of the theories from the other. And when we find the analogy, maybe it is just at that place where we should start hammering away those inconsistencies.

Suppose you take any two pieces of the universe that your mind is willing to call 'matter.' What can *you* tell me with assurance, even if no further word is said about their constitutions? You would tell me that they attract each other, and the force of attraction (in the Newtonian view) is determined in part by two numbers, one intrinsic to each of the two pieces of matter—their masses m and M.

Suppose now that I take any piece of the universe that my mind is willing to label 'matter.' What can I tell you with assurance, even if no further word is said about its constitution? I can tell you that with my free will I can write some number of messages into it. Moreover, I can choose to write them in such a way that that piece of matter will reveal (with some probability) whether anyone else has had a look at my stored message. Both the number of messages I can write into the matter and my best probability of catching an eavesdropper is determined by a single number: The matter's Hilbert space dimension D.

In my paper `quant-ph/0205039` "Quantum Mechanics as Quantum Information (and only a little more)" and in my web samizdat *Quantum States: What the Hell Are They?* (on my homepage), I have argued strenuously that it is ONLY the Hilbert space dimension D that can be taken as a property intrinsic to a quantum system (a piece of matter). The quantum state, nothing to do with entanglement, or even anything to do with a Hamiltonian is intrinsic to it: Just the single, lonely number — the dimension.

It strikes me that we have here a phenomenon of the power and scope of universal gravitation, or maybe the principle of equivalence.

Why did I name this note 'solid ground'? It has to do with something I tried my best to express the last time I was at the Perimeter Institute, where it looks to me like there are so many people flailing about without a clue as to what they should be up to when they speak of combining general relativity with quantum mechanics. Lee Smolin asks me how I could possibly imagine that the linear structure of quantum mechanics will remain when one moves into such a nonlinear regime as that given by the laws of gravity? I say I'm not fazed at all: Most of what he means when he speaks of quantum mechanics as an expression of physics, is for me but a law of thought. A wave function and its evolution are not properties intrinsic to the system for which they are about. Rather, if they are properties of anything at all, they are properties of their user's head—for they capture all his judgments about what might occur if he were to interact with the system of interest.

The quantum foundational task as I see it is to baldly accept that A LARGE PART of the theory is simply not about a world without observers: It is only about our interface with the world. But there is a part that remains, and that part must be given a firm identification. For only once we know how to do that will we know how to move forward when it comes to gravity. Only then will we see that almost all of the ways that have hitherto been considered for combining quantum mechanics with general relativity were far too unconstrained: I am willing to bet that they all essentially boil down to sheer speculation.

So I write this note because I am starting to get a sense that there might be some solid ground in these considerations. Hilbert space dimension is the universal factor for quantum systems that mass is for gravitating systems ... or something like that. Could they be the same thing? ... Or something like that?

"What nonsense is this!??!? Any of the simplest 'real world' quantum systems—not these paltry, specialized things you study in quantum information theory—has an infinite dimensional Hilbert space. Just think of the hydrogen atom!" Maybe. But it's caused me to wonder if I should take the Bekenstein entropy bound more seriously than I have in the past. The trouble that I had had with it before—after making my transition to the subjective/Bayesian/Gibbsian school of entropy—was that a bound on the ignorance one can have just doesn't seem to make much sense: entropy is not an objective property. But we have to be careful in these things: there are levels of subjectivity. What I mean by this is that, though, the cardinality N of a sample space may be subjective in the same sense that a probability assignment is, once one has set it, one is obliged to declare a maximum ignorance, $\log N$, with respect to that setting. And that may be what is really going on the Bekenstein bound.

In other words, maybe in my language, all and only the number ER/h signifies is a Hilbert space dimension. That is one thread of thought, wildly speculative and departing from solid ground though it may be.

But the other thread of thought is in the interpretation of this previous wild speculation. If it's even on the right track, where might it go? Let me give you an example of very bad language: "The Hilbert space dimension signifies the number of potential states a system can inhabit." The lesson of all my (and Bell's, and Mermin's, and many other guys) quantum foundational work is that that is nonsense. Hilbert space dimension signifies something else.

When I get poetic about it, I like to say it signifies something about a system's "sensitivity to the touch", and when I'm getting downright Paulian, I like to say it signifies something about its potential for taking part in creation. With my light touches, I can send it off in ways unknown (to me and to it) and, counter to intuition, the larger it is, the more I can do that. Wojciech Zurek has spent his life making up stories about why big things are "classical." I say he has it all wrong, the bigger the thing, the more Hilbert space it has, the more quantum it is! The more sensitive it is to the touch. And thus the more ignorance an outside observer generally has about it (except with respect to a very small number of features) — not for any particular reason to do with very particular properties of Hamiltonians, but simply because it is big. It is not the system that is classical, but the poor observer's description of it that is.

So, let me leave you with an image for your flight. It's about the fleshpots of creation. Take the Bekenstein bound with more than a grain of salt—I'm not sure that I do yet, but it is fun to play—and take a given region of space. Ask yourself what you should imagine to be there if you want the region to have the most potential for creation. And if you stumble across the answer I'm guessing you will, what on earth could it mean?

PS. Let me append another note I wrote a few months ago to David Mermin [6 January 2003]. Maybe there's a connection in there somewhere.

04-07-03 *Bekenstein Bound Status* (to W. G. Unruh)

Can you fill me in on the latest to do with Bekenstein's entropy bound? Is it still controversial? Have any loopholes been plugged? Is it dead in the water? Things like that. Or can you give me a pointer to something to read on the latest state in the debate?

The reason I ask has to do with a recent *analogy* I've been drawing between (finite) Hilbert

space dimension and mass. Maybe it's taking me down heretical lines ...

Thanks!

07-07-03 *Bekenstein Bound Status, 2* (to W. G. Unruh)

Unruhism 1: *My take has not changed. It is possible that some such entropy to energy bound exists in the real world. It is certainly not necessary and I can imagine theoretical worlds in which it is not true (the simplest is that the entropy goes up as the number of species of say massless particles goes up, so one can always violate any bound by assuming a large enough supply of species). But it is also not needed for saving black hole thermodynamics, which was what he invented it for.*

That is an almost trivial argument, and I was aware of it — heard it from you long ago. If, however, one could argue that (by fiat) in the GR setting, the assumption of a ER/h value is the *assumption* of a QM Hilbert space of the same dimension, then one would have it. Not a logical necessity, but in essence a new postulate. Still, it would have to be motivated from some considerations: So I guess I was asking about something along the lines of your last sentence above. What is the best thing to read in that regard?

08-07-03 *Slogans, Slogans* (to G. L. Comer)

Comerism 5: *Thinking a little about your idea of black holes and the dimensionality of the Hilbert space, how does the dimensionality of the spacetime enter? Thinking classically for the moment, the degrees of freedom of a Schwarzschild black hole can (at least qualitatively) be understood using the quasinormal modes. I imagine that one must sum over more and more l, m etc mode numbers the higher the spacetime dimension gets. If we could 'quantize' those modes, would not the dimension of the Hilbert space change as the spacetime dimension changes?*

Oh, I don't know about any of these things; I hadn't thought about spacetime dimensionality at all.

Mostly it's just my habit of driving research with slogans—you don't know where you're going (or even where you want to go) unless you can make a slogan of it. As I was walking in, I was thinking I could have just as well compacted my last long note into the following little play. Basically I was thinking, wouldn't it be so nice if ...

> Greg: How much Hilbert space do you think this tin can has?
> Chris: I don't know; let's weigh it.

Don't tell me about its construction, its composition, its history; just weigh it.

It's because I've been having outlandish thoughts like this, that I started wondering about this Bekenstein bound again. Maybe it's all crap. (Overwhelmingly likely it's all crap!)

Does Mr. Bekenstein's bound depend upon spacetime dimension? (I'll dig up some papers when you get here.)

By the way, when do you get here?

08-07-03 *The Common Fear?* (to N. D. Mermin & R. W. Spekkens)

The last three days as I've been walking to work, I've been reading a little book on the thoughts of Jürgen Habermas. I thought of both of you (or, actually, my perceptions of both of you) when

I read the following lines. Let me record them.

> Every undergaduate with a unit of behavioural psychology in his or her academic record will have had some experience of the most vulgar example of what Habermas means. In this, as in every other field of postivistic science, we are asked to approach the object world as a disaggregated jumble of discrete objects of perception, as a jumble of 'its'. We are set the task of uncovering the regularities in the behaviour of these atoms of substance by means of an experimental method. The criterion of success lies in the predictive power of the uncovered 'laws' that must produce replicable results. This means results that are *independent* of the author, the inventor, in short of the *thinking subject* who in the first place conceived the problem, the method and the experiment and who thereby created the knowledge. One half of the underlying assumption is that knowledge is always reducible to the totality of discovered properties of the object world. The other half is that the *subject*—the actor, the creator, the knower, the inventor, the scientist—is at worst a pollutant in his own purely objective world, or at best, a ghost in the machine of science and something that must be methodologically controlled and, so far as is possible, eliminated.

10-07-03 *The Anointed Snark* (to N. D. Mermin)

Merminition 111 :

> [CAF wrote:] Importantly though—just trying to keep you on the cutting edge—have you read the de Finetti paper "Probabilismo" in your preparation?

Raced through it, probably too quickly, on a very hard bench in the library, after spending 30 minutes hunting it down. Non circulating journal. Too long to Xerox. Has it been reprinted at anybody's web site? It didn't help me much the first time through. The stuff on correlations without correlata seems to be just a footnote.

What has the four page limit of Physical Review Letters done to our brains? Raced through it?! I tell you that it's the best explication/defense of subjective Bayesianism I have ever seen, and you raced through it? I send you evidence that Rüdiger thinks the same, and you raced through it?!? Rüdiger and I both write detailed letters to show how embracing some of the ideas in it may be just the analgesic your 1985 paper needs, and you raced through it?!?!

Tomorrow when I go in to the office, I will Xerox my copy for you and send it across the ocean. Please don't race through it. (But feel free to give honest, open criticism of anything that doesn't make sense in it. With that, we'll all certainly learn something.)

One question in the meantime concerning this:

Merminition 112 : *Glad it's helped you. Hasn't helped me much yet, but I'm still interested in keeping it up.*

Fair enough. But can you tell me this: Did I, in my note "Utter Rubbish and Internal Consistency, Part I" capture what you think is the essential mystery of the subjective view of the quantum state? If so, then I've at least got the right starting point, and I can try to refine what needs to be said.

PS. Yes, the stuff about correlations without correlata was only a footnote with no further mention. I was just trying to use everything at my disposal to lure you into the paper. One thing

really of significance though: All those philosophers he mentions near that footnote represent the Italian school of (Jamesian) pragmatism. My love affair with James only deepens and deepens. Some mixture of James, de Finetti, Pauli, and modern quantum information are gonna ultimately rule the day on these issues.

10-07-03 *Solipsism* (to N. D. Mermin)

Merminition 113: *Thanks, I'd enjoy having a copy.*

I think my problem is not with the subjective view of probability. Between what I've learned from you and Rüdiger, reading around in Jaynes, Cox's famous paper [which I came upon ten years ago — did I ever tell you that I independently derived and solved precisely his functional equation in my "Relativity Without Light" paper (in boojums)], the stuff I read in the Business Library (!) after first meeting Rüdiger, etc., it all makes a lot of sense.

My problem is, and remains, how it ties in with Quantum Mechanics, which appears (among other things) to tell you how to derive precise probabilities under (apparently) precisely defined — if idealized — circumstances. "Quantum Mechanics Is a Law of Thought" is certainly a good slogan for getting started, and your poetry is very soothing, but I can't help feeling there is still an enormous gap, despite nice things like your quantum de Finetti theorem. Declaring the circumstances also to be subjective judgments appears to be a good (perhaps even necessary) move, but I don't find it convincing and (at best) it seems to lead into some kind of infinite regress in which everything becomes subjective and we're back to solipsism (which is irrefutable and therefore trivial). I haven't been able to articulate what I feel is missing well enough to send it on to you even under the Littlewood-Hardy rules, but I'm still thinking about it.

Rule 1 definitely applies to the above paragraph.

A) What do you mean by solipsism? And, B) how does the "Paulian idea" I sent you still strike you as falling into that category?

11-07-03 *THE Paulian Idea* (to N. D. Mermin)

Merminition 114: *Which "Paulian idea"?*

The one for which there are some notes on:

> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.
>
> Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.
>
> Like an ultimate fact without any cause, the individual outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case ...

Or in more modern language (from 11/4/02):

1) The POVM as a function from raw data to meaning.

We generally write a POVM as an indexed set of operators, E_d. Here is how I would denote the referents of those symbols. The index d should be taken to stand for the raw data that can enter our attention when a quantum measurement is performed. The whole object E_d should be construed as the "meaning" we propose to ascribe to that piece of data when/if it comes to our attention. It is important here to recognize the logical distinction between these two roles. The symbol d stands for something beyond our control, something that enters into us from the world outside our head. The ascription of a particular value d is not up to us, by definition. The *function* E_d, however, is of a completely different flavor. It is set by our history, by our education, by whatever incidental factors that have led us to believe whatever it is that we believe when we walk into the laboratory to elicit some data. That is to say, E_d has much the character of a subjective probability assignment. It is a judgment.

11-07-03 *More Solipsism* (to N. D. Mermin)

Still trying to get your worry straight. By your definition, could a solipsist make changes? (The note below makes sense of this question.) [See 21 July 2001 note, "The Reality of Wives," to A. J. Landahl and J. Preskill.]

11-07-03 *One Final Thing in the Wee Hours* (to N. D. Mermin)

Merminition 115: *Solipsism because when I try to look at things your way I find that whatever I try to condition my subjective probabilities on turns out also to be subjective and conditional on further subjective judgments, ad infinitum.*

Then, for you, does Bayesianism in general equal solipsism (independently of quantum mechanical issues)? For every Bayesian has that infinite regress: They ball it all up into something called "the prior" and ask not where it comes from. (As Savage said it very nicely in the passage Rüdiger recommended you read, the prior might simply be a product of Darwinian evolution.)

Good night! (It's after 3:00 AM for me now.)

11-07-03 *Friendship* (to N. D. Mermin)

Your absorbing this de Finetti paper is important for our discussions. Consider it a present from the slush fund Kiki allows me to dip into once a year (for the really important things). It will arrive at your office in Clark Hall sometime Monday. (FedEx tracking number 8410-8544-4295)

Solipsism indeed!

11-07-03 *One Project Done* (to G. L. Comer)

Well I got one project done that I had promised myself to do before your coming to Dublin (even if I finished no others): I finished reading my copy of *Becoming William James*. I just couldn't bear the thought that you might know something about William James that I didn't know! But let me

apologize to you: I found that I hated the book, and I'm sorry I burdened you with it. The only thing that comes across in it is that WJ and the whole James family is troubled. One gets no feel for the utter greatness that was developing all along at the same time as these (overemphasized) other bits. And the guy was just too full of it with of his own psychological theories, rather than giving us a rounded glimpse of the lives involved. Anyway, you can see it's on the bottom of my list of James biographies now. I wish I had dug you up a copy of Perry's *The Thought and Character of William James* instead. It carries the message I had wanted you to get, where this other book utterly fails.

See you tomorrow.

17-07-03 *Merminism* (to R. Schack)

Just got the note below from David Mermin. Is this man being a smart ass or what? Or is he being serious?

Merminition 116: *Thanks for sending me Probabilism. It does indeed require careful perusal. I shall bring it along to the sea shore next week.*

Meanwhile I note that near the beginning it offers what I would call a dictionary definition of solipsism. I'm also inclined to recommend it to my constructivist sociologist friends who I suspect would find its point of view highly congenial.

17-07-03 *Seven Pines VIII* (to J. Preskill)

Preskillism 4: *Is this a good thing to do?*

I don't know. Probably. In my own case, I'd be interested in talking to Earman, Howard, Milburn, Unruh, and Wald. So it could be worthwhile.

In particular, I'd like to have the chance to field some questions to the GR people ... as the universal characteristics of Hilbert-space dimension have been on my mind, and I find myself wondering to what extent there might be a (conceptual) analogy to gravitational mass and universal gravitation here. Far-fetched in my usual way ... but maybe not completely stupid.

I think it'd be great if you'd be there.

18-07-03 *Solipsism Concerns* (to N. D. Mermin)

Referring to

Merminition 117: *I think my problem is not with the subjective view of probability. Between what I've learned from you and Rüdiger, reading around in Jaynes, Cox's famous paper [which I came upon ten years ago — did I ever tell you that I independently derived and solved precisely his functional equation in my "Relativity Without Light" paper (in boojums)], the stuff I read in the Business Library (!) after first meeting Rüdiger, etc., it all makes a lot of sense.*

My problem is, and remains, how it ties in with Quantum Mechanics, which appears (among other things) to tell you how to derive precise probabilities under (apparently) precisely defined — if idealized — circumstances. "Quantum Mechanics Is a Law of Thought" is certainly a good slogan for getting started, and your poetry is very soothing, but I can't help feeling there is still an enormous gap, despite nice things like your quantum de Finetti theorem. Declaring the circumstances also

to be subjective judgments appears to be a good (perhaps even necessary) move, but I don't find it convincing and (at best) it seems to lead into some kind of infinite regress in which everything becomes subjective and we're back to solipsism (which is irrefutable and therefore trivial). I haven't been able to articulate what I feel is missing well enough to send it on to you even under the Littlewood-Hardy rules, but I'm still thinking about it.

and

Merminition 118 : *Solipsism because when I try to look at things your way I find that whatever I try to condition my subjective probabilities on turns out also to be subjective and conditional on further subjective judgments, ad infinitum.*

and

Merminition 119 : *Thanks for sending me Probabilism. It does indeed require careful perusal. I shall bring it along to the sea shore next week.*

Meanwhile I note that near the beginning it offers what I would call a dictionary definition of solipsism. I'm also inclined to recommend it to my constructivist sociologist friends who I suspect would find its point of view highly congenial.

I guess I am seriously concerned by the charge of solipsism you have made of our program. It is a serious charge. (All one has to do is look into the daily news to see the dangers of it. See the Salon article pasted below about G. W. Bush's shenanigans for a particularly moving example.) Why is it that we fail to communicate on this point?

The world is not what I will it to be. And there is nothing in this view of quantum mechanics that our group is trying to construct that hints of this.

Solipsism would come about if from the quantum formalism one could prove

1. that there are no "instruction sets," and
2. that the outcomes of all interventions (measurements) could actually be controlled by the agent instigating the intervention.

But that is not the case. I was thinking harder about this last night as I was doing a little editing on my CG Fire Series, Vol. II. The frontispiece contains a quote from John Wheeler that I'll paste below because it emphasizes precisely the right point. The radical constructivism or solipsism that you fear is blocked for each and every quantum agent by his own stark admission that the outcomes of his interventions are beyond his control. If they are beyond his control, then there is no need to suppose that they are products of his mind. What more needs to be said?

Let me leave it at that for this round. Rüdiger is hoping to construct a note for you within the next three hours or so—hopefully before you leave for the seashore—making explicit reference to some of the dangers we are starting to perceive in the opening sections of Probabilismo . . . probably precisely the ones that are worrying you. So, stay tuned to your email before leaving for vacation! (You're our most valued customer.)

First the Wheeler quote:

> The Universe can't be Laplacean. It may be higgledy-piggledy. But have hope. Surely someday we will see the necessity of the quantum in its construction. Would you like a little story along this line?
>
> Of course! About what?

About the game of twenty questions. You recall how it goes—one of the after-dinner party sent out of the living room, the others agreeing on a word, the one fated to be a questioner returning and starting his questions. "Is it a living object?" "No." "Is it here on earth?" "Yes." So the questions go from respondent to respondent around the room until at length the word emerges: victory if in twenty tries or less; otherwise, defeat.

Then comes the moment when we are fourth to be sent from the room. We are locked out unbelievably long. On finally being readmitted, we find a smile on everyone's face, sign of a joke or a plot. We innocently start our questions. At first the answers come quickly. Then each question begins to take longer in the answering—strange, when the answer itself is only a simple "yes" or "no." At length, feeling hot on the trail, we ask, "Is the word 'cloud'?" "Yes," comes the reply, and everyone bursts out laughing. When we were out of the room, they explain, they had agreed not to agree in advance on any word at all. Each one around the circle could respond "yes" or "no" as he pleased to whatever question we put to him. But however he replied he had to have a word in mind compatible with his own reply—and with all the replies that went before. No wonder some of those decisions between "yes" and "no" proved so hard!

And the point of your story?

Compare the game in its two versions with physics in its two formulations, classical and quantum. First, we thought the word already existed "out there" as physics once thought that the position and momentum of the electron existed "out there," independent of any act of observation. Second, in actuality the information about the word was brought into being step by step through the questions we raised, as the information about the electron is brought into being, step by step, by the experiments that the observer chooses to make. Third, if we had chosen to ask different questions we would have ended up with a different word—as the experimenter would have ended up with a different story for the doings of the electron if he had measured different quantities or the same quantities in a different order. Fourth, whatever power we had in bringing the particular word "cloud" into being was partial only. A major part of the selection—unknowing selection—lay in the "yes" or "no" replies of the colleagues around the room. Similarly, the experimenter has some substantial influence on what will happen to the electron by the choice of experiments he will do on it; but he knows there is much impredictability about what any given one of his measurements will disclose. Fifth, there was a "rule of the game" that required of every participator that his choice of yes or no should be compatible with *some* word. Similarly, there is a consistency about the observations made in physics. One person must be able to tell another in plain language what he finds and the second person must be able to verify the observation.

— *John Archibald Wheeler*
Frontiers of Time, 1979

And, now from Joe Conason's Journal in *Salon*: "President Bush's Astonishing New Reason for the War with Iraq: Saddam Wouldn't Let Weapons Inspectors In."

July 15, 2003 — A "darn good" quote that almost nobody quoted "We gave him a chance to allow the inspectors in, and he wouldn't let them in."

George W. Bush uttered that amazing sentence yesterday to justify the war in Iraq, according to the Washington Post.

What? Yes, I promise that's what the man said. (And by "him," the president clearly meant Saddam Hussein – not Kim Jong Il, who actually has refused to let international inspectors into North Korea.)

Now a presidential statement so frontally at variance with the universally acknowledged facts obviously presents a problem for the White House press corps. He wasn't joking, and he didn't sound disoriented or unwell. Although Dana Priest and Dana Milbank wrote the story as delicately as they possibly could, they couldn't make it seem less weird:

"The president's assertion that the war began because Iraq did not admit inspectors appeared to contradict the events leading up to war this spring: Hussein had, in fact, admitted the inspectors and Bush had opposed extending their work because he did not believe them effective."

Appeared to contradict the events leading up to war? Indeed, that's an exceedingly mild description of what Bush said. There's no plausible explanation, unless the president suddenly flashed back to his Yale sophomore philosophy seminar, grappling with the argument that everything we perceive is mere illusion.

For the moment, however, let's just assume reality does exist. What possessed the president to make an assertion that everyone on the planet knows to be untrue? ...

18-07-03 *Most In One Place* (to N. D. Mermin & R. Schack)

As I've told both of you, I'm starting to put together another samizdat—this one to contain the last year's discussions. Let me drop it off with you as it stands at the moment. Even though it's not complete you might find it a little useful. I think it already contains all of the emails from my side in the latest round of discussions ... if that's useful.

Parts of it are certainly diatribic ... and needlessly so. But on the other hand, there are some notes—the note of 10 January 2003, "Filth Under the Rug," comes to mind—that I think express a truly heartfelt frustration.

David is right: "'Quantum Mechanics Is a Law of Thought' is certainly a good slogan for getting started, and your poetry is very soothing, but I can't help feeling there is still an enormous gap, despite nice things like your quantum de Finetti theorem." There is so very much that needs to be done by way of technical work. But that should not stop anyone from seeing that we are moving absolutely in the right direction. Just precisely what is it that is blocking the vision?

19-07-03 *Definitions from Britannica* (to N. D. Mermin)

Merminition 120: *As far as I know solipsism is the claim that there is nothing more than my own sense impressions. It does not imply that I can control what those sense impressions are. They could be like a film I am doomed to keep watching — or an uncontrollable dream.*

I believe that's the correct use of the term, but if I'm wrong, give my "serious charge" another name. Perhaps this clarification makes it less serious? Hope not.

I looked up several definitions for the heck of it. The first set below comes from the 2001 *Encyclopedia Britannica*. Following that, I place my search results from several dictionaries for the word solipsism in particular. I guess the definition from *Wikipedia* best captures what I thought the word meant.

In any case, regardless of the labels, David's biggest worry seems to be this:

Merminition 121 : *A) Solipsism because when I try to look at things your way I find that whatever I try to condition my subjective probabilities on turns out also to be subjective and conditional on further subjective judgments, ad infinitum.*

Let me try to tackle this one directly, now that maybe I understand your worries better and after having talked to Rüdiger yesterday. Take a good solid physicist like Steven Weinberg who stakes his career on the search for a grand unified field theory. Suppose he finds it. To find it (I presume) is to declare: The world's Lagrangian is L. Now suppose I were to ask Mr. Weinberg, "Why L? Why not M?" I know for sure his answer will be of the form, "L just is. It is the starting point. It is an ultimate fact of nature; it calls for no explanation. In any case, if it calls for an explanation, its answer must come from outside the realm of science—religion? theology?—but I see no reason to go to such lengths."

Would that make Weinberg a solipsist? A sensationalist? A phenomenalist? The point is Weinberg's stance has nothing to do with any of these labels.

Similarly for the Bayesian (even of the de Finettian variety, despite the mumbo jumbo in the opening sections of Probabilismo). For him, "the prior" on any event space is treated as an ultimate fact—an ultimate fact about the agent. There is no infinite regress because, just as with Weinberg, ultimate facts call for no further explanation.

Bayesian practice—and Rüdiger and I would claim the formal structure of quantum mechanics too—is all about what to do once a prior is established. It is not about what to do before the prior is established. In the quantum mechanical case, establishing "a prior" is 1) to write down a quantum state for all systems considered, and 2) to write down a (conditional) quantum operation for all measuring devices considered.

If there are two agents, there may well be two priors in the sense above —i.e., two ultimate facts (with respect to this level of inquiry). In that sense, the priors are "subjective", but that does not take away their status as ultimate facts in this treatment. It only calls for a recognition that the facts are about the agents.

The role of the separate system and measuring device—now specializing on quantum mechanics—is that when the two are combined they give "birth" to a new ultimate fact: The "click." There is no sense, however, in which this new ultimate fact is *about* either of the agents: It has a life of its own. (In fact, it is because of these lines of thought that I sometimes call my view "the sexual interpretation of quantum mechanics.")

Does this clarify anything? Does this in any way address your fears of solipsism/sensationalism/phenomenalism?

Definitions below.

> **Solipsism:**
>
> in philosophy, formerly, moral egoism (as used in the writings of Immanuel Kant), but now, in an epistemological sense, the extreme form of subjective idealism that denies that the human mind has any valid ground for believing in the existence of anything but itself. The British idealist F. H. Bradley, in *Appearance and Reality* (1897), characterized the solipsistic view as follows:
>
> "I cannot transcend experience, and experience is my experience. From this it follows that nothing beyond myself exists; for what is experience is its (the self's) states."
>
> Presented as a solution of the problem of explaining human knowledge of the external world, it is generally regarded as a reductio ad absurdum. The only scholar who seems to have been a coherent radical solipsist is Claude Brunet, a 17th-century French physician.
>
> **Subjective Idealism:**

a philosophy based on the premise that nothing exists except minds and spirits and their perceptions or ideas. A person experiences material things, but their existence is not independent of the perceiving mind; material things are thus mere perceptions. The reality of the outside world is contingent on a knower. The 18th-century Anglo-Irish philosopher George Berkeley succinctly formulated his fundamental proposition thus: Esse est percipi ("To be is to be perceived"). In its more extreme forms, subjective idealism tends toward solipsism, which holds that I alone exist.

Sensationalism:

in epistemology and psychology, a form of Empiricism that limits experience as a source of knowledge to sensation or sense perceptions. Sensationalism is a consequence of the notion of the mind as a tabula rasa, or "clean slate." In ancient Greek philosophy, the Cyrenaics, proponents of a pleasure ethic, subscribed unreservedly to a sensationalist doctrine. The medieval Scholastics' maxim that "there is nothing in the mind but what was previously in the senses" must be understood with Aristotelian reservations that sense data are converted into concepts. The Empiricism of the 17th century, however—exemplified by Pierre Gassendi, a French neo-Epicurean, and by the Englishmen Thomas Hobbes and John Locke—put a greater emphasis on the role of the senses, in reaction against the followers of René Descartes who stressed the mind's faculty of reasoning. Locke's influence on 18th-century French philosophy produced the extreme sensationnisme (or, less often, sensualisme) of Étienne Bonnot de Condillac, who contended that "all our faculties come from the senses or more precisely, from sensations"; that "our sensations are not the very qualities of objects [but] only modifications of our soul"; and that attention is only the sensation's occupancy of the mind, memory the retention of sensation, and comparison a twofold attention.

Phenomenalism:

a philosophical theory of perception and the external world. Its essential tenet is that propositions about material objects are reducible to propositions about actual and possible sensations, or sense data, or appearances. According to the phenomenalists, a material object is not a mysterious something "behind" the appearances that people experience in sensation. If it were, the material world would be unknowable; indeed, the term matter itself would be unintelligible unless it somehow could be defined by reference to sense experiences. In speaking about a material object, then, reference must be made to a very large group or system of many different possibilities of sensation. Whether actualized or not, these possibilities continue during a certain period of time. When the object is observed, some of these possibilities are actualized, though not all of them. So long as the material object is unobserved, none of them is actualized. In this way, the phenomenalist claims, an "empirical cash value" can be given to the concept of matter by analyzing it in terms of sensations.

Positivism:

in philosophy, generally, any system that confines itself to the data of experience and excludes a priori or metaphysical speculations. More narrowly, the term designates the thought of the French philosopher Auguste Comte (1798–1857). The basic affirmations of Positivism are (1) that all knowledge regarding matters of fact is based on the "positive" data of experience, and (2) that beyond the realm of fact is that of pure logic and pure mathematics, which were already recognized by the Scottish Empiricist and Skeptic David Hume as concerned with the "relations of ideas" and, in a

later phase of Positivism, were classified as purely formal sciences. On the negative and critical side, the Positivists became noted for their repudiation of metaphysics; i.e., of speculation regarding the nature of reality that radically goes beyond any possible evidence that could either support or refute such "transcendent" knowledge claims. In its basic ideological posture, Positivism is thus worldly, secular, antitheological, and antimetaphysical. Strict adherence to the testimony of observation and experience is the all-important imperative of the Positivists. This imperative is reflected also in their contributions to ethics and moral philosophy, and most Positivists have been Utilitarians to the extent that something like "the greatest happiness for the greatest number of people" was their ethical maxim. It is notable, in this connection, that Auguste Comte was the founder of a short-lived religion, in which the object of worship was not the deity of the monotheistic faiths but humanity.

Further definitions of Solipsism:

- **Merriam-Webster Online Dictionary:** a theory holding that the self can know nothing but its own modifications and that the self is the only existent thing.
- **Cambridge International Dictionary:** the belief that only one's own experiences and existence can be known with certainty.
- **American Heritage Dictionary:** 1. The theory that the self is the only thing that can be known and verified. 2. The theory or view that the self is the only reality.
- **Encyclopedia Britannica, 1911 edition:** a philosophical term, applied to an extreme form of subjective idealism which denies that the human mind has any valid ground for believing in the existence of anything but itself. It may best be defined, perhaps, as the doctrine that all 'existence is experience, and that there is only one experient. The Solipsist thinks that he is the one!" (Schiller). It is presented as a solution of the problem of explaining the nature of our knowledge of the external world. We cannot know things-in-themselves: they exist for us only in our cognition of them, through the medium of sense-given data. In. F. H. Bradley's words (Appearance and Reality):

  "I cannot transcend experience, and experience is my experience. From this it follows that nothing beyond myself exists; for what is experience is its (the self's) states."
- **Wikipedia:** Solipsism is a metaphysical belief that one is like a God, creating the reality in which one exists. Solipsism is logically coherent, but not falsifiable, so it cannot be established by current modes of the scientific method.

  The classic objection to solipsism is that people die. However, you have not died, and therefore you have not disproved it.

  A further objection is that life causes pain. Why would we create pain for ourselves? There may be some reason which we have decided to forget, such as the law of Karma, or a desire not to be bored.

  Solipsism is a common theme in eastern philosophy. Various interpretations of Buddhism, especially Zen, teach that the entire universe exists only in one's mind.

24-07-03 *Subjective and Objective, Precursor* (to A. Sudbery)

I promised to say something a little more scientific this afternoon, rather than administrative. But time slipped away, and I ended up doing some mathematics with Chris King at the chalkboard. Consequently, I didn't get Nagel's article finished, which I started on the train yesterday.

But let me build up to a discussion on it (and then after it, on your paper 'Why Am I Me?' in the coming weeks). Mostly, I just want to say that, so far, it's not looking so good for Mr. Nagel in my eyes: I'm finding that I'm not liking the paper as much as I was hoping to. Of course, I could have guessed that I would get tangled in a sparring match with it ... but I didn't think I would get tangled in disagreement even with the motivation it sets out in the first couple of pages.

Let me try to show you why. Attached is an excerpt, relevant to the present subject, from my smaller samizdat "Quantum States: What the Hell Are They?" posted on my webpage. [See notes to Wiseman starting on pages 210 and 217 respectively.] I hope it shows why I would be resistant to an 'external view' at the very outset. I will certainly read Nagel thoroughly before this is all over with, but already on the very first pages it looks that we come from very different worlds.

More eventually ...

25-07-03 *Agendas and Rubrics* (to R. W. Spekkens)

Spekkensism 2: *If it stands, I hope that this result may serve as another clue in the mystery, another aid in determining the nature of the conceptual innovation that needs to occur before one can devise a proper realist interpretation of quantum theory, if such an interpretation can be had.*

What a novel idea! I like the technical part.

Question: What does it take to be a proper realist interpretation of quantum theory?

I never did finish that note on black holes to you; I'll paste the point I got up to in it below. The only point I wanted to make (but I wanted to do it in style!) was that, in my thinking about quantum mechanics, Hilbert-space dimension plays a conceptual role that is a bit like black-hole mass. I.e., Hilbert-space dimension is the ontic state of a quantum system.

Now, why wouldn't that fall under the rubric of "proper realist interpretation" in your eyes? I'm guessing that it won't. In particular, I'm guessing that it has something to do with the "Common Fear?" I wrote you and Mermin about a couple of weeks ago. But, I don't know, you tell me.

— — — — — — — — — —

From just before the IBM meeting:

I spent a little while digging around in my email archive this morning and was finally able to pull out this piece from June 7, 1991. It was in a letter to Greg Comer.

> The other night I had a dream in which I was trying to calculate how many black holes would fit in my ice chest. I could already see one in there and I was trying to figure out how many more would fit. They're really beautiful, you know—contrasting against the white of the ice chest and the glistening of the ice. So, I pulled out another beer and watched for a while.

Consider a Schwarzschild black hole, to the exclusion of all else in the universe. What is the black hole's ontic state?

There is only one thing to pin it on: It is the black hole's mass, M. Do you balk at that? Does the starkness of the black hole's characterization specify a dearth or emptiness of phenomena

associated with this lovely physical object? Not at all, once the black hole is embedded within the wider context of general relativity, the orbits of various test bodies, and so forth.

Especially in light of the immense physics that comes from such a single, simple number M when ... [Never finished.]

25-07-03 *Short Replies* (to G. L. Comer)

Comerism 6: *I like that word "interlocutor."*

I'm not so taken with it. It's probably because I'm not so taken with the "asking questions of nature and getting answers back" imagery any more. I'm more taken with the imagery of "push on nature and see how it reacts." We never ask of nature, we only push on it—or at least that's my take at the moment. (It's the reason I'm calling my CG Fire Series, Vol. II, project "The Activating Observer".) But like my philosophy, I can be malleable.

25-07-03 *No, Thank You!* (to J. Honner)

Thanks for the nice compliments/complements. Why did you leave academia? Not only did I enjoy your Bohr/Derrida article, but many years before that, I enjoyed your book on Bohr—reading it was one of my formative experiences in the field. I'd love to get hold of it again, as now in working on my "The Activating Observer: Resource Material for a Paulian/Wheelerish Conception of Nature" project (partially posted at my webpage) I'm going through many old things once again, but with my own fine-toothed comb. (Though, I guess I have to admit, I did make a bit of fun of your book and its genre in my paper `http://www.arxiv.org/abs/quant-ph/0104088`. You can see what I mean in endnote 5 and the text leading to it.)

25-07-03 *Relative Onticity* (to R. Schack)

There are two points I like in your last note:

Schackcosm 72: *What makes all this exciting for me is the fact that there appears to be a straightforward logical argument from your "Is the Moon There?" article to de Finetti's position. Particles don't carry instruction sets. Hence $|x\rangle$ is subjective.*

and

Schackcosm 73: *I believe that you get an infinite regress only if you try to find the objective ground behind your probability assignments. This is futile. Your infinite regress argument is an argument FOR the subjective viewpoint. The latter is not solipsism, but simply the realization that our description in terms of maths is not the same thing as the external reality.*

Let's see what kind of impact they'll have on Mr. Mermin now.

I thought a little more about our phone conversation on my walk to work this morning—in particular, your point about, "Why not let Mr. Caves (under the appropriate conditions) act AS IF his Hamiltonians are real? What's to be lost by it?" I think the thing to be lost is our hard won category distinction between A) the probability function $P(x)$ and B) the values x of its argument. I.e., the beliefs and the facts. Even when the Bayesian gives an "as if" theorem, like with the de Finetti theorem, he never drops this category distinction. I.e., he does not confuse the data he is

gathering (which with the assumption of exchangeability will allow him to asymptotically settle on a product probability distribution) with the updated belief.

At best, the "as if" theorems can give us something like the new semi-ontic category that Shimony seeks: the potentia. That is to say, there are "actualities" represented by the CLICKS, but then there are the "potentia" represented by the probabilities of which we can act AS IF they are objective.

I continue to think that it is of ultimate importance for this program to not let Mr. Caves go only halfway. Where we are heading, I think, is toward a wholly new kind of ontology—despite what looks to me like your present attraction to sensationalism. For that task, we just shouldn't stop halfway. I see us discovering and finding convincing explanation for the idea that the world is truly on the make. To stop with the "as ifs" that we can all agree upon (presumably after inculcation by the right physics community)—let us say they really are Hamiltonians—strikes me overpoweringly as an effort to once again introduce an overriding stasis to the universe. The real, unchanging ground of the universe is *the* Hamiltonian, full stop. And what is a Hamiltonian? In effect, it is the potentia, or even a propensity. (I.e., it is the generator of a conditional probability which has been given an objective status by fiat.)

Here's maybe another way of putting what I fear. If we stop with identifying the conditions under which one can act AS IF, then at best we will draw a new picture of existing quantum mechanics. We will give new names to old prejudices. It is simply not radical enough for my tastes; when we solve the old problem of quantum interpretations, we must get something new. At the very least a new world view, but I suspect something much more substantial and technical than that.

But, anyway, why am I any better off with my Hilbert-space dimension? Surely there is a sense in which it too is a subjective judgment. True enough. But then, why do I not see myself as falling into the same trap as Mr. Caves? I want to claim that in opposition to the Hamiltonian which is on the A) side of the category distinction above, Hilbert-space dimension should be more rightly thought of as on the B) side. Hilbert-space dimension should be thought of as taking the role of cardinality of the sample space in classical probability theory. And as you and I have already discussed, the setting of a sample space (classically) is certainly a subjective judgment. But nevertheless the elements within the sample space play the role of (potential) FACTS, not BELIEFS, after the setting is made. So, it is a relative onticity of a sort, but that is all I have ever been trying to capture with my "objective with respect to the theory" (or whatever phrase I used to use).

Enough. I'll say more when I can say more. Whereof one cannot speak, thereof one must be silent ...

28-07-03 *Understanding Buridan* (to R. Schack)

Schackcosm 74: *I think this is usually quoted in discussions about free will versus determinism. I like it because it illustrates the absurdity of indeterminism.*

Does this mean that deep underneath it all you're a determinist who believes in instruction sets? This worries me a bit.

28-07-03 *New Twenty Questions* (to R. Schack)

Schackcosm 75: *Presently I am very much under the antirealist spell of de Finetti. The way I understand de Finetti, indeterminism is just as meaningless as determinism. But maybe I am*

taking the first three paragraphs of Probabilismo too seriously...

The lines below are from my paper `quant-ph/0204146`. Part of them, even I am in disagreement with now. But the part that gives a definition of indeterminism, I am still OK with. Are you OK with that, or do you still contend that it is meaningless?

> In choosing one experiment over another, I choose one context over another. The experiment elicits the world to do something. To say that the world is indeterministic means simply that I cannot predict with certainty what it will do in response to my action. Instead, I say what I can in the form of a probability assignment. My probability assignment comes about from the information available to me (how the system reacted in other contexts, etc., etc.). Similarly for you, even though your information may not be the same as mine. The OBJECTIVE content of the probability assignment comes from the fact that *no one* can make *tighter* predictions for the outcomes of experiments than specified by the quantum mechanical laws. Or to say it still another way, it is the very existence of transformation *rules* from one context to another that expresses an objective content for the theory. Those rules apply to me as well as to you, even though our probability assignments *within* each context may be completely different (because they are subjective). But, if one of us follows the proper transformation rules—the quantum rules—for going to one context from another, while the other of us does not, then one of us will be able to take advantage of the other in a gambling match. The one of us that ignores the structure of the world will be bitten by it!

28-07-03 *New Twenty Questions, 2* (to R. Schack)

Different question, but again referring to this:

Schackcosm 76 : *Presently I am very much under the antirealist spell of de Finetti. The way I understand de Finetti, indeterminism is just as meaningless as determinism. But maybe I am taking the first three paragraphs of Probabilismo too seriously...*

What do you say about Mermin's anti-instruction-set derivation? Is that now contentless (at worst) or superfluous (at best) from this anti-realist view?

28-07-03 *Your Newest Turn* (to R. Schack)

I did truly get into quite a bit of trouble tonight, not getting home until 7:30. In general it's just been a very bad day for me.

Let me give you an example of something I don't think you can allow yourself to say in your newest turn.

When I wrote my ill-fated note "Got It!," I wrote in Section 4 of it:

> 4) POVMs and radical pluralism.
>
> Now let me go into a bit of the metaphysics of this. Here's a point of view that I'm finding myself more and more attracted to lately.
>
> I think it is safe to say that the following idea is pretty commonplace in quantum mechanical practice. Suppose I measure a single POVM twice—maybe on the same system or two different systems, I don't care—and just happen to get the same outcome

in both cases. Namely, a single operator E_d. The common idea, and one I've held onto for years, is that there is an objective sense in which those two events are identical copies of each other. They are like identical atoms ... or something like the spacetime equivalent of atoms. But now I think we have no warrant to think that. Rather, I would say the two outcomes are identical only because we have (subjectively) chosen to ignore almost all of their structure.

That is to say, I now count myself not so far from the opinion of Ulfbeck and Bohr, when they write:

> The click ... is seen to be an event entirely beyond law. ... [I]t is a unique event that never repeats ... The uniqueness of the click, as an integral part of genuine fortuitousness, refers to the click in its entirety .... [T]he very occurrence of laws governing the clicks is contingent on a lowered resolution.

For though I have made a logical distinction between the role of the d's and the E_d's above, one should not forget the very theory-ladenness of the set of possible d's. What I think is going on here is that it takes (a lot of) theory to get us to even recognize the raw data, much less ascribe it some meaning. In Marcus Appleby's terms, all that stuff resides in the "primitive theory" (or perhaps some extension of it), which is a level well below quantum mechanics. What quantum mechanics is about is a little froth on the top of a much deeper sea. Once that deeper sea is set, then it makes sense to make a distinction between the inside and the outside of the agent—i.e., the subjective and the objective—as we did above. For even in this froth on the top of a deeper sea, we still find things we cannot control once our basic beliefs—i.e., our theory—are set.

Without the potential d's we could not even speak of the possibility of experiment. Yet like the cardinality of the set of colors in the rainbow—Newton said seven, Aristotle said three or four—a subjective judgment had to be made (within the wide community) before we could get to that level. If this is so, then it should not strike us as so strange that the raw data d in our quantum mechanical experience will ultimately be ascribed with a meaning E_d that is subjectively given. (I expressed some of this a little better in a note I wrote to David last month; I'll place it below as a supplement.) More particularly, with respect to the EPR example above, it should not strike us as odd that the phenomenon comes about solely because of an interpretive convention we set: All quantum measurement outcomes are unique and incomparable at the ontic level. At least that's the idea I'm toying with.

to which you replied at the time:

Schackcosm 77: *Section 4), I like. It highlights the chasm that exists between our approach and the many-worlders and decoherence people. We all agree that a click is not an elementary phenomenon. They want to reduce it to something more fundamental. We say it's irreducible (which does not mean that we cannot analyze a measurement apparatus or decoherence of a quantum register in as much depth and detail as anybody else). Your metaphysical bit is nice in that it makes clear that "irreducible" is not the same thing as "elementary". As you say it,*

> *All quantum measurement outcomes are unique and incomparable at the ontic level.*

In your newest turn, such discussion would be "meaningless"—correct? You would no longer allow yourself to contemplate what the CLICK d might be in its own essence. You would no longer say that "successive clicks with a single value d are truly the same," nor would you contemplate that "successive clicks with a single value d may actually be truly different."

That is to say, in this new philosophy you are toying with, if something (i.e., an event, a fact, etc.) is not a "hook" upon which a probability can be conditioned, you are not willing to speak of it. It is meaningless—I believe you say. You don't even let yourself conjecture about the stuff that is out there independently of us. (Or at least I don't see how you're going to be able to do this with such a strongly positivistic line.)

As I tried to express today, and as I'm trying harder to articulate now, I guess I don't like that.

Pragmatists are not positivists, but more opportunists. James allowed personal religions to be parts of reality, and I guess I'm inclined to that.

The part of de Finetti's introductory section that I do like is his organicist take on scientific theories. I'm inclined to view the PRESENT scientific theory of any type as part of the specification of our species. Such theories are part of the account of our being at the moment; they express our possibilities and our limitations. By this account, for instance, Hamilton and Lagrange belonged to ever slightly different species than you and me. Silly, huh? I don't think so: For what's important about this is that one sees by it that a theory (and the entities within it) are every bit as real or unreal as biological species. Whatever they are, they have a temporary hold on our description of things that cannot be denied. Are they all and only AS IF statements?

I guess I don't think so. In other words, I guess I'm saying that I think you're going too far with your ASIFism. As far as I can see, all a representation theorem (say of the de Finetti type) can give us is the conditions under which we can act AS IF our impredictability is coming from a true but unknown PROPENSITY. In particular, what the AS IF theorems do not give are the sample spaces. They are always set before these representation theorems can be posed at all. And, in that way, I say sample spaces obtain a status—though subjective in the character of how they are set—that is somehow different from what a representation theorem can give.

My goal continues to be ultimately realist in tone. All I have ever wanted to do in these last couple of years is strip away the objective character of all probability statements and any part of quantum mechanics that smells of being a probability statement in disguise. I have never imagined (nor do I think we have any warrant from anything technical within probability theory) that the whole structure of QM will go up in smoke in the process.

By the way—on a slightly different subject—let me readdress one of the points in your Buridan's Ass note again:

Schackcosm 78 : *Well, Buridan's ass stands in front of two buckets of hay, neither of which looks to him any better than the other. Hence the poor ass remains standing there and eventually dies. I think this is usually quoted in discussions about free will versus determinism. I like it because it illustrates the absurdity of indeterminism. The state* $|R\rangle + |L\rangle$ *is symmetric with respect to left and right. If nature really IS in a symmetric state with respect to left and right, then what breaks the symmetry? If it is our state of belief, however, that is symmetric, there is no problem. The ass chooses one or the other, it's me who has no clue which one it will be. Either one or the other detector goes click, it's me who has no clue which one it will be.*

I would say the very lesson of Bell and Kochen-Specker is that we cannot (or rather should not) act AS IF nature is not at a juncture in the quantum measurement setting. "Unperformed Measurements Have No Outcomes."

30-07-03 *Convictions, Courage, Clear Thinking* (to R. Schack)

Your note was so chock-full, I had quite a sleepless night. How I wish I had time to reply to you right now in great detail—it is something I need internally, there is no better therapy for me than composing a note—but it is going to have to wait most likely until I am in Germany.

I think you were right on most (or at least many) counts. In particular, I think this discussion represents a lack of courage on my part, along with a small lack of clear thinking: The former likely caused the latter. Funny how many times I have accused Carl (and sometimes you) for not having the courage of your convictions. Stones and glass houses.

Also there is a slight problem of emphasis and particular choices of words that might be getting in our way—i.e., that may be one of the things that helped shunt us from complete and immediate agreement. These things need fleshing out.

Mostly I want to say for the moment that your note inspired me, and I think (maybe unseen to you) it gives me a way to have my cake and eat it too.

I will be back with a vengeance in a few days (for hopefully an adequate mix of email and verbal communication) ...

In the meantime, though, would you think about the tension in these three statements of yours:

Schackcosm 79 : *Well, Buridan's ass stands in front of two buckets of hay, neither of which looks to him any better than the other. Hence the poor ass remains standing there and eventually dies. I think this is usually quoted in discussions about free will versus determinism. I like it because it illustrates the absurdity of indeterminism. The state* $|R\rangle + |L\rangle$ *is symmetric with respect to left and right. If nature really IS in a symmetric state with respect to left and right, then what breaks the symmetry? If it is our state of belief, however, that is symmetric, there is no problem. The ass chooses one or the other, it's me who has no clue which one it will be. Either one or the other detector goes click, it's me who has no clue which one it will be.*

and

Schackcosm 80 : *NOOOOOOO! You are joking, aren't you? The direct lesson of Bell and Kochen-Specker is indeed that we should not act AS IF unperformed measurement had outcomes. To conclude that nature is at a juncture is adding a lot of baggage, however. I am sure de Finetti would be rotating in his grave if he read this!*

and

Schackcosm 81 : *What de Finetti's anti-realism attacks is the prejudice that to the terms of our description literally corresponds some real stuff out there in nature. Isn't it ridiculous, he asks, to conclude that nature is in the situation of Buridan's ass, only because we aren't certain about what will happen next? Does that mean de Finetti believes in instruction sets? I think this would do him injustice.*

I'm with you on the middle one at the moment. But the three together sure look like a tense mix to me. What actually would do de Finetti justice on this issue? It is not clear to me at all.

There's a lovely passage in an 1872 diary entry of William James: I wish I had it here to get the exact wording. It was something of the order, "my first act of free will shall be to believe in free will ... I will give it a shot for one year and see where that gets me."

01-08-03 *Mortified, Yes* (to N. D. Mermin)

Merminition 122 : *SHPMP Sept 2003 just arrived.*

Well I knew you hated Copenhagen Computation*, but making it the one article in the collection that you don't say a word about in the Introduction seems a bit much, particularly since your theme is the vindication of Bohr over Einstein. Putting it last underlines the omission. It will be clear*

to readers that I foisted it on you. (As I remember I didn't want to send in anything, but you kept insisting. . .)

I love you anyway. Actually I assume it was an accident — Freudian, of course — and trust that you are properly mortified.

I very, very, very much apologize. I feel like utter crap. How I could let that happen I do not know. Jeff wrote the first draft, and I jiggled some things in it, but that I did not notice the omission is inexcusable.

All I can say now is that I apologize, and I've got to find a way to make it up to you in some other aspect of life.

It is not true that I did not like your paper. I liked it a lot, and I especially liked your presentation at the Bennettfest. It seems to me that you get to the essential point when you point out the non-problematicity of the ultimate readoff: it is no more or less mysterious than reading off of a piece of information from a computer screen. Either they are both deep problems, or neither of them are. Showing that that is the best way to teach quantum computing to CS people makes a deep statement . . . and you're in tune with it.

Please do forgive me. It wasn't intentional, and it wasn't even Freudian.

12-08-03 *Me, Me, Me* (to N. D. Mermin & R. Schack)

Me, me, me; it's always about me! —Yes. But nonetheless it is simply not solipsism. Let me explain.

I guess I was actually fortunate today: For the second time in a month, I was called a solipsist by one of my friends. (This time the accuser was Howard Wiseman.) On top of that, Asher Peres gave a talk this morning that made me cringe, saying things like, "When no one performs a measurement, nothing happens [in the world]." The combination of these two bad experiences caused me to wander the streets of Aarhus this afternoon in spite of the horrible heat. I suppose I needed to find a way to sweat the poisons from my body.

The fortune in this is that it caused me once again to strive for a clearer and more consistent form of expression. I want to try to capture some of that in this note. Mostly it is about not allowing oneself to get hung up in someone else's (inconsistent) expectations for what quantum theory ought to be.

In our 2000 opinion piece in Physics Today, Asher and I wrote:

> The thread common to all the nonstandard "interpretations" is the desire to create a new theory with features that correspond to some reality independent of our potential experiments. But, trying to fulfill a classical worldview by encumbering quantum mechanics with hidden variables, multiple worlds, consistency rules, or spontaneous collapse, without any improvement in its predictive power, only gives the illusion of a better understanding. Contrary to those desires, quantum theory does *not* describe physical reality. What it does is provide an algorithm for computing *probabilities* for the macroscopic events ("detector clicks") that are the consequences of our experimental interventions. This strict definition of the scope of quantum theory is the only interpretation ever needed, whether by experimenters or theorists.

But that is misleading and trouble-making. In the second to last sentence—with the experience of three more years of thinking on this subject—I so wish we had said something more to the tune:

> What quantum theory does is provide a framework for structuring MY expectations for the consequences of MY interventions upon the external world.

At least that is what the formal structure is about. There is no "we," there is no "our." At this level of consideration, quantum theory has nothing to do with intersubjective agreement. (By the way, I'm not fooling myself: Of course we could not have said what I said above without restructuring the whole article—it would have opened a can of worms! I just want to try to do the idea better justice right now.)

Here it is: Any single application of quantum THEORY is about ME, only me. It is about MY interventions, MY expectations for their consequences, and MY reevaluations of MY old expectations in the light of those consequences. It is noncommittal beyond that. This is not solipsism; it is simply a statement of the subject matter.

Is there any contradiction in this? I say no, but how do I get you into a mindset so that you might say the same? Maybe the best way to do this is to run through a glossary of quantum terms as I did once before ... but now with all the latest slant.

- SYSTEM: In talking about quantum measurement, I divide the world into two parts—the part that is subject to (or an extension of) my will, and the part that is beyond my control (at least in some aspects). The idea of a "system" pertains to a part beyond my control. It counts as the source of my surprises, and in that sense obtains an existence of its own external to me. (Point 1 against solipsism, but I will return for another.)

- POVM: In the theory, this counts as an extension of my will. It counts as a freely chosen action on my part. The whole concept of a "measuring device" as something distinct from me—I am now thinking—just gets in the way. It is a point that Pauli made, but I am coming ever more to appreciate it. A "measuring device" is like a prosthetic hand; its conceptual role is for the purpose of recovering from our natural incapacities and, thus, might as well be thought of as part of ourselves proper. I perform a POVM on a system—captured mathematically by a set—and one of its elements comes about as a consequence.

- QUANTUM STATE: As usual, the catalog of MY expectations for the consequences of MY actions (i.e., POVMs) ... but now with absolute, utter emphasis on the MY.

- UNITARY READJUSTMENT: I'm talking here about the readjustment appearing in Eq. (95) of my paper `quant-ph/0205039`. This, like a quantum state, also captures a belief or expectation. Its purpose is to quantify the extent to which I feel the need to deviate from Bayes' rule after learning the consequence of my action. This is what takes account of the nonpassive nature of MY interventions.

- QUANTUM DYNAMICS: This is the unitary readjustment (or mixture of decompositions and unitary readjustments) that I judge I ought to apply if my action on the system is passive, i.e., if my POVM is the singleton set. It is how I readjust my expectations when I am learning nothing.

Summing up the glossary, I would say quantum theory in its single user implementation is about ME. I act on the world and it reacts in a way unpredictable to me beyond the expectations I build from MY quantum state (about the system).

Why is this not solipsism? Because quantum theory is not a theory of everything. It is not a statement of all that is and all that happens; it is not a mirror image of nature. It is about me and the little part I play in the world, as gambled upon from my perspective. But just as I can use

quantum theory for my purposes, you can use it for yours. Thus, if I had not been seeking dramatic effect above, I should have more properly said, "Any single application of quantum THEORY is about the ME who applies it." (Don't correct my English.) When David Mermin is a practitioner of quantum theory, what the theory does is provide a framework for structuring HIS expectations for the consequences of HIS interventions upon HIS external world. ... And that is Point 2 against solipsism.

Recall the definition of solipsism I dredged up from the Encyclopedia Britannica:

> in philosophy ... the extreme form of subjective idealism that denies that the human mind has any valid ground for believing in the existence of anything but itself.

It seems to me we have plenty of valid ground for believing in the existence of something besides ourselves: It comes from all the things we cannot control. Indeed, as already emphasized, for those things we can control, we might as well think of them as extensions of ourselves. Thus, to my mind, quantum theory already gives a karate chop to solipsism because of the indeterminism it entails: With each quantum measurement there is immediately something beyond my control.

Beyond Point 1, though, there is Point 2. It is a question of finally getting straight what should and should not be in the purview of the theory. In this account, quantum theory is a theory of personal action (and reaction). The law-of-thought aspect of it comes out with respect to each individual who uses it. The textbook poses an exercise that starts out, "Suppose a hydrogen atom is in its ground state. Calculate the expectation of ... blah, blah, blah." One might think it is asking us to calculate some objective feature of the world. It is not. It is only asking us to carry out the logical consequences of a supposed state of belief and a supposed action that one could take upon the system. And here's the clincher about Bayesianism. Just as no student in his right mind would find it worthy to ask why the textbook writer posed the problem with the ground state rather than the first excited state, no quantum theorist should make a big to-do about it either. It is simply an assumed starting point. An agent in the thick middle of a quantum application can no more ask where he got his initial beliefs from, than a pendulum can ask where it got its initial conditions from. The cause of bottom-level initial conditions is ALWAYS left unanalyzed. If such was not a sin in Newtonian mechanics, it should not be a sin in a Bayesian formulation of quantum mechanics.

So, it seems to me, if anything, the Bayesian account of quantum theory is essentially the opposite of solipsism. Rather than a unity to nature, it suggests a plurality. An image that might be useful (but certainly flawed) comes from Escher's various paintings of impossible objects. The viewer would initially like to think of them as two-D projections of a three dimensional object; but he cannot. Now imagine how much worse it would get if we were to have two viewers with two slightly different paintings, each purporting to be a different perspective on "the" impossible object. Since neither viewer can lift from his own two-D object to a three-D one, there is no way to unify the pictures into a single whole.

Yet we live in one world, you say. Maybe. But, you should remember that these quantum states we speak of are not perspectives. They are personal possessions. To paraphrase Tilgher's quote at the beginning of de Finetti's Probabilismo,

> A quantum state is not a mirror in which a reality external to us is faithfully reflected; it is simply a biological function, a means of orientation in life, of preserving and enriching it, of enabling and facilitating action, of taking account of reality and dominating it.

"Are there other minds beside your own?," Howard Wiseman asks. If a mind is what it takes to write down a quantum state, then why not? "If you leave the origin of the quantum state

unanalyzed, why would two minds ever agree on anything?" That is the issue of intersubjective agreement—something thankfully we can study within the context of quantum theory. But the first thing to get straight is why the single user of quantum theory uses the very structure. What is it precisely that he is believing of the world and his place in it that leads him to the choice of quantum theory as his law of thought?

That is, it is about ME and what I believe. But what do I believe? That's the research program!

13-08-03 *The Tense Mix* (to R. Schack)

Now that I'm back in Munich, I've tried to become re-engaged with the conversation we left on, but I'm having difficulty. I've read your 30/7/03 note "levels" three times over since yesterday, and I'm not sure what I can add at the moment. Clearly I've absorbed some of it into my mentality. In particular, I like the scolding you gave me:

Schackcosm 82 : *NOOOOOOO! You are joking, aren't you? The direct lesson of Bell and Kochen-Specker is indeed that we should not act AS IF unperformed measurement had outcomes. To conclude that nature is at a juncture is adding a lot of baggage, however. I am sure de Finetti would be rotating in his grave if he read this!*

You are right; the best I can say is that the lesson of quantum mechanics is that we should not act AS IF unperformed measurements had outcomes. And that is good enough for me. Like with William James, I will act as if nature is undetermined and see where that leads me. I can't do more than that. (In fact, I have said it many times that one may never be able to disprove either many-worlds or Bohmian mechanics. I only bank that those trains of thought will not lead in any productive directions. I suppose I suspended such carefulness as my passions started to flare in our discussion.)

But as you might guess from the paragraph I just wrote, I still see something of an uncomfortable tension in what you've written. For instance, I don't even understand your very next sentences:

Schackcosm 83 : *I think de Finetti was both anti-realist and anti-indeterminist. The Buridan's ass passage illustrates this beautifully. Your position is clearly not anti-indeterminist. Be careful who you recommend Probabilismo to!*

Also I guess I don't like this emphasis of yours on the MEANINGLESSNESS of various statements. It doesn't ring true to me, especially with the determinist/indeterminist issue. It strikes me as perfectly useful to take a stance on the issue (of course in the AS IF sense that you have emphasized); and in that way it is not meaningless. That, I think, is why I brought up religions in an earlier note; religions are never verifiable or falsifiable, but faith in them is not meaningless.

It seems to me clear—and I don't think your note has changed this—that when we practice quantum mechanics, we are implicitly stating some beliefs about the world (as it is independently of the agent). You are right that we cannot move from that step to saying that that is actually the way the world is. Nonetheless we are expressing *beliefs* about the world without our presence. The issue is to make those beliefs explicit—and that is your program of AS IF. I am OK with that. To put so much emphasize on the meaningless of this or that, though, repels me slightly. That may be what caused me to swing the pendulum too far in the opposite direction.

I'm really looking forward to your visit to Munich. I hope we can find a way to get together. Aarhus was a useful meeting: Klaus Mølmer, Richard Gill, Ray Streater, Vladimir Buzek, Mauro D'Ariano, Hideo Mabuchi, and Howard Wiseman showed a some decent interest in the program. But I must say, the divergences between us and our desires and Rob Spekkens are becoming clearer

... and more disappointing. I see him as having the potential to cause real damage. It really would be useful to have a more thoughtful critique of his "classical interpretation of probability over a fundamental event space" position. Bernardo and Smith's dismissal is simply too curt.

Somehow over the course of this meeting, I've decided the time is right to try to put together a paper explicitly and solely on the Penrose argument. The little title I've toyed with in my head has been "On Quantum Certainty." What would you think? Might we throw in on it together? (I guess I was partially induced to this by Spekkens' assertion that we still have no adequate reply to Penrose without the picture he is trying to construct ... based on ignorance of an ontic state.) Anyway, I started thinking that that might be riper and more ready to go than a critique of the Horodeckis. Also it strikes me as paving the ground for all the other discussions, and worthy of its own thorough treatment.

This is the first of a couple of notes I'll be writing you today. Another one will be coming down the tubes in an hour or so.

13-08-03 *Renouvier* (to R. Schack)

I'm back a little faster than I thought I would be. In this note, I just want to send you a passage that I scanned into my computer from a book on Charles Renouvier—the philosopher who convinced William James to try out free will. There is something about the argument in the last two paragraphs of page 2 that takes me—but I want to see it made more rigorous, or stated better. Also I hope you are as impressed by Footnote 13 as I was. If those passages are not too far out of context, they may show that he is going down the same lines as de Finetti and us.

I'd love to get my hands on anything more technical to do with Renouvier, but apparently there is almost nothing written on him in the English language. The only article I have been able to unearth that might possibly be in some library in your university is: S. Hodgson, "M. Renouvier's Philosophy," Mind **??**, 31–61, 173–211 (1881). If you have time, could you check on that before Munich?

From: William Logue, *Charles Renouvier, Philosopher of Liberty*,
(Louisiana State University Press, Baton Rouge, 1993), pp. 86–92.

> Under the influences of discussions with his friend Jules Lequier, he [Renouvier] became convinced of the reality of human free will and its central importance for the understanding of everything else.[2] This conviction came to Renouvier while he was still deeply under the influence of his first contact with the Saint-Simonians. He experienced, not an overnight liberation from their deterministic viewpoint, but a more gradual readjustment of his views, which became a complete detachment from them only after 1851. Perhaps the failure of the socialist movements in 1848, rooted as they were in the would-be scientific philosophies of the preceding three decades, finally persuaded him of the dangers of rejecting free will.[3] Alienated from political life during the Second Empire, he would spend nearly two decades in the construction and elaboration of his philosophy of liberty, establishing its foundations and exploring its consequences.

[2]Renouvier's account of his "conversion" to free will is in the last part of Vol. II of the *Esquisse*. Lionel Dauriac ("Les Moments de la philosophie de Charles Renouvier," *Bulletin de la société francaise de philosophie*, IV [1904], 23) defined the high point of Lequier's influence—the writing of the *Deuxième essai*—as one of four "moments" in Renouvier's philosophical development.

[3]Mouy (*Idée de progrès*, 43) argues that the disappointments of 1848 played a key role in shaping Renouvier's idea of liberty. For Renouvier, free will came to be seen as the ultimate basis of political liberty (*Deuxième essai*, 551). See *Histoire*, IV, 431, and especially *Esquisse*, II, 382, and *Deuxième essai*, 371*n*1.

Renouvier was aware that for a long time the question had been of mainly religious significance: whether man's salvation depended on free will or on predestination.[4] This debate had reached its peak, in both vehemence and subtlety, in the famous exchange between Erasmus and Luther in the sixteenth century. The emergence of a secular debate over free will was a result of the rise of the scientific worldview in the seventeenth century. The ascendancy of the idea that the world was governed by invariable laws, taking the role previously occupied by an all-powerful, all-knowing God, seemed to leave less and less room for the view that man was somehow an exception to the general rule. The most heroic task for the modern philosopher was to find a means of validating science and free will simultaneously, and the most heroic effort of the eighteenth century was that of Immanuel Kant. But for many in the next century, it seemed that Kant had saved free will only at the cost of making it irrelevant.[5] Fichte tried to rescue Kantian philosophy from this unhappy outcome, but in the general opinion (only recently challenged by Alexis Philonenko, Luc Ferry, and Alain Renaut) his effort led to the fairyland of absolute idealism, denying reality to the material world.[6]

Against the rising tide of determinism, Renouvier would try to show that Kant could be the launching pad for a defense of free will that would maintain its practical relevance and demonstrate its compatibility with natural science, properly understood. He did not claim to be presenting any new arguments in favor of free will; he felt they were in any case unnecessary.[7] Renouvier's reasons for coming to the defense of free will were partly shared with Kant and partly his own. As we have seen in the previous chapter, the shared part was the most familiar: a concern for the connection between free will and moral behavior. Free will was for Kant the essential basis of *practical reason*; without it, the whole idea of moral obligation ceased to have meaning. For Renouvier, this consideration remained central. Without moral responsibility, man would not be distinct from the rest of the animal kingdom, and the whole of civilization would be meaningless. But this was not the sole basis for his concern with free will, and this additional concern moved Renouvier beyond Kant and Fichte, bringing him closer to our own time.[8]

It is not just the moral aspect of civilization that hangs on the reality of free will, in Renouvier's opinion, but the whole of our intellectual life. Free will is also the foundation on which philosophy and the natural sciences rest.[9] Without free will, our ability to know anything, whether about man or about nature, is fatally undermined. Scientists do not need to believe in free will, and as he knew, they prefer to avoid this sort of question. In practice, they can legitimately do so because in their narrow

[4]Renouvier saw free will as one of the basic concepts of both philosophy and Christian doctrine (*Histoire*, IV, 277).

[5]What does it matter to man if he has freedom in the world of the noumena if his world of phenomena is entirely determined? Renouvier later felt that Kant held on to free will solely for the sake of morals while not really believing in it (*Quatrième essai*, 35–36).

[6]Renouvier praised Fichte as a defender of freedom and criticized him as a mystic (*Quatrième essai*, 46). See Alain Renaut, *Le Système de droit: philosophie et droit dans la pensée de Fichte* (Paris, 1986); Luc Ferry, *Le Système des philosophies de l'histoire* (Paris, 1984), Vol. II of Ferry and Renaut, *Philosophie politique*; Alexis Philonenko, *La Liberté humaine dans la philosophie de Fichte* (Paris, 1966).

[7]Renouvier was concerned to establish a rationalist and not an empiricist view of science. He saw free will as perhaps the main issue dividing the rationalists and empiricists (*Histoire*, IV, 262). He indicated that there had been no new arguments in favor of free will since Kant and Rousseau (*Esquisse*, I, 280).

[8]Renouvier saw Kant's German disciples as having abandoned liberty for determinism, optimism, and pantheism (*Histoire*, IV, 467).

[9]See *Deuxième essai*, 227.

spheres of inquiry they have developed techniques of investigation that work even when the scientist is unconscious of the fundamental assumptions on which his method rests. But without free will, the certainty of scientific truths becomes illusory; a consistent determinism must lead to a profound skepticism.[10] Renouvier would never despair of convincing the scientists that just as our concepts of right and wrong depend on free will, so do our concepts of true and false. Indeed, without free will, we could not even talk sensibly about things being true or false.

If, as he pointed out, I hold such and such a view to be true and I am determined by forces outside my control to hold this view, the person who disagrees with me is equally determined by outside forces in his position. If these mutually contradictory positions are equally necessary, what grounds can we have for the certainty that either view is the correct one?[11] If our belief that our ideas are determined is itself determined, so is the other person's belief in free will determined. Under these conditions how could it make any sense to speak of one view as "right" and the other as "wrong"? If, on the other hand, our choices are free, I may freely choose to believe in free will or in spite of the apparent contradiction, to believe in universal determinism. Of the four possible positions revealed by this analysis, the only one that can serve as a foundation for a rational certainty in the truth of our beliefs is to freely believe in freedom.[12] But as Renouvier insists, this means that we must give up any pretension to the absolute certainty of our beliefs.[13] The truth of free will cannot be proved so that no rational person can doubt it. It is a relative truth, like all our other truths, but more important because it plants a relativism at the very core of our thought.[14]

Scientists, Renouvier thought, should have no difficulty understanding and accepting this because science is built on an awareness of the conditional character of our knowledge, an openness to the discovery of new truths and the abandonment of old ones.[15] In fact, he had to admit, many scientists were still under the sway of older metaphysical conceptions of truth, except in the conduct of their personal research, and were unaware of any inconsistency in their position.[16] Some who were aware were evidently afraid that to admit that an act of belief was at the base of scientific knowledge would risk

[10] *Histoire*, IV, 399; *Deuxième essai*, 327.

[11] *Histoire*, IV, 399; *Deuxième essai*, 306–307 (according to Hamelin, *Système de Renouvier*, 242). Necessity destroys truth: "If everything is necessary, error is necessary just as much as truth is, and their claims to validity are comparable" (*Deuxième essai*, 327). For a restatement of his argument that freedom is essential to the certainty of our knowledge, see *Esquisse*, II, 270–74; see also, *Science de la morale*, II, 377.

[12] The four are (1) we are determined to believe in freedom; (2) we are determined to believe in determinism; (3) we freely believe in determinism; (4) we freely believe in freedom. See *Deuxième essai*, 478; *Histoire*, IV, 399; Hamelin, *Système de Renouvier*, 273–74.

[13] "Certitude is not and cannot be an absolute. It is, as is too often forgotten, a condition and an action of man: not an action or a condition where he grasps directly that which cannot be directly grasped—that is to say, facts and laws which are outside or higher than present experience—but rather where he places his conscience such as it is and as he supports it. Properly speaking, there is no certitude; there are only men who are certain" (*Deuxième essai*, 390). For Renouvier's battle against the idea of evident truths, see *Histoire*, IV, 75, 261; certitude is a sort of "personal contract," "a real contract that a man makes with himself" (Lacroix, *Vocation personnelle*, 114).

[14] *Deuxième essai*, 309–10 (according to Hamelin, *Système de Renouvier*, 242). There were, however, "great probabilities in its [free will's] favor" (*Deuxième essai*, 475). "It ought to be a universally accepted maxim that *everything that is in the mind is relative to the mind*" (*Deuxième essai*, 390). Philosophy needs to take into account the existence of disagreement among philosophers (*ibid.*, 414). Renouvier's approach to the existence of these disagreements is one of the distinctive features of his philosophy.

[15] On the use of hypotheses in science, see *Premier essai*, 200.

[16] Renouvier credited the English empiricists, following Hume, with freeing science from the metaphysical concept of cause (*Histoire*, IV, 273).

undermining the claim of science to objectivity and, even worse, open the way to the proliferation of pseudoscientific beliefs.[17] In reality, pseudoscientific beliefs were already proliferating under the aegis of the belief in determinism. Without a critical analysis of the nature and limits of scientific knowledge, however, our intellectual life is subject to a constant abuse of the name and prestige of science.

The abuse of science takes many forms: the application of research methods to fields where they do not apply, the application of particular concepts to areas other than those where they originated, the confusion of "Science" with the operations of particular sciences. One of the main intellectual trends of the nineteenth century, which Renouvier called *scientisme*, usually rendered as "scientism" in English, was the product of this abuse. Renouvier's relativism does not justify believing in whatever we want to believe.[18] It insists on submitting our opinions to every possible test of logic, experiment, and experience. But we have to admit that our logic, the hypotheses on which our experiments are based, the schemas of thought by which we interpret our experience, all rest ultimately on acts of belief and not on absolute certainties.[19]

If free will is thus essential to both morals and science, just what does he mean by it? Over the centuries, most of the debate over free will has failed to advance our understanding because of the lack of agreement about what is meant by the term.[20] I cannot solve that problem, but I think we will see that Renouvier's view makes the issue more comprehensible.

Free will, for Renouvier, is a capacity possessed by human beings, and only by human beings, that enables them to choose whether to accept one idea or another, whether to perform one act or a different one. It is thus a rejection of the doctrine that holds that all events, mental or physical, are absolutely determined and cannot be other than what they are.[21] Free will is also a rejection of the doctrine of chance, for it is an active power and not the "liberty of indifference" so belabored by determinists.[22] Chance is also hostile to liberty, since it denies man a real power of decision.

The existence of free will requires a measure of indetermination in the universe but could not exist if nature were essentially indeterminate.[23] Our acts of free will are the beginnings of chains of consequences and would have no meaning if their consequences were not subject to cause and effect. "Free acts are not effects without causes; their cause is man, the ensemble and fullness of his functions. They are not isolated, but are always closely attached to the preceding condition of the passions and of knowledge. *A posteriori* they seem henceforth indissoluble parts of an order of facts, although a

[17] This is the concern of Parodi (*Du positivisme à l'idéalisme*, 184–85), who finds in Renouvier a dangerous fideism. So does Brunschwicg (*Progrès de la conscience*, 625). For Renouvier's praise of Boutroux's argument that the contingency of the laws of nature is not a threat to science, see *Histoire*, IV, 673–74.

[18] Dauriac ("Moments de la philosophie," 30–32) strongly makes this point. It would be interesting to compare Renouvier's conclusion on this point with the similar view expressed by Richard Rorty, coming from a rather different direction.

[19] *Histoire*, IV, 692; on the need for faith in reason, even though such faith is in itself not a rational act, see Popper, *High Tide of Prophecy*, 218–19.

[20] Adler, *Idea of Freedom*.

[21] See definition of free will in *Histoire*, IV, 337; on liberty as choice, see *Deuxième essai*, 466; on real alternatives, see *ibid.*, 339. Renouvier is rejecting a causal necessity, not analytic necessity, as in the syllogisms of logical operations (*Premier essai*, 232–36).

[22] *Deuxième essai*, 330–34, 336, 337; Hamelin, *Système de Renouvier*, 242–43,249.

[23] "Liberty does not require the complete indetermination of particular future events, even of those that are directly connected to it" (*Deuxième essai*, 459); see also *ibid.*, 357; Hamelin, *Système de Renouvier*, 244.

different order was possible *a priori*."[24] The laws that permit us to say this is followed by that do not admit of an infinite regression into the past, according to Renouvier. Therefore, every series of phenomena—and indeed the existence of any phenomena—must have a beginning that we cannot explain in terms of antecedents.[25]

The act of creation of the universe is thus replicated (in a much smaller way!) in every act of free will. Every act of free will is the creation of a new series of phenomena, a series that would not otherwise have existed.[26] These new chains of cause and effect are not simply the product of the intersection of existing but independent series, as A.-A. Cournot argued, for such intersections, though they appear random from the point of view of any one of the colliding series, would be necessary from a higher viewpoint.[27] They must be new beginnings, arising from a conjuncture in which, given the antecedents, more than one consequence was possible: "ambiguous futures," Renouvier called them.[28] Free will is the capacity to opt for one or another of those futures.

17-08-03 *Tearing Off the Duct Tape* (to N. D. Mermin)

Merminition 123 : *Please take the damn tape off your mouth. If I'm going in the wrong direction I don't wish to waste anybody's time.*

I apologize for keeping you waiting for another two days. Once I signed off Friday at Beer:30, I wasn't able to get back to email until this morning. The whole family ended up in Munich proper yesterday (rather than a leisurely day here in Zorneding), and then by the evening I was exhausted.

So, let's get to it.

Merminition 124 : *Quantum mechanics says (a) If two gates are measurement gates and (b) if no other unitary gates are in the circuit but those two then (c) if a qubit is sent through the circuit then with probability 1 both gates will give the same reading.*

This appears to be an assertion that under certain conditions any rational person will strongly believe that both gates will give the same reading. Furthermore, the conditions are not that in the past million runs only 00 and 11 have been registered and never 01 and 10 (as in the story you tell me about how Bob comes to his strongly held belief). They are conditions about the structure of the circuit, through which no qubit may ever have been sent. If, under these conditions, all rational people must agree that the gates will give the same reading, some might worry that it was nitpicking (rather than deeply insightful) to nevertheless insist that this must be viewed as a subjective judgment rather than an objective property of the circuit.

Yes, exactly; your last sentence would certainly be my own take if it were to stop at that. I doubt you remember this, but it was the BFM paper that started pushing me down the radical

[24] *Deuxième essai*, 359; see also *Science de la morale*, II, 361–62.

[25] *Premier essai*, 237; *Science de la morale*, II, 360–61. Most scientists today reject the idea that infinite regression is an absurdity; *Esquisse*, II, 378–79. We cannot explain beginnings because they are by definition at the limits of our possible knowledge.

[26] *Esquisse*, II, 196-97.

[27] A.-A. Cournot, *Considérations sur la marche des idées et des événements dans le temps modernes* (Paris, 1973), 9–10, Vol. IV of Cournot, *Oeuvres complètes*, ed. André Robinet.

[28] *Deuxième essai*, 210. "The real indetermination of various phenomena envisaged in the future" (*Premier essai*, 240). "[A determinist] would renounce everything called reflection and reason, for these functions do not work without the consciousness of a *representative self-motivation*, which is itself linked to an awareness of the *real ambiguity of future conditions* before it takes action" (*Histoire*, IV, 769). See also *Troisième essai*, xlvii; Hamelin, *Système de Renouvier*, 230.

Bayesian path precisely because I perceived its message to be something like your point above. (Of course you didn't say "subjective judgment" but rather "knowledge," but for me it had the same effect.)

I just took a walk down memory lane rereading my correspondence to you, Brun, Finkelstein, Caves, and Schack between 7 August and 2 September 2001 to see if I could find any good quotes along those lines. Kind of depressing really: The sad thing was that I seemed to be far more lucid then than I was by the end of our Montreal meeting in November 2002 (capped off by my fatal mistake on 4 November). For instance, when I look back at my 22 August 2001 note to Caves and Schack "Identity Crisis," I see that it had a perfectly good discussion of CERTAINTY—the very starting point of this latest email conversation, as rekindled by Rüdiger's 18 June note this year. Traveling in circles and circles. Reread some of that stuff and tell me whether you don't think it is dead on the mark for today's very discussion. Let me give you an outtake, from a note to you 2 September 2001:

> The point of separating the categories "knowledge" and "reality" (or "subject" and "object" for that matter) is not to make knowledge an objective reality in its own right or, even worse, to make it the sole reality. Rather it is to say that there is a distinction and that that distinction should be recognized. ...
>
> What I have ultimately NOT been able to stomach about your wording of the whose-knowledge "answer", and Jerry's wording of the whose-knowledge "answer"—some of Todd's versions would actually survive—is that you say, under certain circumstances, two scientific agents (observers, or what have you) MUST assign "consistent" quantum states to a given system. In the case of pure states, the two agents MUST assign the *same* pure state to the system. ...
>
> What I object to is the word MUST. Todd once wrote it this way,
>
> **Brunism 8 :** *We have been describing a consistency criterion.* If *one wishes to combine two state descriptions of a single system into a* single *state description, the criterion tells one* when *it is consistent to do so (i.e., when the two descriptions are not actually contradictory).*
>
> *I agree that nobody is holding a gun to Alice's head and forcing her to incorporate Bob's information.*
>
> and to this way of speaking I can agree. But if you take away Todd's "*If*", then everything collapses in my mind. Enforcing that two agents MUST make the same state assignment if they are going to be "right" at all reinstates the very objectivity, the very agent-independence of the quantum state that the Mechanica-Quantica-Lex-Cogitationis-Est program has been working so hard to exorcise. ...
>
> It is much like the old debate. Is materialism right? Or is it Berkeley's idealism that is right? Who cares, I say. Both philosophies are just simple samples of realism: They only disagree on the precise concept which ought to be taken as real, mundane matter or sublime consciousness. The way you characterize it above, one would think that the only fruit of the Mechanica Quantica program would be the RENAMING of a material reality into an ideal one—a shift more of emphasis, rather than anything of grit.

So, let me get to your better formulation straight away:

Merminition 125 : *I believe your answer to such worriers would be something like this: that* (*a*) *and* (*b*) *imply* (*c*) *is not an objective fact about the world of circuits, but a law of thought (though*

it remains a part of the research program to show precisely how it emerges as a law of thought). In isolation it cannot affect anybody's degree of belief — for example it cannot guide betting behavior — unless (a) *and* (b) *can be established. But* (a) *and* (b) *are subjective judgments. Bob requires a million runs not to establish the law of thought* $(a) + (b) \Rightarrow (c)$*, but to build up his strongly held belief in* $(a) + (b)$*.*

Yep, you're roughly on track here. The law-of-thought (or maybe better, law-of-ideal-thought) character in the implication $(a) + (b) \Rightarrow (c)$ is due to Gleason's theorem plus Dutch-book coherence. It's the kind of point we tried to make in `quant-ph/0106133`. The definition of your first measurement gate in (a) is that it gives "maximal information" about a reapplication of itself, i.e., it gives certainty for the click (as judged by any agent for which it is actually a measurement gate). DB coherence turns that certainty into a $p = 1$ assignment, and Gleason's theorem further turns that into a unique pure state. The judgment of (b) allows this pure state to be reconverted into a second $p = 1$ assignment for the outcome of the later measurement gate.

The goal of the research program is indeed to show that all of this together is a kind of Dutch-book coherence or, at least, Dutch-book coherence modulo some particular assumption about the properties of the (contingent, physical) world. (Rüdiger and I may still have slightly different goals here; its hard to tell at this point.)

Merminition 126 : *In isolation it cannot affect anybody's degree of belief — for example it cannot guide betting behavior — unless* (a) *and* (b) *can be established.*

Yes. In fact, it is just as normal Dutch-book coherence sets no probability assignments at all. (Emphasizing, of course, that even in the case of "certainty" we have had to recognize the subjectivity of the judgment.) If DB coherence is taken to be akin to a dynamical law in classical physics; (a) and (b) should be taken to be akin to the initial conditions. (I don't know if that analogy helps.)

Let me only take point with one issue:

Merminition 127 : *But* (a) *and* (b) *are subjective judgments.*

You should stop your development right there. Conceptually, the origins of (a) and (b) should be left unanalyzed (just as with initial conditions in classical mechanics). You can of course do what you do in the next sentence (to somehow make it all more palatable),

Merminition 128 : *Bob requires a million runs not to establish the law of thought* $(a)+(b) \Rightarrow (c)$*, but to build up his strongly held belief in* $(a) + (b)$*.*

but you should realize that in doing this you expand the context of the problem without adding anything of deeper significance to it. The data from those million runs are of no significance without still a further prior judgment. (Rüdiger introduced exchangeability to bring the language to the turf of your 1985 paper, but exchangeability is still nothing more than a judgment.) To know that a million runs (rather than a billion runs or a trillion runs) is of significance requires still more to be said: In particular one must choose a particular exchangeable assignment rather than another. So at some level, one is always stuck with making a quantum state (or quantum operation) assignment just to get the ball rolling. Thereafter, everything is empirical data plus law of thought applied to that initial judgment.

For (a) and (b) to be "subjective judgments" in our sense means (1) that their origins need not be analyzed, and (2) there is nothing in the world that *requires* two agents to have the same such

judgments. The value of the game foundation-wise is that it tells you that if you are looking for the "objective" or "intersubjective" in quantum mechanics, you should look elsewhere.

I hope you'll send the second installment soon! Rüdiger and I are now set to meet late morning Tuesday, and it would be great to discuss your note face to face.

18-08-03 *The Big IF* (to A. Sudbery & H. Barnum)

I'm running far, far behind in all the things I've wanted to do this summer, but maybe there's a chance I'll catch up. In particular, I have been trying to give Mr. Nagel a concerted effort during my vacation here in Munich. I went out and bought *The View from Nowhere* and am a little ways into it. When I finish that, I'll re-approach your article "Why Am I Me?" and give you a detailed appraisal.

It's probably too early in my reading to tell, but my troubles with Nagel may all boil down to "The Big IF." That is, they may boil down to the religion that lies behind this passage plucked out of his article "Subjective and Objective." (I'll capitalize the big IF and a couple of other appropriate words so that you'll know what I'm talking about.) Here goes:

> Since a kind of intersubjective agreement characterizes even what is most subjective, the transition to a more objective viewpoint is not accomplished merely through intersubjective agreement. Nor does it proceed by an increase of imaginative scope that provides access to many subjective points of view other than one's own. Its essential character, in all the examples cited, is externality or DETACHMENT. The attempt is made to view the world not from a place within it, or from the vantage point of a special type of life and awareness, but from nowhere in particular and no form of life in particular at all. The object is to discount for the features of our pre-reflective outlook that make things appear to us as they do, and thereby to reach an understanding of things as they really are. We flee the subjective under the pressure of an assumption that everything must be something not to any point of view, but in itself. To grasp this by DETACHING more and more from our own point of view is the unreachable ideal at which the pursuit of objectivity aims.
>
> Some version of this polarity can be found in relation to most subject matter—ethical, epistemological, metaphysical. The relative subjectivity or objectivity of different appearances is a matter of degree, but the same pressures toward a more external viewpoint are to be found everywhere. It is recognized that one's own point of view can be distorted as a result of contingencies of one's makeup or situation. To compensate for these distortions it is necessary either to reduce dependence on those forms of perception or judgment in which they are most marked, or to analyze the mechanisms of distortion and discount for them explicitly. The subjective comes to be defined by contrast with this development of objectivity.
>
> Problems arise because the same individual is the occupant of both viewpoints. In trying to understand and discount for the distorting influences of his specific nature he must rely on certain aspects of his nature which he deems less prone to such influence. He examines himself and his interactions with the world, using a specially selected part of himself for the purpose. That part may subsequently be scrutinized in turn, and there may be no end to the process. But obviously the selection of trustworthy subparts presents a problem.
>
> The selection of what to rely on is based partly on the idea that the less an appearance depends on contingencies of this particular self, the more it is capable of being

> arrived at from a variety of points of view. IF THERE IS A WAY THINGS REALLY ARE, which explains their diverse appearances to differently constituted and situated observers, then it is most accurately apprehended by methods not specific to particular types of observers. That is why scientific measurement interposes between us and the world instruments whose interactions with the world are of a kind that could be detected by a creature not sharing the human senses. Objectivity requires not only a departure from one's individual viewpoint, but also, so far as possible, departure from a specifically human or even mammalian viewpoint. The idea is that if one can still maintain some view when one relies less and less on what is specific to one's position or form, it will be truer to reality. The respects in which the results of various viewpoints are incompatible with each other represent distortions of the way matters really are. And if there is such a thing as the correct view, it is certainly not going to be the unedited view from wherever one happens to be in the world. It must be a view that includes oneself, with all one's contingencies of constitution and circumstance, among the things viewed, without according it any special centrality. And it must accord the same DETACHED treatment to the type of which one is an instance. The true view of things can no more be the way they naturally appear to human beings than the way they look from here.
>
> The pursuit of objectivity therefore involves a transcendence of the self, in two ways: a transcendence of particularity and a transcendence of one's type. It must be distinguished from a different kind of transcendence by which one enters imaginatively into other subjective points of view, and tries to see how things appear from other specific standpoints. Objective transcendence aims at a representation of what is external to each specific point of view: what is there or what is of value in itself, rather than *for* anyone. Though it employs whatever point of view is available as the representational vehicle—humans typically use visual diagrams and notation in thinking about physics—the aim is to represent how things are, not *for* anyone or any type of being. And the enterprise assumes that what is represented is DETACHABLE from the mode of representation, so that the same laws of physics could be represented by creatures sharing none of our sensory modalities.

The two key ideas in this passage that I think quantum mechanics plays the most havoc with are:

1. the DETACHED agent (observer, scientist, etc.), and
2. IF there is a way things really are ...

I honestly believe one can take the Nagel worldview seriously—I suspect there is no logical flaw in it. One can legitimately try to make quantum mechanics fit that worldview with more or less success. My only point is the strong personal suspicion that with such a project one forces quantum mechanics into shoes it does not fit. And, as I see it, what bunions that will cause in the future!

The whole subject matter of my *Notes on a Paulian Idea* is in toying with the idea that the cleanest expression of quantum mechanics will come about once one realizes that its overwhelming message is that the observer cannot be detached from the phenomena he HELPS bring about. I capitalize the word HELPS because I want you to take it seriously; the world is not solely a social construction, or at least I cannot imagine it so. For my own part, I imagine the world as a seething orgy of creation. It was in that orgy before there were any agents to practice quantum mechanics and will be in the same orgy long after the Bush administration wipes the planet clean. Both of

you have probably heard me joke of my view as the "sexual interpretation of quantum mechanics." There is no one way the world is because the world is still in creation, still being hammered out. It is still in birth and always will be—that's the idea. What quantum mechanics is about—I toy with—is each agent's little part in the creation (as gambled upon from his own perspective). It is a theory about a very small part of the world. In fact, I see it as a theory that is trying to tell us that there is much, much more to the world than it can say. I hear it pleading, "Please don't try to view me as a theory of everything; you take away my creative power, my very promise, when you do that! I am only a little theory of how to gamble in the light of a far more interesting world! Don't shut your eyes to it."

The question is, how to get one's head around this idea and make it precise? And then, once it is precise, what new, wonderful, wild conclusions can we draw from it? That is the research program I am trying to define.

Is it a SCIENTIFIC research program? I think so, and in the usual sense. There will be lemmas, theorems, and corollaries. (I would like to think that my work and the work of the fellows I've drawn down this path already evidences this.) Ultimately there will be calls for experiments. There will be technologies suggested and money to be made from the program's fruits. Failure of nerve? Anything but!:

Sudberyism 1 : *Maybe you and [Rorty] can shift me from my instinctive reaction to pragmatism, which is that for a scientist it represents a failure of nerve, a failure of imagination, and most seriously a failure of curiosity. Being useful cannot, for a scientist, be the end of the story about a statement or a theory; we immediately want to know* why *one theory is more useful than another. That "why?" leads us to an external world of some kind, maybe very strange (the stranger the better, i.e. the more interesting, I would say) and to refuse to follow where it leads seems to me to be a scientific copout.*

I see it as anything but a failure of curiosity or a copout! What you wrote me above reminds me of a conversation I had with Chris Timpson in a pub one night. I made the mistake of mentioning William James, and Chris quickly intoned, "ALL James was about was the nonsense that truth resides in what is useful." The word ALL just boomed! A man's whole life was dismissed in a single sentence. I cut him short, "William James was about many things, ONE OF WHICH was that the correspondence theory of truth holds no water." Similarly I will say to you, there is far more explored by the pragmatist thinkers than that which is delimited by their ideas on truth and warranted belief. Pragmatism is not positivism; it is not that there is nothing to be sought in science beyond the connections between sense perceptions. I see the classical pragmatists (and myself) as ultimately realists, but honest realists—ones who have realized that our theories are not mirror images of the underlying reality, but rather extensions of our biological brains.

But that is going in a direction I don't want to go down at the moment. In any case, don't read Rorty first! Read James' little book Pragmatism to start off with. More immediately, with respect to the present Nagelian discussion, read "Genesis and the Quantum" on pages 122–123, the dialogue between Adam and God on pages 118–120, "Evolution and Physics" and "Precision" on pages 267–270, and some of Jeff Bub's expressions on the idea in Chapter 9, most notably pages 139–140 and 141–142—all these things in the samizdat I sent you. The game of ASSUMING the possibility of a detached observer, as Nagel does, is just that: a game of assuming. Thereafter, Nagel tries to make sense of our more personal worlds in spite of this. The pages I've just referred to in my samizdat try to sketch what quantum mechanics might be talking about if one does not make such an assumption. In fact, they try to justify NOT making the assumption at all. I hope from these readings you will get the impression that though there may be a fundamental disagreement

between Nagel and me at the outset, such a disagreement does not necessarily amount to a copout on my part.

Finally, let me paste in a note below that I sent to David Mermin and Rüdiger Schack the other day. It's titled "Me, Me, Me!" and gives my very latest attempt to express the content of quantum mechanics in these lights. I think it does an adequate job ... but experience tells me I am always over-optimistic. In any case, it is directly related to all that was said above.

I'll be back again after completing Nagel and your paper.

25-08-03 *Coordinate Systems* (to G. L. Comer)

I read two things this morning that put me in the mood of coordinate systems. The first was a remarkable letter from Pauli to Bohr, dated 11 March 1955. The second was your latest draft of "linear structures." I'm just trying to get these things clear in my head and put them into a common language. (By the way, I'm sorry I've been so absent lately. Unfortunately, after this brief return I'll probably be absent again for a while; I've got to get a book review to Physics World by September 4, and I am far, far behind on my ARDA roadmap duties. Also I've got to finally get my application off to the Bohr Institute this week—a much more difficult process than you might imagine. In total I'm going to be inundated through mid-September.)

Anyway, I want to attach a PDF file containing two letters on quantum mechanics that I've written recently (one to Mermin and Schack, and one to Sudbery and Barnum), along with the Pauli letter mentioned above. Strangely enough—you must get tired of my saying this!—I feel like I've had another epiphany in expression. My two letters try to capture that.

Now for the common language business. What I found remarkable upon rereading the Pauli letter—I last read it maybe a year ago—was his discussion of coordinate systems and the way he alludes to calling them "actions on the part of the observer." Or, at least in my cherry-picking mood, that's what I would like to glean from the letter (ignoring that he did also use the word "knowledge"). You'll see why that takes me when you read the letter I wrote to Mermin and Schack. There I explicitly identify the set of POVMs (i.e., the full set of quantum measurements upon a system) as the set of ACTIONS one's *will* can take upon the system.

Clearly, Pauli sees the coordinate system as the classical analog of the POVM (and vice versa, the POVM as the quantum analog of the coordinate system). They are both mathematizations of "the observer," or better "the agent," in the respective theories. But there are differences. For one, classical coordinate systems are completely passive—this Pauli recognizes. Equipping a manifold with a coordinate system does not change the manifold or any of its contingent properties (i.e., any of the fields living on it). On the other hand, POVMs are anything but passive. This was a major point for Pauli. A POVM elicits the world to do something it would not have otherwise done. The combination of the action of one's will (the POVM) with a system external to it gives rise to a birth of sorts—the flash, the event which we usually call (in older language) the measurement outcome.

What is funny, however, is Pauli's discussion of Einstein. As far as I can tell, what he intends by the word "consequent" would better be expressed by "consistent." (Actually, upon looking up the word in my dictionary, I find that this is its very meaning!) Consequently I find Pauli's contention that Einstein was being inconsistent puzzling. The very reason Einstein felt no qualms about introducing coordinate systems when necessary was because in relativity theory the underlying observer-free description—i.e., the manifold equipped with its fields—had been achieved. In contradistinction, this is something quantum theory has yet to achieve (and may never be able to). In fact, it was this that troubled Einstein ... saving him from inconsistency.

Let me belabor the similarity and distinction. I think you are certainly right to point out one

of the similarities via the mutual use of projections in the two theories. In the case of relativity theory, a projection is used to tell what one WILL "see" from one's perspective—you gave energy as an example. The projection is an operation used for expressing an aspect of a preexisting reality, eg., a tensor field. In the case of quantum mechanics, one projects one's state of knowledge (i.e., one's density operator for a system) onto the potential consequences of one's action (i.e., onto the POVM elements) to obtain a probability distribution. This is just the Born rule in a generalized setting. The probability distribution so obtained expresses one's expectations for the consequences of one's action, not one's expectations for a preexisting reality.

So, projections both, but they encode very different things.

Let me close with another distinction that I'm wondering what to make of. It's connected to a point in the Anti-Växjö paper I sent you and a point in your latest draft. As far as I can tell, I believe both:

> **Me:** The OBJECTIVE content of the probability assignment comes from the fact that *no one* can make *tighter* predictions for the outcomes of experiments than specified by the quantum mechanical laws. Or to say it still another way, it is the very existence of transformation *rules* from one context to another that expresses an objective content for the theory. Those rules apply to me as well as to you, even though our probability assignments *within* each context may be completely different (because they are subjective). But, if one of us follows the proper transformation rules—the quantum rules—for going to one context from another, while the other of us does not, then one of us will be able to take advantage of the other in a gambling match. The one of us that ignores the structure of the world will be bitten by it!
>
> **You:** Even if the observers are at the same place, at the same time, when they make their measurement, they will in general not record the same energy because their motions will in general be different. So how can they ever agree on anything? Relativity tells them how to "translate" one measurement into the other so that a consistent description results. What is objective about the energy? Nothing. It's value depends on the motion of the observer. Where is the objectivity? Only in the rules of translation. The only objective thing to me seems to be how one object is to be compared with another.

I.e., both theories make use of transformation groups in a crucial way. But what different ways! In the case of relativity, I guess I would have said that the translations are for external purposes. They tell me how to connect one observer's potential observations to another's. Or another way to say it, they tell me how to translate from one aspect of the preexisting reality to another. In the case of quantum theory, however, the translation is all internal: What should I believe of this, given that I believe such-and-such of that? The quantum transformation rules I am thinking of here say nothing of how to translate one person's beliefs into another person's. In fact, the whole point of personalistic probability is that there is IN PRINCIPLE no way to connect someone's beliefs to anyone else's: Probabilistic statements are personal betting strategies.

Thus I guess I find no warrant in what Pauli says when he tries to make the point: "That the situation in quantum mechanics has a deep similarity with the situation in relativity is already shown by the application of mathematical groups of transformation in the physical laws in both cases."

But then again, I start to wonder that if I took Pauli's starting point completely seriously—i.e., if I were to start to think of coordinate systems as ACTIONS of an individual observer—all my troubles would melt away and maybe I'd start to see a deeper analogy as he saw it. I guess I'm still very much in the process of thinking.

Enough for now. (Don't forget the attached files—the one already mentioned plus one further one relevant to the Sudbery discussion.)

Letter from Wolfgang Pauli to Niels Bohr, dated 11 March 1955

Dear Bohr,

I find your letter of March 2nd very youthfull, which is just the reason that it is not easy for me to answer. Although we have the same view "as regards the fundamental physical problems which fall within the scope of the present quantum mechanical formalism" and although I agree with some parts of your letter, the situation is now complicated by your use in a publication of a phrase like "detached observer" (without comment!) which I used already in some publications in a very different way. I believe that this should be better avoided to prevent a confusion of the readers[29] and I don't cling at all to particular words myself. I also felt, already before your letter arrived, that my brief characterisation of the observer in quantum theory as "non-detached" is in one important respect misleading. As is well known to both of us, it is essential in quantum mechanics that the apparatus can be described by classical concepts. Therefore the observer is always entirely detached to the *results* of his observations (marks on photographic plates etc.), just as he is in classical physics. I called him, however in quantum physics "non-detached", when he chooses his experimental arrangements.[30]

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

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

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

There is an important *agreement* between us that we find Einstein not consequent in this formulation of the "ideal E". Indeed, there is no a priori reason whatsoever to introduce here a difference between the *motion* of the observer on the one hand, and the realization of specified experimental conditions by the observer on the other hand. If Einstein were consequent he had to "forbid" also the word coordinate system

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

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

in physics (as not being objective). That the situation in quantum mechanics has a deep similarity with the situation in relativity is already shown by the application of mathematical groups of transformation in the physical laws in both cases.

In this way I reached the conclusion to distinguish sharply between the "ideal of an objective description" (meaning science) on the one hand (which I warmly supported just as you do) and the "ideal of the detached observer" on the other hand (which I rejected as much too narrow).

What really matters for me is not the word "detached", but the more active role of the observer in quantum physics, which is already implied in your [constatation?] of the "indivisibility of the phenomena and the essential irreversibility involved in the very concept of observation". According to quantum physics the observer has indeed a new relation to the physical events around him in comparison with the classical observer, who is merely a spectator: The experimental arrangement freely chosen by the observer lets appear *single* events *not* determined by laws, the ensembles of which are governed by *statistical* laws.[31] It is not relevant to me, if you say the same thing using *different* terminologies (but please use [essentially?] different words than I). They will only confirm my statements again as all these statements on the observer are part of an "objective description".

I confess, that very different from you, I do find sometimes scientific inspiration in mysticism[32] (if you believe that I am in danger, please let me know), but this is counterbalanced by an *immediate* sense for mathematics. The result of both seems to be my kind of physics, whilst I consider epistemology merely as a logical comment to the application of mathematics in physics.[33] Thus when I read a sentence as "how to eliminate subjective elements in the account of experience" my immediate association is "group theory" which then determines my whole reaction to your letter. Although the first step to "objectivity" is sometimes a kind of "separation", this task excites in myself the vivid picture of a superior common order to which all subjects are subjected, mathematically represented by the "laws of transformations" as the key of the "map", of which all subjects are "elements".

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

From March 16th till about 27th I am away in Germany and Holland and when I come back I hope either to see you or to hear from you (I wrote to Basel to get informations on your lecture there).[34]

Hoping that you will in the future (just as I do myself) enjoy the enrichment coming from the different kind of access to science by different scientists, expressed in different,

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

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

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

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

but not contradicting terminologies, I am sending, also in the name of Franca, all good wishes to yourself, to Margrethe and to the whole family,

as yours complementary old

W. Pauli

31-08-03 *Acceleration is Intervention!* (to G. L. Comer)

What is it about Sundays that sets off the thought muscles? Anyway, I hope my title says it all. That was the thought that hit me as I was washing the dishes this morning. Maybe coordinate systems are interventions after all, when considered in the quantum context. [Cf. last Sunday's discussion.] What happens when a "particle detector" is accelerated? You've told me before that it goes click. The usual explanation goes that it has something to do with the perceived quantum state of the field – namely it becomes something like a thermal density operator. But maybe instead, we should think of the process as a change in the character of the measurement device. That is, maybe an accelerated particle detector is a distinct POVM from its inertial cousin. I.e., it intervenes on the field in way different than the inertial detector. (In older language, that is to say, it "measures" a different observable.)

At least that's the thought that hit me.

Look at me getting pulled down the path of GR: Analogies between Hilbert-space dimension and gravitational mass, analogies between coordinate systems and measurement devices. What's happening to me?!?!

01-09-03 *Answering Correspondence with Correspondence* (to S. Savitt)

Savittism 1: *I've always (though perhaps incorrectly) thought of solipsism as the view that my mind was the only mind that exists (or that I knew exists, or that I could know exists). This way of looking at it is, of course, compatible with materialism.*

The predominant view seems to be that it connotes an extreme form of idealism in which (one) self is the only reality, or at the very least, the ground of reality. I have a correspondence with Mermin in which I compile several definitions for the word pulled from various sources; I'll attach that rather than pull it out of context.

Savittism 2: *I didn't get your remarks about Unitary Readjustment. Is the kind of updating of your beliefs that you do as the result of a measurement in QM supposed to be incompatible with standard Bayesian updating. (I thought one theme of your "Quantum Information" paper is to show that these processes were at root the same.) Alas, the details of your massive paper have begin to blur in my mind.*

Yes it is generally incompatible; the deviation represents part of the "only a little more" in the paper's title. In Sections 6 and 6.1 of my paper I deemphasized the incompatibility (or rather, "deviation from Bayes") because I wanted to first clinch it in the minds of the readers that quantum collapse has a large component that is simply Bayesian conditionalization. The unitary readjustment quantifies the "deviation from."

There is an interesting case where quantum collapse is nothing at all more than Bayesian updating: That is, when there is no physical interaction with the system being considered—for

instance in the standard EPR scenario. By making a measurement on one of an entangled pair of particles, one updates one's description for the other particle. The interesting thing one sees by writing collapse in the way I endorse is that, in this case, the unitary readjustment is just the identity operation ... which makes collapse precisely Bayesian conditionalization.

The idea is that, when there is a deviation from Bayes, what one is seeing is a mild trace of the much deeper statement: The observer cannot be detached from the phenomena he helps bring about.

Savittism 3: *Finally, despite your protest and because many will read your writing, I will make the odd remark about grammar. It's not the one you forestalled, though. It's that 'like' is a preposition, introducing a clause. So on page one of the 12 August note, I think you really should say "Maybe the best way to do this is to run through a glossary of quantum terms as I did once before..." And there's a similar mis-use on p. 2. Obviously, I don't think that Winstons carried the day on this one.*

Much appreciated. I only started learning grammar about ten years ago, and it shows. I will readjust the sentences as soon as I send off this email. Please explain the Winstons remark though. What does it refer to?

Savittism 4: *Well, maybe I have another thing to add. I have read only little of Shimony's technical work in QM, but here's someone who worked on personal or subjective Bayesianism and doubtless had some ideas as to how it applied to QM. Looking at your brief email correspondence with him in the samizdat, it occurs to me that* perhaps *your difference (if there really is a difference) as to whether an interpretation of QM must be "ontological" or not obscures the fact that his Bayesianism is like yours.*

Oh, there's quite a difference between Shimony and me on this score. But there is enough mutual understanding that progress may be had in the future. In the attachment, I'll also include some correspondence to and from Shimony. The way I would characterize Abner is that he is Bayesian (of a flavor) *about* theories, but, within a theory, he is objectivist about its uses of probability (or at least this is so for him with respect to quantum theory).

To me, making that move—i.e. giving up on the Bayesian account *within* quantum theory—is the greatest impediment to understanding. The empirical evidence I would give for this is simply the number of years Abner spent taking as serious and reasonable a search for collapse theories (say of the GRW variety). Also the lengthy discussions he's had about the "surprising" consistency between special relativity and wave-function collapse—an effect he dubbed "passion at a distance" to contrast it with "action at a distance" (i.e., trying to make it clear that one could not use wave-function collapse for signaling superluminally).

The thing Abner does not yet appreciate is the immense leading power of taking Bayesianism seriously *within* quantum theory. The way I would characterize the present stage of my research program is, "Seek and ye shall find." An awfully good example comes from the thing I pointed out above: The EPR scenario actually represents pure Bayesian conditionalization if written in the right way. Far from being anything mysterious—like passion at a distance—it simply represents the usual conception of conditionalization. In fact, it is only the method of updating one's probabilities for systems with which one makes physical contact that has any mystery or calls for an explanation. (I.e., the situations where one must insert an extra unitary update.)

So, seek and ye shall find: Look for a Bayesian reason for this or that within quantum mechanics, and one finds—my experience tells—that the theory is a much tighter package than maybe first imagined. It's only in the slight deviation from Bayesianism that things really get interesting.

01-09-03 *One Further Thought* (to S. Savitt)

One further thought. I retumbled the note I wrote you earlier this morning in my head as I was walking to work, and it dawned on me that maybe I went too far in the other direction in trying to clarify things for you. I wrote:

> Yes it is generally incompatible; the deviation represents part of the "only a little more" in the paper's title. In Sections 6 and 6.1 of my paper I deemphasized the incompatibility (or rather, "deviation from Bayes") because I wanted to first clinch it in the minds of the readers that quantum collapse has a large component that is simply Bayesian conditionalization. The unitary readjustment quantifies the "deviation from."

I want to emphasize that this deviation only concerns *updating*. However, I show in the fat paper that there is another role Bayes' rule plays in quantum measurement (just prior to the update), and in that role it is indeed "Bayes' rule full stop" that is the crucial idea. Namely, I show that there is a one-to-one correspondence between the full set of quantum measurements (i.e., the POVMs) and applications of Bayes' rule.

So, when you wrote

Savittism 5: *Is the kind of updating of your beliefs that you do as the result of a measurement in QM supposed to be incompatible with standard Bayesian updating. (I thought one theme of your "Quantum Information" paper is to show that these processes were at root the same.)*

you were more right than I was giving you credit for. There is a one-to-one correspondence between possible applications of Bayes' rule and measurement devices (or, as I expressed it in "Me, Me, Me," actions that my will can take). But, whereas in classical measurement the updating stops there, in the quantum case there is a little more.

01-09-03 *Fragmentation and Wholeness* (to N. D. Mermin)

I hope you are enjoying all the wholeness in well-being Japan and the bodhisattva can afford.

As for your fragment, I am having quite some difficulty understanding it: There was only one thing I could latch onto enough to dislike. Namely, I object to this phraseology:

Merminition 129: *It's terrible for teaching computer scientists quantum mechanics. The subject can't get off the ground with warnings that you can't be sure a measurement gate is a measurement gate, a cNOT is a cNOT, that no gates act except the ones you set up, etc.*

You still slip into a mode of language that acts as if there is some magical fact out there that is going to confirm that a cNOT is really a cNOT or not. The point is, I would just never talk this way. Let me give an example. Suppose I am talking about a single toss of a coin, and I say, "For me, personally, the probability that it will land heads is 50/50." In making such a statement, should I worry myself that maybe my personal probability is not really 50/50 after all? Or to put it another way, and borrow your sentence structure, would one ever say of classical probability theory:

> Things I don't like.
> 1. It's terrible for teaching gamblers how to gamble. The subject can't get off the ground with warnings that you can't be sure a 50/50 probability assignment is a 50/50 probability assignment, ...

In our language, a measurement gate is a prior. A cNOT is a prior. Neither are open to empirical verification beyond soul-searching. The only thing that ever needs be said to the student is, "IF your judgment of this is 'measurement gate', and your judgment of that is 'cNOT', and that is 'phase gate', etc., etc., then for coherence you must judge the outcomes of the computation with such-and-such a kind of probability." Notice the single quotes; they are important. I did not say "If your judgment of this is that IT is a measurement gate, and your judgment of that is that IT is a cNOT, . . . " The latter kind of phraseology would lead one to think that the words 'measurement gate' correspond to a FACT of which one's judgment can either be correct or incorrect.

I'll leave it at that for now. Honestly, I got quite confused on everything else you said. Perhaps Rüdiger will fair better than I did.

As you once told me, don't eat the fugu! (At least until after you clean up your fragment a bit.)

03-09-03 *Old Dreams* (to C. M. Caves)

Did I ever tell about the time when, in high school, I dreamed that I would become a Catholic priest? It really made me very, very depressed. I even had to talk to my Mom about it. I thought, what a stagnant and empty life.

I had a flashback of that dream this morning. When I opened my email I found an invitation to speak at a philosophy conference at U. Western Ontario. . . . But it wasn't that that depressed me. It was that when I looked at my calendar to see if I could make it, I found some conflicts: Just before the Ontario meeting I'm invited to speak at a philosophy meeting in Maryland, and overlapping with the Ontario meeting I'm invited to speak at a philosophy meeting in Minnesota.

It does sort of trouble me. What a stagnant and empty life, that of the philosopher.

07-09-03 *correspondentx.com* (to Correspondent X)

I have gone to your website and looked at your open letter. You seem like a sincere person—one who practices a healthy amount of self-criticism, which is one of the most important assets of a scientist.

Concerning the content of your letter however, I am not the best person to write. Unfortunately I have gathered the reputation of an enthusiastic correspondent: No doubt it comes about predominantly from my having published reams of my own correspondence. In the last year alone, I count 14 unsolicited letters from people unknown to me offering either a) to derive quantum mechanics on the cheap, or b) to demonstrate that quantum mechanics is completely wrong. This is not an exaggerated number; I keep all these letters in a single folder within my email program.

What I mean by "deriving quantum mechanics on the cheap" is a magical combination of English words—very rarely an equation—that makes the whole edifice (i.e., all those equations, relations, and mathematical structures) of quantum mechanics make sense or even, all of a sudden, become obvious.

You've got to realize it is a difficult call for me. On the one hand, you might be the one who really has hit that magical combination—one that conveys an idea that has never before been conveyed, or at least, of an old idea, says it in a way that finally makes it become crystal clear and undeniable. Who am I to stifle that? On the other hand, of your letter, I can say it didn't "feel right" to me.

Now, I could give you reasons for that, but if I were to do so I would have another correspondent on my list. And that would mean more Saturdays and Sundays away from my family as I plug

away on letters to another complete stranger who hasn't yet entertained me. It's harsh, but I have to make a judgment call or I would be inundated—I already am inundated! You ask is there a difference between POVMs and q-numbers somewhere in the middle of your letter, and I think, "If he couldn't get this straight from my paper, what good is more correspondence going to do?"

All I can say is, if you are confident of your work, publish it or post it on `quant-ph`. Someone will eventually take the bait and give you feedback, and if no one does, that is feedback in itself.

As I say, and I am serious about this, you strike me as a sincere person. I apologize if I have hurt your feelings, but I really can't take on another correspondent right now.

10-09-03 *cond-mat/0309188* (to Steve Simon)

> I've heard some buzz about the quantum measurement paper `cond-mat/0309188`. Have you guys looked at it? Any comments?

Well, I see I'm depicted as one of the good guys in their reference 8. Still I doubt they've absorbed what Caves, Schack and I have been writing about.

The way I approach the issue of quantum measurement is to first forget about quantum mechanics and talk about undergraduate probability theory. The textbook gives you a probability distribution $P(h, d)$ over two variables h and d. From it you can derive a marginal distribution over h alone; namely $P(h)$. Now suppose you gather an explicit piece of data d. That may be some information you can use for updating your expectations about h. The mechanism is simply to use Bayes' rule: You update from $P(h)$ to $P(h|d)$.

No big deal, right? So, let me ask you this: Might I have pulled the wool over your eyes any? In particular, shouldn't I tell you about the precise mechanism the brain uses for updating from $P(h)$ to $P(h|d)$? Shouldn't I give a physical explanation for the process? Isn't the undergraduate textbook treating the problem incompletely by not requiring that I fill in those missing steps?

I think you'd be crazy if you said "yes" to any of those things. The axiom of updating is primitive within probability theory. It calls for no physical mechanism behind it, and none should be sought.

With that out of the way, let us go back to quantum mechanics and quantum measurement. One can show formally that quantum measurement is precisely the process above—i.e., Bayes rule in action—modulo an extra readjustment that takes into account the idea that in quantum measurements one might actually have to touch the system one is interested in (and hence cause some kind of disturbance). This is worked out in Sections 4.2, 6 and 6.1 of my paper `quant-ph/0205039`. (Have a look at it: I'll bet money—real money—that my paper is clearer than theirs.)

Anyway, from this point of view, collapse calls for no explanation in the same way that the process of updating from $P(h)$ to $P(h|d)$ calls for no explanation. In fact, you would have to give a physical explanation for latter in order to give a physical explanation for the former—i.e., you would have to give a physical mechanism for Bayes' rule itself. And we've already agreed (or at least we should have) that that would be silly.

Now concerning the paper you've asked about: They clearly think they've done something. But I'll bet, in the last analysis, they've done essentially nothing ... except lead the reader down an infinite regress that they haven't had the guts to analyze.

Anyway, that's my take after giving the paper not more than a three-minute skim. After thinking about these issues for years, one realizes that papers like this aren't so different than the latest proposal for a perpetuum mobile. It's a question of finding the flaw, and then—and this is always the hard part—convincing the authors that it is a flaw. It's almost never worth the time.

Now, my sarcasm aside: Why has there been some buzz about this paper in the community? What's been found exciting in it?

10-09-03 *Careful Reading Sir* (to D. M. Appleby)

... will be rewarded.

Thanks for the long note and the self-esteem course. I needed the latter!

And I was happy for the former too. However, I think most of the fight you fight in those pages has to do with a Chris that was too sloppy with his words in his youth.

A better Chris to fight with is the one represented in the "Me, Me, Me" note. You might think you read it carefully, but all the evidence shows that you didn't.

In particular, I would localize a good bit of your troubles in your paragraph:

Applebyism 1: *You say somewhere that Alice has the right to assign any state she wants, just as Bob has the right to assign any state he wants. So what is to stop Bush saying he has the right to assign any probability he wants to the proposition "every person held at Guantanamo Bay is a terrorist murderer"?*

But how does that mesh with the following lines from "Me, Me, Me":

> The textbook poses an exercise that starts out, "Suppose a hydrogen atom is in its ground state. Calculate the expectation of ... blah, blah, blah." One might think it is asking us to calculate some objective feature of the world. It is not. It is only asking us to carry out the logical consequences of a supposed state of belief and a supposed action that one could take upon the system. And here's the clincher about Bayesianism. Just as no student in his right mind would find it worthy to ask why the textbook writer posed the problem with the ground state rather than the first excited state, no quantum theorist should make a big to-do about it either. It is simply an assumed starting point. An agent in the thick middle of a quantum application can no more ask where he got his initial beliefs from, than a pendulum can ask where it got its initial conditions from. The cause of bottom-level initial conditions is ALWAYS left unanalyzed. If such was not a sin in Newtonian mechanics, it should not be a sin in a Bayesian formulation of quantum mechanics.

In the words of the great Marcus Appleby, "It's really hard to believe something you don't actually believe." When—long ago!—I said that Alice had the "right" to assign, I meant that there is nothing about the system itself that determines her state for it. I did not mean that she had the right to believe something she does not actually believe.

Here's another way that I've put it more recently, in a different excerpt from the same samizdat:

> Take a good solid physicist like Steven Weinberg who stakes his career on the search for a grand unified field theory. Suppose he finds it. To find it (I presume) is to declare: The world's Lagrangian is L. Now suppose I were to ask Mr. Weinberg, "Why L? Why not M?" I know for sure his answer will be of the form, "L just is. It is the starting point. It is an ultimate fact of nature; it calls for no explanation. In any case, if it calls for an explanation, its answer must come from outside the realm of science—religion? theology?—but I see no reason to go to such lengths."
>
> Would that make Weinberg a solipsist? A sensationalist? A phenomenalist? The point is Weinberg's stance has nothing to do with any of these labels.

Similarly for the Bayesian (even of the de Finettian variety, despite the mumbo jumbo in the opening sections of Probabilismo). For him, "the prior" on any event space is treated as an ultimate fact—an ultimate fact about the agent. There is no infinite regress because, just as with Weinberg, ultimate facts call for no further explanation.

Bayesian practice—and Rüdiger and I would claim the formal structure of quantum mechanics too—is all about what to do once a prior is established. It is not about what to do before the prior is established. In the quantum mechanical case, establishing "a prior" is 1) to write down a quantum state for all systems considered, and 2) to write down a (conditional) quantum operation for all measuring devices considered.

If there are two agents, there may well be two priors in the sense above—i.e., two ultimate facts (with respect to this level of inquiry). In that sense, the priors are "subjective", but that does not take away their status as ultimate facts in this treatment. It only calls for a recognition that the facts are about the agents.

The role of the separate system and measuring device—now specializing on quantum mechanics—is that when the two are combined they give "birth" to a new ultimate fact: The "click." There is no sense, however, in which this new ultimate fact is *about* either of the agents: It has a life of its own. (In fact, it is because of these lines of thought that I sometimes call my view "the sexual interpretation of quantum mechanics.")

Now, I will make the bold claim (because I don't have three days to spend on writing a new note), that if you look carefully at the words above and carefully back at "Me, Me, Me," you will see a lot of your other troubles melt too. For instance, look harder at the glossary. Is there not something in there that corresponds to your "a way things really are"? (Though I would prefer, "a way things really come out.") And further, notice the explicit dualism in the system/POVM definitions. I'm going to claim that that dualism saves me from being *either* an idealist or a materialist.

I believe in FACTS just as much as the next guy: Quit accusing me of not. I only stress that facts are made, they do not preexist. And it is because of this that there is no one way the world is. (The capitalization in Nagel, by the way, was all my addition.)

Finally, here are two excerpts from your note that I really, really liked. I tend to agree with the first (whether you agree with it or not) and I tend to like the sound of the second.

Applebyism 2: *One final point. You say that probability assignments float above the world (or words to that effect). This is true (on your principles) however much they are updated. Rationality resides purely in the updating procedure. The assignments themselves are never rational. Not only are they not rational at the outset, before any updating has been performed. They are no more rational at the finish, after all the evidence has come in. In short, they float. They tell us nothing at all about "the way things really are".*

and

Applebyism 3: *I am tempted to say that the ordinary human world essentially CONSISTS of probability assignments. That probability, so far from floating, is actually constitutive of the world (what we normally and humanly take to be the world).*

10-09-03 *Facts* (to D. M. Appleby)

One final thought, to complete my last note. Looking back at this passage:

Applebyism 4: *One final point. You say that probability assignments float above the world (or words to that effect). This is true (on your principles) however much they are updated. Rationality resides purely in the updating procedure. The assignments themselves are never rational. Not only are they not rational at the outset, before any updating has been performed. They are no more rational at the finish, after all the evidence has come in. In short, they float. They tell us nothing at all about "the way things really are".*

though I generally liked it, I would not use the word "evidence." Facts and data are just facts and data. To say that they are "evidence" already presupposes a (subjective) probability assignment.

11-09-03 *Bayesian Beginnings* (to G. M. D'Ariano)

D'Ariano-ism 1: *I really enjoyed our discussions in Aahrus on Quantum Mechanics, and when I got back to US, I careful read your long paper "Quantum Mechanics as Quantum Information (and only a little more)" published on the Proceedings of the Växjö Conference, keeping also notes for myself (something that I do with quite few papers). I should say that I completely agree with your point of view, and I was happy to see that in many considerations that I made recently, I'm not alone. You know, my school was Masanao Ozawa, and in the last years I taught quantum mechanics of measurements using the Bayes principle. However, after reading your paper I really became a strenuous Bayesian (including tomography).*

Thank you so much for your heartwarming letter. I'm really glad you got something out of my paper. I've been really enjoying your new ones on the structures of POVMs too: But I already told you that.

Yes, it would be great to visit Pavia. Maybe we could work out a good time in the Spring.

D'Ariano-ism 2: *Sorry if I bothered you. My problem is that now I became too much involved in such re-considerations, that I can hardly think to anything different.*

You didn't bother me at all. This is what I live for! They say every parent wants a better world for their children—a better world than they themselves grew up in. This is true: I want to see a complete understanding of the structure of quantum mechanics developed before my daughters go to college.

We're going to do it!

17-09-03 *More Than Semantics, I Think* (to A. Peres)

Asherism 6: *Information is not an abstract notion. It is a physical object which requires a physical carrier, and in particular is* localized.

Our disagreement is localized in your phrase (effectively) "information is a physical object." I would say the thing that we have learned from quantum information is that we cannot, even in abstracto, ignore the physical properties of information CARRIERS. That was something ignored in the original Shannon information theory. But recognizing that is a far cry from saying "information is a physical object" ... which, to me at least, conveys the idea that information is something that can exist independently of the agent possessing it.

At the end of your article, you write: "Quantum states are not physical objects: they exist only in our imagination." True enough. But, by the view we expressed in our *Physics Today* Opinion,

a quantum state is nothing beyond one's information about a system. Here's one of the ways we said it then:

> From this example, it is clear that a wavefunction is only a mathematical expression for evaluating probabilities and depends on the knowledge of whoever is doing the computing.

It seems to me that if you are going to claim that information is a physical object you will be stuck in an inherent contradiction (at least in phraseology).

18-09-03 *Instrumentalism* (to A. Peres)

After reading your new article, I looked up the word "instrumentalism" in my *Encyclopedia Britannica*. Here is what I found.

> **Instrumentalism:** also called Experimentalism, a philosophy advanced by the American philosopher John Dewey holding that what is most important in a thing or idea is its value as an instrument of action and that the truth of an idea lies in its usefulness. Dewey favoured these terms over the term pragmatism to label the philosophy on which his views of education rested. His school claimed that cognition has evolved not for speculative or metaphysical purposes but for the practical purpose of successful adjustment. Ideas are conceived as instruments for transforming the uneasiness arising from facing a problem into the satisfaction of solving it.

19-09-03 *de Finetti vs. Jaynes* (to D. Poulin)

Poulinism 7: *In thermodynamics, the hypothesis of exchangeability seams well motivated, for noninteracting systems at least. Therefore, if one was to measure a complete set of observables, one would be left with a product state of the form* $(\rho \otimes \rho \otimes \rho \otimes \cdots)$*, i.e. the "probability distribution over states" appearing in the de Finetti representation would have converged to a delta. But if one measures less observables, e.g. a single one, his state assignment should not be a product space unless his "prior distribution over state" has a very special form, which I wouldn't know how to justify. On the other hand, Jaynes tells us that after having measured say the average energy, our state assignment should be a product state as above with* $\rho = \exp(-\beta H)$*. As you know very well, this is done by maximizing the entropy given the constraint observed: the product emerges as a consequence that the systems are not interacting.*

I understand your problem, and it's a mistake a lot of people make. The main point to keep in mind is that a max-ent assignment is a SINGLE system assignment. One cannot extend it to a multi-system assignment unless one is confident that further measurements will reveal no further information useful for updating.

There is a fairly thorough discussion of this issue in `quant-ph/0010038` by Brun, Caves, and Schack, though in a different context than the one you bring up above. The substance of the issue is the same, however. The Horodecki's had made the assumption that max-ent gives a multi-system assignment and it caused them to get a nonsense result. But rather than thinking hard about where they might have gone wrong, they just plowed ahead and declared the max-ent program to be "wrong" in quantum mechanics.

A more important thing to read is Jaynes' own discussion of the issue. You can find that in reference 13 of the paper above.

Poulinism 8 : *How are you? Here, things are going very well, many interesting research projects on the way. As always, I am preaching for the epistemic interpretation of quantum mechanics. Believe it or not, I have almost convinced Valentini that this is the right way to think about states.*

Now this is very interesting!! The last I remembered, you were still fairly agnostic on the issue. I'm really glad to hear of this.

I've been doing some fairly technical things lately. In particular, I've been trying to shore up my old idea that quantum mechanics looks the way it does because it is predominantly about how we should manipulate and update our information in light of the fact that quantum systems have a kind of "sensitivity to the touch." Thus I've been playing with this measure of quantumness that Sasaki and I posted on `quant-ph` a while ago. In the coming month I'll put another paper on `quant-ph` showing how the quantumness of a Hilbert space (by this measure) connects up to doing quantum mechanics on a simplex (remember the pictures I incessantly drew in Montreal?).

The deeper thing on my mind, though, has been how this "sensitivity to the touch" is universal ... in a weird analogy with gravitation. An 18 dimensional Hilbert space, for instance, does not care whether it is embedded in copper or gold. Just like a given amount of gravitational mass does not care if it is embedded in copper or gold. (I use copper and gold because I think those were the metals used in the Eötvös experiment; I should look it up.) This intrigues me to no end. I'm going to give a talk at Caltech on it in October; I wish you could be there to keep me on my toes.

Also Schack and I are finally distilling my big thing "Quantum States: What the Hell Are They?" into a single paper. The tentative title is "On Quantum Certainty." In it we'll tackle the Penrose argument head on and also how quantum operations are epistemic in exactly the same way as quantum states. Your revelation of non-agnosticism, by the way, was the second piece of good news I read today. Workers of the world unite!, I say. Good hearing from you, and I hope the max-ent references given above will make the answer to your question clear—I think they will.

22-09-03 *Cosmology* (to C. H. Bennett)

Here's something I've been wanting to tell you about for a while, but I kept forgetting. Do you recall the conversation we had after our dog Wizzy died? You thought briefly that I had beat around the bush with Emma, not quite telling her the truth about the event. (I'll place the old note below in case you don't remember.)

Anyway, about six months to a year after that—I wish I could remember precisely when—Emma was hanging out in my office one day, kind of swinging back and forth with the door, and out of the blue asked, "Dad, will you die one day?" That one was a tough question: I knew I had to say yes, but I didn't want to say yes. In the end, I told her, "Yes, but you shouldn't worry about it so much; it won't happen until you're grown and have children of your own." Now, that WAS beating around the bush—of course I couldn't know when I'm going to die. Funny though how I thought the promise of having one's own children at the time would soften the blow.

The reason I'm telling you this story after all this time is because Saturday seemed to mark another, perhaps not unconnected, event in Emma's development. For the first time she asked me a cosmological question. She's about 4 and 2/3 years old now. She asked, "What was here before the dinosaurs?" I explained a little about the earth before life, and about how the earth was formed in the formation of the sun. Then she asked, "What was here before the sun?" I said, "Ah, you see the pattern: You can always ask a 'before' question, can't you?" She said, "Wow!" (Not a lie.) Then I said, "Well I don't really know to any extent what was here before the sun," deciding to skip a discussion about the big bang and all that. I guess she decided to have some fun with me because then she came back with, "You don't know what happened before the sun??!" I said,

"Nope, no one does really," deciding to be metaphorical for the moment—i.e., switching the sun and the big bang in my mind. She questioned in surprise, "Not even William James?" I said, "Not even William James."

23-09-03 *The Trivial Nontrivial* (to S. Savitt)

Savittism 6 : *Secondly, I realized (as I had not before) that a good way to think about the issue I'm currently struggling with (in philosophy of time, of course) would be to see it as trying to reconcile subjective (or, at least, perspectival) and objective views of time. So, I am using that slender excuse to send you a copy of my paper-in-progress. If you do look at it, you won't see the connection till you get to the second half, the constructive argument.*

Well, I tried to give your paper a shot over the weekend, but it was pretty tough going for me. At times like this, I really feel the lack of a philosophical training! It can be a real impediment. Probably the best critique I could muster for you would be to point out two typos ("Temporal Ontolgy" and "simpliciter, on might say") and tell you to take away the hyphens behind your nons. (Except for possibly "non-usual," which is a non-usual word anyway.)

More seriously, I wonder if you could help me classify the view of George Herbert Mead presented in his little book *The Philosophy of the Present*? For the moment it strikes as neither a species of presentism nor a species of eternalism (to the extent that either of those terms are coherent, or more to the point, to the extent that Mead is coherent!). I'll attach a copy of a passage I scanned out of Arthur Murphy's introduction to the book that I just happen to have it in my computer (for my "Resource Material for a Paulian-Wheelerish Conception of Nature" project)—you've probably long since forgotten about Mead.

Weirdly I'm starting to take this view more seriously than I had before—even though I have previously used a version of it via John Wheeler's slogan "the past exists only insofar as it is recorded in the present" for dramatic purposes. (See the "Postpartum" chapter, pp. 680–681, at the end of my samizdat.)

Of course, the reason I'm taking it weirdly seriously has to do with quantum measurement theory. It has to do with the Bayesian updating I wrote you about previously. One of the things I emphasized there is that quantum updating (upon the recognition of a measurement result) can be viewed as Bayes rule simpliciter—thanks for teaching me that word—as long as the quantum system whose quantum state is being updated is causally disconnected from the measurement action. That is true. One thing I did not emphasize, however, is the nature of the argument, x, for such a probability function, $P(x)$—i.e., the one we are talking about using Bayesian conditionalization upon.

The value of the random variable x cannot be viewed as a pre-existent fact of which we are ignorant. Rather it must be viewed as a potential consequence of our actions (i.e., measurements, in older language). Our ignorance is about what will be the actual consequence of our actions, not about what is pre-existent. Well, the same holds true when we are using the outcomes of our quantum measurements here and now for updating our quantum states for something there and then. The updating does not update our ignorance of what was existent there and then, but rather updates our predictability of what would come about *were* we to interact with that system there and then. Unfortunately, the latter we cannot do. The best we can do is to wait for the system to move into our present and interact with it then.

I think this is an important point. Let me try to put it another way: Our quantum measurements in the present never tell us about the past. They only tell us about the consequences of the past for any of our other measurements in the present.

I wish I could put it more clearly than that, but that's the best I can do for now. Despite first appearances, I don't think this is a species of presentism: It does not say "only things in the present exist" (unless it is going to be in your non-usual way). It just says, the quantum states I write down are always ultimately about the present: They gauge my expectations for what *would* happen as a consequence of my reaching out and touching my systems *now*.

Maybe I'll let it go at that for the moment.

From: "Introduction" to George Herbert Mead's *The Philosophy of the Present*, by Arthur E. Murphy

> The present is to be taken as the locus of reality. This means, I take it, that to consider anything as real is to consider it as existing in, or in relation to, a present. Now what, in relation to any present, is the status of its past? This is not to ask what it was when it was present, for then it was not past and did not stand in that relation by virtue of which it acquires the status of pastness. The past of an event is not just an antecedent present. This is Mr. Mead's main thesis throughout, but it does not often get as clearly expressed as in the following statement. "When one recalls his boyhood days he cannot get into them as he then was, without their relationship to what he has become; and if he could, that is, if he could reproduce the experience as it then took place, he could not use it, for this would involve his not being in the present within which that use must take place. A string of presents conceivably existing as presents would not constitute a past."
>
> The distinctive character of the past in its relation to the present is manifestly that of irrevocability. As conditioning the present, as making its occurrence possible, the past must have been of a determinate character. It expresses the settled condition to which the present must conform and without which it could not have been what it is. And this means not merely antecedent occurrence, it means causal determination or, as Mr. Mead tends to put it, the "carrying on of relations." The past is that out of which the present has arisen and irreversibility—the appeal might here have been made to Kant—has its critical value in terms of such conditioning.
>
> Yet this carrying on of identical relations is never the whole story. The doctrine of emergence asks us to believe that the present is always in some sense novel, abrupt, something which is not completely determined by the past out of which it arose. A present, if it is really new at all, will have in it an element of temporal and causal discontinuity. Recent quantum physics has taught us to believe that such indetermination is quite consistent with rigorous physical analysis. But how is it possible to reconcile this novelty with scientific determinism?
>
> The answer to this question supplies the basic principles of the theory. Before the emergent has occurred, and at the moment of its occurrence, it does not follow from the past. That past relative to which it was novel cannot be made to contain it. But after it has occurred we endeavor to reconstruct experience in terms of it, we alter our interpretation and try to conceive a past from which the recalcitrant element does follow and thus to eliminate the discontinuous aspect of its present status. Its abruptness is then removed by a new standpoint, a new set of laws, from which the conditions of our new present can be understood. These laws could not have been a part of any previous past, for in the presents with relation to which those pasts existed there was no such emergent element. To assume a single determinate past to which every present must wholly conform is to deny emergence altogether. But at the same time, to treat the

emergent as a permanently alien and irrational element is to leave it a sheer mystery. It can be rationalized after the fact, in a new present, and in the past of that present it follows from antecedent conditions, where previously it did not follow at all. As the condition of the present, the past, then, will vary as the present varies, and new pasts will "arise behind us" in the course of evolution as each present "marks out and in a sense selects what has made its own peculiarity possible."

Is there any contradiction between this novelty of the past and its essential irrevocability? None at all, for the two apply in different senses. The irrevocable past is the past of any given present, that which accounts for its occurrence. Its determining conditions will be ideally if not actually fully determinable in the present to which it is relative. But when a new present has arisen, with emergent facts which were really not contained in the former present, its determining conditions, hence its past, will of necessity be different. The determinism then holds of the past implied in any present, the emergence in the relation of one such present, with its past, to another.

This hypothesis, in Mr. Mead's opinion, has two main advantages. In the first place it accounts for the attitude of the research scientist toward the data he is describing, an attitude otherwise highly paradoxical. The laws of any science do in a sense reconstruct the past out of which its given elements have arisen. So much is assumed in the establishment of determinate laws, and for the scientist to suppose that the present did not follow from the past in terms of the laws he had established would be to deny their adequacy to the data they interpret. So far as it goes in any field science tends to be deterministic. Yet this "following" of present from past is wholly relative to the data on which the interpretation is based, and the scientist looks forward with equanimity to a new interpretation, and hence a new past, relative to the emergent data which the future will supply. And this combination of relative determinism and future reconstruction which holds for the research scientist, holds also, on this theory, for the nature he is describing.

Secondly, this view is in harmony with the emergence of novelty in experience, and the reorganization of experience in terms of it. This is the theme of the first Supplementary Essay. Even those who "bifurcate" nature most relentlessly must admit that in experience data may appear as intrusive elements in a world which has, in its present constitution, no place for them. They stand in contradiction to that world as currently interpreted and set a problem for reconstruction. To interpret the world exclusively in terms of the conditioning objects which a given period has isolated as the permanent background of becoming is to relegate novelty to a merely subjective experience. But in the case of data relevant to his own problems a scientist makes no such bifurcation. Rather does he treat the data as provisionally isolated in a world that does not now account for them, but as candidates for admission to a reconstituted world which may make the facts previously rejected the very center of its interpretation. So it was, for example, in the status of the Michelson-Morley experiment, first in its relation to classical mechanics, then in the theory of relativity. Within experience new objects are continually arising and a new present reorients the settled conditions of an older era in the light of its discoveries. And if the past is this orientation of settled conditions with respect to present data, the past does empirically change as evolution proceeds. This empirical description has been a part of Mr. Mead's philosophy for many years. The novelty of the present account arises from its correlation with the structure of temporal reality as such, in the relation of a determining past to an emergent present.

At this point the reader will be all too likely to object that it is clearly only our

viewpoint or interpretation of the past that has altered here. The past in itself has surely not been changed by the new way in which we have come to look at it. This however is just the distinction that Mr. Mead's whole analysis attempts to supersede. For a temporalist philosophy the past "in itself" is not a past at all—its relation to the present is the ground of its pastness. And this relation is empirically a causal one. If becoming is real that causal relation is never such as to exclude emergence. When emergence occurs a new perspective of the past, a new relatedness, will ensue—a relatedness which is a natural fact about the new situation, though it could never have occurred in the old. And what is here new is precisely the way in which what, in the older present, was merely novel and abrupt has become a part of the world of causal objects, hence a part of the past through which they are supposed to operate. The relatedness is real, and the perspective past it generates, the past of the new present, is the real past of that present, and only for a present can the past be real at all.

Mr. Mead's most objective version of his thesis occurs in Chapter Two, in the contrast between the past as relative to a present and the past as absolute. He holds, especially in criticizing Alexander, that the past which physics requires is simply the expression of identical relations in nature, not an antecedent environment, existing in itself and giving rise, in its isolated being, to all subsequent reality. Space-Time in Alexander's metaphysic, seems to be a mathematical structure taken out of relation to the physical data it interprets and transformed, in all its abstract independence, into a metaphysical matrix from which all the complexities of nature are somehow to be derived. This, on Mead's view, is just what the past "in itself" would be, a conditioning phase of natural process turned into a metaphysical substance. The search for such a substance is not ruled out for those whom it may concern. But the research scientist cares for none of these things.

We seem, then, to have discovered in temporal transition itself a unique sort of relativity, and a set of what we are now to describe as "temporal perspectives" or "systems." Each such system is distinguished by the temporal center from which its relation to past events is organized, and they differ primarily in this, that what is external, contingent, hence "emergent" for one such standpoint will "follow from" and hence be reflected in the past of another. How are such perspectives related, and how does the transition from one to another take place? The answer can be given only when we have inquired into the nature of relativity, and into its social implications.

and

The argument returns at the end, as it should, to its point of departure. It is in a present that emergent sociality occurs. And we can now see that such a present is no mere moment of time, arbitrarily cut out from an otherwise uniform "passage of nature." A present is a unit of natural becoming; it is the period within which something temporally real can happen. What has been and what may be have their focus and actualization in a present standpoint and it is from such a standpoint that creative intelligence, transforming the novelty of emergence and the fatality of mere repetition into a measure at least of meaningful development, brings to articulate and self-conscious expression the pervasive form of natural process. It is as the scene of such process that the present is the locus of reality.

So original a hypothesis will naturally raise doubts and generate formidable problems.

24-09-03 *The Helping Hand* (to R. W. Spekkens)

Spekkensism 3 : *This allegory is especially apt for the situation in a toy theory universe, but it misses part of what is going on in quantum theory, specifically, the contextuality of quantum theory: the involvement of the observer in what is observed. My sense is that contextuality is telling us that we need to abandon part of the traditional conceptual framework of the realist, specifically, the notion of a primary quality. This notion dates back at least to Galileo and was refined after the advent of Newtonian mechanics by John Locke. It is meant to be a quality that is inherent to a system and independent of its relation to an observer. I think that many of Berkeley's criticisms of this notion are valid, but whereas his idealism seems to me to have little content or guidance for a theoretical physicist, the relationalism espoused by Leibniz may be a useful conceptual alternative. My hope is that if one takes seriously the idea that the paradigm of systems and properties should be replaced by some sort of paradigm of relations and relations among relations, then the quantum state will be found to have a natural interpretation as a state of knowledge about these relations.*

Quantum theory is whispering something important to us, and these vague ideas are the best I have come up with so far in trying to make out the subject of the conversation.

I was reading a book as I was walking home the other day, as is my habit here in Dublin, trying to avoid running into street lamps, etc., when I came across a passage that seemed perfect for this desire of yours. It was actually something I had read before but never thought to bring up to you. Anyway, I scanned the passage in this morning and will attach it as a PDF file to this note. Let me know if it helps you articulate what you're trying to get at.

For myself, it still doesn't feel sexual enough, but I have to admit it feels like a move in the right direction. It'll be interesting to hear your reaction.

> **NOTE**: The following passage was taken from Richard Rorty's article "A World without Substances or Essences"—pp. 47–71 in R. Rorty, *Philosophy and Social Hope* (Penguin, London, 1999)—but it is *not* copied verbatim. In particular, I have removed all instances of the words "Dewey," "James," "Peirce," "pragmatism," "human purpose," and a few other words of the same ilk. Also I deleted some whole sentences and paragraphs and, at least once, substituted the word "antiessentialist" for "pragmatist." The goal was to see how the passage would be received with such an ever so slightly different slant. Of course, I didn't reveal this to Spekkens at the time; I revealed it only after the experiment was complete.

We need to break down the distinction between intrinsic and extrinsic—between the inner core of X and a peripheral area of X which is constituted by the fact that X stands in certain relations to the other items which make up the universe. The attempt to break down this distinction is what I call antiessentialism. For antiessentialists, there is no such thing as a nonrelational feature of X, any more than there is such a thing as the intrinsic nature, the essence, of X.

In the rest of this essay I shall be trying to sketch how things look when described in antiessentialist terms. I hope to show that such terms are more useful than terminologies which presuppose 'the whole brood and nest of dualisms' which we inherit from the Greeks. The panrelationalism I advocate is summed up in the suggestion that we think of everything as if it were a *number*.

The nice thing about numbers, from my point of view, is simply that it is very hard to think of them as having intrinsic natures, as having an essential core surrounded by a penumbra of accidental relationships. Numbers are an admirable example of something which it is difficult to describe in essentialist language.

To see my point, ask what the essence of the number 17 is—what it is *in itself*, apart from its relationships to other numbers. What is wanted is a description of 17 which is different in kind from the following descriptions: less than 22, more than 8, the sum of 6 and 11, the square root of 289, the square of 4.123105, the difference between 1,678,922 and 1,678,905. The tiresome thing about all *these* descriptions is that none of them seems to get closer to the number 17 than do any of the others. Equally tiresomely, there are obviously an infinite number of other descriptions which you could offer of 17, all of which would be equally 'accidental' and 'extrinsic'. None of these descriptions seems to give you a clue to the intrinsic seventeenness of 17—the unique feature which makes it the very number that it is. For your choice among these descriptions is obviously a matter of what purpose you have in mind—the particular situation which caused you to think of the number 17 in the first place.

If we want to be essentialist about the number 17, we have to say, in philosophical jargon, that *all* its infinitely many different relations to infinitely many other numbers are *internal* relations—that is, that none of these relations could be different without the number 17 being different. So there seems to be no way to define the essence of seventeenhood short of finding some mechanism for generating *all* the true descriptions of 17, specifying all its relations to *all* the other numbers. Mathematicians can in fact produce such a mechanism by axiomatizing arithmetic, or by reducing numbers to sets and axiomatizing set theory. But if the mathematician then points to his neat little batch of axioms and says, 'Behold the essence of 17!' we feel gypped. There is nothing very seventeenish about those axioms, for they are equally the essence of 1, or 2, of 289, and of 1,678,922.

I conclude that, whatever sorts of things may have intrinsic natures, numbers do not—that it simply does not pay to be an essentialist about numbers. We antiessentialists would like to convince you that it also does not pay to be essentialist about tables, stars, electrons, human beings, or anything else. We suggest that you think of all such objects as resembling numbers in the following respect: there is nothing to be known about them except an initially large, and forever expandable, web of relations to other objects. Everything that can serve as the term of a relation can be dissolved into another set of relations, and so on forever. There are, so to speak, relations all the way down, all the way up, and all the way out in every direction: you never reach something which is not just one more nexus of relations. The system of natural numbers is a good model of the universe because in that system it is obvious, and obviously harmless, that there are no terms of relations which are not simply clusters of further relations.

To say that relations go all the way down is a corollary of psychological nominalism: of the doctrine that there is nothing to be known about anything save what is stated in sentences describing it. For every sentence about an object is an explicit or implicit description of its relation to one or more other objects. So if there is no knowledge by acquaintance, no knowledge which does not take the form of a sentential attitude, then there is nothing to be known about anything save its relations to other things. To insist that there is a difference between a nonrelational *ordo essendi* and a relational *ordo cognoscendi* is, inevitably, to recreate the Kantian Thing-in-Itself.

For psychological nominalists, no description of an object is more a description of

the 'real', as opposed to the 'apparent', object than any other, nor are any of them descriptions of, so to speak, the object's relation to itself—of its identity with its own essence. Some of them are, to be sure, better descriptions than others. But this betterness is a matter of being more useful tools—tools which accomplish some purpose better than do competing descriptions. All these purposes are, from a philosophical as opposed to a practical point of view, on a par. There is no over-riding purpose which takes precedence.

Common sense—or at least Western common sense—has trouble with the claim that numbers are good models for objects in general because it seems counterintuitive to say that physical, spatiotemporal objects dissolve into webs of relations in the way that numbers do. When numbers are analyzed away into relations to other numbers, nobody mourns the loss of their substantial, independent, autonomous reality. But things are different with tables and stars and electrons. Here common sense is inclined to stick in its toes and say that you cannot have relations without things to be related. If there were not a hard, substantial autonomous table to stand in relation to, e.g., you and me and the chair, or to be constituted, out of hard, substantial, elementary particles, there would be nothing to get related and so no relations. There is, common sense insists, a difference between relations and the things that get related, and philosophy cannot break that distinction down.

The antiessentialist reply to this bit of common sense is pretty much the one Berkeley made to Locke's attempt to distinguish primary from secondary qualities. The contemporary, linguistified form of Berkeley's reply is: All that we know about this hard, substantial table—about the thing that gets related as opposed to its relations—is that certain sentences are true of it. It is that of which the following statements are true: It is rectangular, it is brown, it is ugly, made out of a tree, smaller than a house, larger than a mouse, less luminous than a star, and so on and on. There is nothing to be known about an object except what sentences are true of it. The antiessentialist's argument thus comes down to saying that since all sentences can do is relate objects to one another, every sentence which describes an object will, implicitly or explicitly, attribute a relational property to it.[35] We antiessentialists try to substitute the picture of language as a way of hooking objects up to one another for the picture of language as a veil interposed between us and objects.

Essentialists typically rejoin, at this point, that psychological nominalism is a mistake, that we should retrieve what was true in empiricism, and not admit that language provides our only cognitive access to objects. They suggest that we must have some prelinguistic knowledge of objects, knowledge that cannot be caught in language. This knowledge, they say, is what prevents the table or the number or the human being from being what they call a 'mere linguistic construct'. To illustrate what he means by nonlinguistic knowledge, the essentialist, at this point in the argument, usually bangs his hand on the table and flinches. He thereby hopes to demonstrate that he has acquired

[35]The properties usually called 'nonrelational' (e.g., 'red', as opposed to 'on the left-hand side') are treated by psychological nominalists as properties signified by predicates which are, for some purpose or another, being treated as primitive. But the primitiveness of a predicate is not intrinsic to the predicate; it is relative to a way of teaching, or otherwise exhibiting, a use of the predicate. The putative nonrelationality of a property signified by a predicate is relative to a certain way of describing a certain range of objects having the predicate. One way of putting the lessons taught by both Saussure and Wittgenstein is to say that no predicate is intrinsically primitive.

For a firm statement, of the contrasting view, see John Searle, *The Rediscovery of the Mind* (Cambridge, Mass.: MIT Press, 1992), p. 211.

a bit of knowledge, and a kind of intimacy with the table, which escapes the reach of language. He claims that that knowledge of the table's *intrinsic causal powers*, its sheer brute *thereness*, keeps him in touch with reality in a way in which the antiessentialist is not.

Unfazed by this suggestion that he is out of touch, the antiessentialist reiterates that if you want to know what the table really, intrinsically, is, the best answer you are going to get is 'that of which the following statements are true: it is brown, ugly, painful to banging heads, capable of being stumbled over, made of atoms, and so on and on'. The painfulness, the solidity, and the causal powers of the table are on all fours with its brownness and its ugliness. Just as you do not get on more intimate terms with the number 17 by discovering its square root, you do not get on more intimate terms with the table, closer to its intrinsic nature, by hitting it than by looking at it or talking about it. All that hitting it, or decomposing it into atoms, does is to enable you to relate it to a few more things. It does not take you out of language into fact, or out of appearance into reality, or out of a remote and disinterested relationship into more immediate and intense relationship.

The point of this little exchange is, once again, that the antiessentialist denies that there is a way to pick out an object from the rest of the universe *except* as the object of which a certain set of sentences are true. With Wittgenstein, he says that ostension only works against the backdrop of a linguistic practice, and that the self-identity of the thing picked out is itself description-relative.[36] Antiessentialists think that the distinction between things related and relations is just an alternative way of making the distinction between what we are talking about and what we say about it. The latter distinction is, as Whitehead said, just a hypostatization of the relation between linguistic subject and linguistic predicate.[37]

Just as the utterance of a noun conveys no information to people who are unfamiliar with adjectives and verbs, so there is no way to convey information except by relating something to something else. Only in the context of a sentence, as Frege told us, does a word have meaning. But that means that there is no way of getting behind language to some more immediate nonlinguistic form of acquaintance with what we are talking about. Only when linked up with some other parts of speech does a noun have a use, and only as the term of a relation is an object an object of knowledge. There is no knowledge of the subject without knowledge of what sentences referring to it are true, just as there is no knowledge of a number without knowledge of its relations to other numbers.

Our sense that we can know a thing without knowing its relations to other things is explained away by antiessentialist philosophers as a reflection of the difference between being certain about some familiar, taken-for-granted, obvious relations in which the thing stands and being uncertain about its other relations. Seventeen, for example,

[36]On the fundamental importance of this latter Wittgensteinian point, see Barry Allen, *Truth in Philosophy* (Cambridge, Mass.: Harvard University Press, 1993).

[37]It is useful to think of this Whiteheadian criticism of Aristotle (a criticism found in other early twentieth-century philosophers—e.g., Russell—who tried to formulate a non subject-predicate logic) as paralleling Derrida's criticism of logocentrism. Derrida's picture of a word as a node in an infinitely flexible web of relationships with other words is obviously reminiscent of Whitehead's account, in *Process and Reality*, of every actual occasion as constituted by relations to all other actual occasions. My hunch is that the twentieth century will be seen by historians of philosophy as the period in which a kind of neo-Leibnizian panrelationalism was developed in various different idioms—a panrelationism which restates Leibniz's point that each monad is nothing but all the other monads seen from a certain perspective, each substance nothing but its relations to all the other substances.

starts out by being the sum of 17 ones, the number between 16 and 18, and so on. Enough such familiar statements, and we begin to think of 17 as a thing waiting to get related to other things. When we are told that 17 is also the difference between 1,678,922 and 1,678,905 we feel that we have learned about a rather remote, inessential, connection between it and something else, rather than more about 17 *itself*. But when pressed we have to admit that the relation between 17 and 1,678,922 is no more or less intrinsic than that between 16 and 17. For, in the case of numbers, there is no clear sense to be given to term 'intrinsic'. We do not really want to say that 17, in the secret depths of its heart, *feels* closer to 16 than to numbers further down the line.

Antiessentialists suggest that we also brush aside the question of whether the hardness of the table is more intrinsic to the table than its color, or whether the atomic constitution of the star Polaris is more intrinsic to it than its location in a constellation. The question of whether there really are such things as constellations, or whether they are merely illusions produced by the fact that we cannot visually distinguish the distance of stars, strikes antiessentialists as being as bad as the question of whether there really are such things as moral values, or whether they are merely projections of human wishes. They suggest we brush aside all questions about where the thing stops and its relations begin, all questions about where its intrinsic nature starts and its external relations begin, all questions about where its essential core ends and its accidental periphery begins. Antiessentialists like to ask, with Wittgenstein, whether a chessboard is *really* one thing or 64 things. To ask that question, they think, is to expose its foolishness—its lack of any interesting point. Questions which have a point are those which meet the requirement that any difference must make a difference.

The residual essentialism of common sense may rejoin to all this that antiessentialism is a sort of linguistic idealism: a way of suggesting that there was really nothing there to be talked about before people began talking—that objects are artifacts of language. But this rejoinder is a confusion between the question, 'How do we pick out objects?' and, 'Do objects antedate being picked out by us?' The antiessentialist has no doubt that there were trees and stars long before there were statements about trees and stars. But the fact of antecedent existence is of no use in giving sense to the question, 'What are trees and stars apart from their relations to other things—apart from our statements about them?' Nor is it of any help in giving sense to the sceptic's claim that trees and stars have non-relational, intrinsic, essences which may, alas, be beyond our ken. If that claim is to have a clear meaning, we have to be able to say something more about *what* is beyond our ken, what we are deprived of. Otherwise, we are stuck with Kant's unknowable Thing-in-Itself. From the antiessentialist's point of view, the Kantian lament that we are for ever trapped behind the veil of subjectivity is merely the pointless, because tautologous, claim that something we define as being beyond our knowledge is, alas, beyond our knowledge.

The essentialist's picture of the relation between language and world drives him back on the claim that the world is identifiable independently of language. This is why he has to insist that the world is initially known to us through a kind of nonlinguistic encounter—through banging into it, or letting it bounce some photons off our retinas. This initial encounter is an encounter with the very world itself—the world as it intrinsically is. When we try to recapture what we learned in this encounter in language, however, we are frustrated by the fact that the sentences of our language merely relate things to other things. The sentences, 'This is brown', or 'This is square', or 'This is hard', tell us something about how our nervous system deals with stimuli emanating

from the neighborhood of the object. Sentences like, 'It is located at the following space-time coordinates' are, even more obviously, sentences which tell us about what the essentialist mournfully calls 'merely relational, merely accidental, properties'.

29-09-03 *Quotes that Bugged Me* (to A. Sudbery)

I give up: My backlog of email that needs replying to has grown too enormous, and I'm going to have to make some cuts.

Let me just lodge a complaint about two of your quotes that bugged me. These are:

Sudberyism 2 : *So I should keep to myself my enthusiasm for ambitious all-embracing theories of reality and not go around insulting people who don't need them, accusing them of copping out and God knows what. But I'm afraid it's still true for me that not to be curious about what makes QM work would be to stop doing science, to switch off what made want to learn about QM in the first place.*

and

Sudberyism 3 : *I have to say that I don't find the existence of objective reality so easy to dismiss as just a faith, which one could simply choose to abandon. The reason that the tension between subjective and objective is a real philosophical* problem *is that there seem to be good reasons for holding the objective view.*

Curt Replies:

1)

Let me try to reiterate the goal of the program Caves, Schack and I are developing. It is not to say that there is no world external to us; it is only to say that there are no quantum states external to us, and then to see where that leads us in understanding quantum mechanics. There is a difference; why can't you see it?

I think it would be hard to call my 59 page paper, `quant-ph/0205039`, a "lack of curiosity about what makes QM work." The whole point of the paper was that we don't have a decent handle on what makes quantum mechanics work. It then tries to systematically explore a particular line of thought—that the quantum state represents information, rather than material property. It does not eschew the very existence of material properties. It simply says that among them, the quantum state is a very bad candidate.

Here is the way I would caricature where you seem to stand in relation to me. Imagine a young student who first learned classical electrodynamics solely in terms of the vector potential. Then one day someone pointed out to him that all the physical phenomena he could see actually depended only upon the fields, not the complete vector potential after all—there is a gauge freedom. Well, flabbergasted, it felt to this student that he was stripped of something he ought to have. So he spent the rest of his life doggedly trying to find a justification for the TRUE gauge. Of course he had to give umpteen ad hoc reasons for why the true gauge could not be measured, but that was the price to pay to do science. (For if you're not trying to see the vector potential as a real property of the world, you're not doing science.) What a pity.

Likewise I would characterize where you want to go and where I want to go with quantum mechanics. You want to see the quantum state simpliciter as a representation of reality. Whereas I think we are more likely to find reality in the "differential." I.e., in the support structure in which quantum states live and in the rules for changing those states in light of how we are stimulated (by

the world external to us). The structure of those rules represent something we are assuming of the world as it is independently of us.

And wouldn't we like to know what we are assuming of the world as it is independently of us?

2)

Why do I go to pains to say things like this:

> SYSTEM: In talking about quantum measurement, I divide the world into two parts—the part that is subject to (or an extension of) my will, and the part that is beyond my control (at least in some aspects). The idea of a "system" pertains to a part beyond my control. It counts as the source of my surprises, and in that sense obtains an existence of its own external to me.

as I did in my "Me, Me, Me" note, when you nevertheless respond with things like this:

> I have to say that I don't find the existence of objective reality so easy to dismiss as just a faith, which one could simply choose to abandon.

In any case, my issue with Nagel—the big IF—is not about whether there is a world external to or beyond all agents—objective reality if you will. But whether it is sensible to assume that there can be a REPRESENTATION of it. Like the boy fixated on his vector potentials, one can always act as if there is such a REPRESENTATION from the outside—the view from nowhere—but also like the boy, one might be wasting one's time in doing so. It is my sense that quantum mechanics hints at the latter.

08-10-03 *EnNobelization* (to N. D. Mermin)

That was great news hearing about Tony Leggett winning the Nobel prize yesterday. Strangely though, it made me feel very nervous. I guess I had always seen Tony as "wasting a perfectly good mind" (I dug almost exactly that phrase in a note I had written to Asher Peres in July 2000). As I wrote to my friend Greg Comer in reply to one of his points:

> Yep, he's definitely a bread and butter physicist. And clearly very smart and rigorous and—if you could see the equations in his talks—you'd understand that he must be absolutely single-minded when it comes to calculating. It is only that all his later work is predicated on the obstinate belief that the quantum state *must* correspond to an objective property. The Nobel prize was for the sort of work you are talking about. I've only seen him in his later incarnation, i.e., worrying about (and proposing experiments for) quantum foundations. He always expresses surprise and confusion that quantum mechanics has not been seen to break down yet.
>
> Anyway, I just sent him a note of congratulations. It is a very impressive feat to so change the world as he (and the other two) have done.

Well, I guess his Nobel prize taught me! A man shouldn't be judged by his interpretation of quantum mechanics alone!

08-10-03 *Can't Resist* (to H. Barnum & A. Sudbery)

I know that I said I would not write again until I had finished reading Nagel's book, but I came across a passage yesterday in an essay of Richard Rorty's that I could not resist scanning into my computer. It deals somewhat with something Howard wrote us:

Barnumism 2: *During the "Bohmian dialogue" years ago at Hampshire (organized by Herb Bernstein), I came to a couple of important realizations mostly in the process of defining my views against "what we've all learned over the last twenty years" (which included things like "you can choose your own myth," as I recall).*

One of them was that, though I am not explicitly religious, I value "transcendence" (the term was being used as a putdown, I think). That is perhaps a nicer word for what Nagel is calling "detachment"... getting beyond your own limited point of view to an expanded point of view.... even though of course that expanded point of view is still gotten to by you, interacting with others, using more of the different modalities and apparatuses available to you. It is part of *"variety and freedom," (the truth will set you free, dontcha know) and "growth," for me.*

What I dislike most in some strains of "antirealist" modern thought, is their disdain for the value of transcendence, their desire to make everything a useful, folksy, comforting tool for humans.... not that useful, folksy, comforting are not good, but so is getting outside oneself and recognizing the vast unbelievableness of what becomes apparent as one does so....

I don't necessarily think your approach will end up negating that value, in end..... our views may be closer than it seems. I'll read on....

In the present passage, Rorty is talking about culture and politics, but he might as well have been talking about views of physical theory. In fact, in much of the rest of the book *Philosophy and Social Hope* he was.

I will return on Nagel (sooner rather than later).

Here's the passage:

> Insofar as 'postmodern' philosophical thinking is identified with a mindless and stupid cultural relativism—with the idea that any fool thing that calls itself culture is worthy of respect—then I have no use for such thinking. But I do not see that what I have called 'philosophical pluralism' entails any such stupidity. The reason to try persuasion rather than force, to do our best to come to terms with people whose convictions are archaic and ingenerate, is simply that using force, or mockery, or insult, is likely to decrease human happiness.
>
> We do not need to supplement this wise utilitarian counsel with the idea that every culture has some sort of intrinsic worth. We have learned the futility of trying to assign all cultures and persons places on a hierarchical scale, but this realization does not impugn the obvious fact that there are lots of cultures we would be better off without, just as there are lots of people we would be better off without. To say that there is no such scale, and that we are simply clever animals trying to increase our happiness by continually reinventing ourselves, has no relativistic consequences. The difference between pluralism and cultural relativism is the difference between pragmatically justified tolerance and mindless irresponsibility.
>
> So much for my suggestion that the popularity of the meaningless term 'postmodernism' is the result of an inability to resist the claims of philosophical pluralism combined with a quite reasonable fear that history is about to turn against us. But I want to toss in a concluding word about the *un*popularity of the term—about the rhetoric of those who use this word as a term of abuse.
>
> Many of my fellow philosophers use the term 'postmodernist relativism' as if it were a pleonasm, and as if utilitarians, pragmatists and philosophical pluralists generally had committed a sort of 'treason of the clerks', as Julien Benda puts it. They often suggest that if philosophers had united behind the good old theologicometaphysical verities—or if James and Nietzsche had been strangled in their cradles—the fate of mankind

might have been different. Just as Christian fundamentalists tell us that tolerance of homosexuality leads to the collapse of civilization, so those who would have us return to Plato and Kant believe that utilitarianism and pragmatism may weaken our intellectual and moral fibre. The triumph of European democratic ideals, they suggest, would have been much more likely had we philosophical pluralists kept our mouths shut.

09-10-03 *Mocking Bird* (to N. D. Mermin)

Merminition 130: *So don't underestimate him* [Tony Leggett]. *He's one of the most impressive and thoughtful theoretical physicists I've ever met. Have you ever looked at his little book "The Problems of Physics"?*

I hope you understand that the note I wrote to you yesterday was written self-mockingly. That was its whole purpose.

I hadn't seen or heard of the book before. I'll try to have a look at it while I'm at Caltech next week.

13-11-03 *Elephants* (to L. Hardy & F. Girelli)

Below I place some excerpts I dug out of my paper `quant-ph/0204146`, "The Anti-Växjö Interpretation of Quantum Mechanics." They certainly do a better job of what I was on about last night than I did ... though in my sober reading this morning they only seem slightly less drunk!

To Lucien's remark that scientific theories are cumulative—which he wanted to use as an indication that succeeding theories are better and better representations of reality—my reply might be that, from this point of view which I'm trying to develop, the more relevant concept is that succeeding theories have better "feedback mechanisms." That is, one might say that the human species is more developed than the elephant species—even though neither species was foreordained by nature—simply because humans can better adapt to the changing conditions around them. The human species has more feedback mechanisms for adjusting to the environment around it. Similarly, let us say for general relativity and Newtonian gravity. The former can survive more experimental onslaughts than the latter because its structure is more malleable and less rigid than the latter's. But I'm just shooting from the hip at the moment.

15-11-03 *Conjugalation* (to G. L. Comer)

I bet you've never used the words "lascivious" and "conjugal" in a talk abstract before. Call me crazy, but I did. (See below.) Somehow it seemed like crazy words were called for at a crazy place. I'm visiting the Perimeter Institute for a week and a half [...]

Maybe one fortunate thing has come from this visit to Perimeter: I've been reading loads of review material on quantum gravity. I have particularly liked the stuff I have read by Jacob Bekenstein and Raphael Bousso. I'm definitely tipping toward the entropy-bound side now, which I am convinced can only mean "assign a finite dimensional Hilbert space to what was once thought to be a continuous system." I think this stuff is very likely revolutionary, and I am so sorry it took me 15 years to appreciate it.

Another thing that has hit me is that with this Bayesian view of quantum operations developed in *Quantum States: What the Hell Are They?* is that there is simply no information-loss "paradox" associated with black holes. From my point of view, as long as a time-evolution map is linear,

completely-positive there is nothing to keep one from assigning it as long as it is actually one's firm belief for a system. In particular there is no requirement that the map be derivable from a unitary evolution on some composite physical system. Thus the paradox, like the black hole itself, evaporates. I'm going to try to write this up a little more clearly and pass it by John Preskill soon. I know, of course, that he won't buy it—he could only do that if he had bought the starting point, i.e., that quantum states and quantum operations are not ontic, but subjective—but it'll still be a good exercise, and I know I'll learn from his reaction (as long as it is not silence).

> Title:
>
> What is the Difference between a Quantum Observer and a Weatherman?
>
> Abstract:
>
> Not much. But where there is a difference, there lies quantum theory's most direct statement about properties of the world by itself (i.e., the world without observers or weathermen). In this talk, I will try to shore up this idea by writing quantum mechanics in a way that references probability simplexes rather than Hilbert spaces. By doing so, the connection between quantum collapse and Bayes' rule in classical probability theory becomes evident: They are actually the same thing up to a linear transformation depending upon the details of the measurement method. Looking at quantum collapse this way turns the usual debate in quantum foundations on its head: only local state changes look to be a mystery. State changes at a distance (as after a measurement on one half of an EPR pair) are completely innocent—they simply correspond to applications of Bayes' rule itself, without the extra transformation; that is, collapse-at-a-distance is nothing more than the usual method of updating one's information after gathering data. Thus the idea develops that if a quantum reality is to be found in the quantum formalism, it will be found only in the formalism's *deviations* from classical probability theory: Reality is in the difference. Time permitting at the end of the talk, I will try to sketch, without getting too lascivious, how such a reality may be best thought of in conjugal terms.

Preskill's Reply

> I was disappointed to hear that you won't learn from my silence. I may need to rethink my method of communicating ...

10-12-03 *City of Light* (to D. B. L. Baker)

Do you know it's been over a year since I've written you a proper letter? (You ought to know by now that a letter from me is not proper unless it contains at least 5K of raw text.)

I'm sure you've guessed from the title of this note that I'm in Paris. What a charming city ... even if it is full of surly waiters! I haven't been here since 1994, and then only for one day in a not-particularly-interesting part of the city. So really my memory has to reach back to 1991 for anything that remotely compares to this weekend. However even then, I would say I've grown so much and learned to appreciate Europe that it is altogether quite something new. Frankly I'm carried away in a romantic/bohemian mood. It probably also helps that I earn a lot more money now, and so don't really mind dropping 70 euro on a meal, as I did last night. It opens up exponentially more of the city, and even takes some of the edge off the waiters. Last night, a colleague and I dined next to a Picasso. It didn't make the food or the ambiance any better, but

maybe *they* gave Picasso the extra something he needed to finally (after all these years) draw my attention more than fleetingly.

The brick streets, the cheese shops, the wine shops, the cafés, the life of the bohemian. I am having a great time. And I'm reawakening slightly.

... The long weekend has come and gone, and I'm now at the airport, waiting on a delayed flight. I thought I'd have a chance to write you much more from the Parisian cafés, but a proper flux of writing juices never materialized. Looking back, what can I tell you? What do I feel like telling you? Maybe the thing that will interest you most had to do with an Italian student, Giovanni Valente, who just finished his master's degree in philosophy in Padua, writing a thesis titled, "Probability and quantum meaning: Chris Fuchs' pragmatism in quantum foundations." He'd been wanting to meet me for a while, so a month ago I sent him my travel schedule, saying that he could drop by Dublin one of the times in between. Out of the blue, I got this from him Friday:

> What about meeting in Paris? You wrote me that you have planned to go there (6-9 December) and I could be there. I've been taking a little rest after my viva and the GRE and my parents own a little flat in front of the Moulin Rouge, so it may be a good occasion to meet. Since I haven't been there for a while, I feel I could spend a few days in my favorite place. Anyway, if you think we may meet and drink something, let me know quite soon: I need to book a flight very quickly.

Well, he wasn't lying! He showed up in Paris the next day, and he actually did have a flat just across the street from the Moulin Rouge! He made me a nice spaghetti dinner and we had a couple of bottles of wine there one evening. The flat was in an old bordello. I certainly never imagined on that infamous New Year's Eve at your house so many years ago that I'd one day get an opportunity like that. Apparently the boy's father was a socialist politician in Italy for some years. Who said socialism was the equalizer of mankind?

Monday I lectured at École Polytechnique. It went well. But, the really nice thing was that they put me up in a hotel nearby. So I was in the middle of the Latin Quarter for the stay. That put a few thousand restaurants within my reach, even if only to walk by and look at (quite an experience in its own right). And I was just a mild walk away from the Pantheon, Notre Dame, the Louvre, Musée d'Orsay, Luxembourg Gardens, the Sorbonne, etc. I walked into the courtyard of the Louvre today around 4:00 PM, just after all the bustle had died down and just as the sun was moving into twilight. (The sun sets early this far north.) One could hear just a tinge of traffic noise coming over the top of the building. What a profound aloneness: I savored every second of it. I just kept saying over and over to myself "Le Grand Palais du Louvre." It is quite a monument to the power and creative force of mankind! Big buildings always do me in.

10-12-03 *First Meeting* (to M. Bitbol)

It was great meeting you the other day. As I told you then, I have greatly enjoyed finally reading some of your papers.

Let me paste below the passage from my samizdat *Quantum States: W.H.A.T.?* that refers to you. The text between the `bjb` and `ejb` symbols are quotes of Jeff Bub. Now that I understand that Jeff's "neo-Kantian" might just mean "somewhat pragmatic," I am much more intrigued than I was before. Have you written anything in English on the "blinding closeness" that Jeff mentions?

If you get a chance to have a look at it, I would greatly appreciate hearing your thoughts on sections 5 and 6 of my paper "The Anti-Växjö Interpretation of Quantum Mechanics". The paper can be found at my web page; there is a link to it below.

Bitbol's Reply

Better late than never (I hope this faithfully translates the French "Mieux vaut tard que jamais").

I thought I had to answer your mail one day, but many other tasks piled up. However, I finally found a free sunday morning and I read some of your stuff with pleasure. The "Anti-Växjö interpretation" sounded very relevant since Andrei Khrennikov is coming to visit us in CREA tomorrow! [...]

As you already know, I am disposed to agree on most of what you say. I was drawn independently to very similar conclusions by combining a radical version of Copenhagen interpretation (plus some insights from Schrödinger) and a network of philosophical readings ranging from neo-kantianism to pragmatism. Here is, to begin with, a short list of points of agreement (with some nuances):

1) Quantum mechanics is a theory of contextual probabilities: Kolmogorovian probabilities in each context, several such probability spaces being "glued" in a unified Hilbert space structure. To summarize this idea, I called QM a "Meta-contextual theory of probabilities".

2) The main terms of the theory refer to certain gambling attitudes. With some stringent constraints however: coherence and intersubjectivity (or rather inter-situationality, invariance of the formal tools with respect to changes in situations/contexts), etc. When this constraint of inter-situationality or intersubjectivity is emphasized, one lands into a central concept of Kant's transcendental philosophy: constitution of objectivity.

3) I also agree that physical theories express the conditions for our successful life within the world rather than a faithful picture of the world. Your insistence on Darwinian evolution, on viability rather than mirroring, is very close to the ideas I developed after a long contact with Varela's autopoietic theory of cognition. Here is a short statement of this theory I recently wrote:

> In the autopoietic theory of cognition, the relevant concept is not inputs provided by the external world, but only local environmental conditions for maintaining an operationally closed unit. The invariants of this type of unit do not represent any feature of the world, but rather identify with steady aspects of its own internal dynamical organisation. As for the appropriate changes of an operationally closed unit, it does not prove that the unit possesses a faithful picture of the world, but only that its internal working is viable in relation to environmental disturbances. One must redefine the "cognitive domain" of the operationally closed unit accordingly. This domain is no longer some fraction of a pre-existing world liable to representation, but a region of the environment which has co-evolved with the closed unit and in which the latter's organization may persist, develop, and reproduce despite the disturbances.

As you rightly pointed out, the structure of an elephant (a special case of autopoietic unit) does not represent its environment, but is the end product of a long history of environmental alterations and adaptative changes: it is a summary of the adaptative moves the species had to do. One interesting (and very relevant) features of this example is that the adaptation to the environment is not purely passive. The behavior of elephants, their search for better-suited environments (by migration) or even their own transforming activity, is also part of the adaptative process. The adaptation then be-

comes somehow mutual. The organism and its environment can be said to "co-emerge". In the same way, our theories are likely to be an elaborated and stabilized byproduct of a cognitive history (made of "conjectures and refutations", gambling rules and failures) which massively involves our experimental activity (our "interventions" by means of apparatuses).

The latter general overview was partly expressed in my "blinding closeness of reality", which unfortunately exists only as a book in French. But the basic idea is easy to summarize: Bernard d'Espagnat (my master in this field) told that reality is quite difficult to picture (actually, in the micro-domain, even impossible to describe) because it is somehow remotely distant from us, because it is separated by a "veil" from us. This was only a metaphor as I soon understood by discussing with B. d'Espagnat, but the happy effect of this was to trigger a diametrically opposite metaphor. My idea was that the reason why reality is impossible to describe as such is that we are so deeply and intricately immersed in it, that we do not have the opportunity of creating the objectifying distance. My motto, inspired from Wittgenstein, can be translated thus: "The subject is not facing the world; it is so much committed in it that this does not allow description." But this does allow orientation in it, or anticipation of part of what may happen for us in it.

I thus warmly approve your "oceanic" picture!

4) Reality manifests itself by its "unpredictable kicks". This is perfectly true. But, as you know, there exists another, very popular, conception of how reality occurs to us. According to many people, we reach some real structure when we have extracted an invariant from phenomena. Since it is invariant, they say, it does not depend on particular points of view, or on particular subjects, and "therefore" (I am very reluctant when I read this "therefore"), it faithfully describes some independent feature of the world "in itself".

Scientists usually combine (in various proportions) these two conceptions of reality in the very dialectic of elaboration of their theories. But they are basically wrong. And, I believe, you are completely right: the only manifestations of something like reality beyond the narrow boundaries of our little persons are unpredictable kicks. Why is it so? Because there is a logical fallacy in the usual inference from invariance to "independent" reality. Any feature of a pre-structured "independent" reality would take for us the form of an invariant; but invariants are not bound to express features of some "independent" reality. We are not even sure that "independent" reality, if this sequence of words is meaningful at all, has already a structure in store, ready to be disclosed (Rom Harre calls the micro-world "the glub" to express the idea that it is likely to be a plastic and dispositional stuff rather than a pre-structured network of actual properties). So, if the cognitive invariants do not disclose the structure of "independent" reality, what do they do? They disclose stable, viable, and intersubjective forms of our anticipations (our gamblings).

Instead of opposing two conceptions of the manifestation of reality (as unpredictable variations and as structural invariants), we should only keep one of them: yours, namely unpredictable variations. The other conception only deals with what Kant calls "objectivity" in his consistently anti-metaphysical acceptation. Reaching objectivity in Kant's sense is by no means identical to mirroring reality. This is what Kant meant when he carefully distinguished objectivity from faithfulness to the "thing in itself".

I applied this remark to the status of QM in several papers (including "Some steps towards a transcendental QM", published by Philosophia Naturalis in 1998 and available

on my website). In a paper in French about "Laws of Nature", I concluded thus:

> The boundary between what is transcendentally necessary and what is irreducibly contingent has moved beyond recognition. All the structural features of QM, including its law of evolution, are transcendentally necessary in so far as they express the conditions of possibility of any coherent (probabilistic) anticipation of the contextual phenomena obtained by means of a systematic experimental investigation. And the only element which remains definitely non-contingent is non-structural; it is the very occurrence of isolated experimental outcomes. Hence the aphorism: in QM, nothing structural is contingent, and nothing contingent is structural.

5) I also agree with you that the many-worlds picture of reality is amazingly daring and much more anthropomorphic than its supporters think. However, there are other possible meta-interpretations of Everett's interpretation than the many-worlds, and some of them proved very close to your (to our!) view. An example of them is the indexical view supported by Simon Saunders (and by me in "L'aveuglante proximite du reel"). Here, the universal state vector is not bound to describe the world as a whole, but rather to display the set of possible particular situations we may occupy within it, obtaining this or that unpredictable isolated outcome as a result of an experiment.

6) At this stage, I have to state my major point of disagreement with you: I suspect the remark you made about the "many-worlds interpretation" applies quite well to some of what you say. Let me explain this.

You write: "What I find egocentric about the Everett point of view is the way it purports to be a means for us little finite beings to get outside the universe and imagine what it is doing as a whole." I deeply agree with you.

But, then, you begin to do essentially the same as the naive Everettian. You try to describe our situation in the world from a sort of vantage point. Here is the sentence in your paper that made me suspicious: "I think the solution is in nothing other than holding firmly—absolutely firmly—to the belief that we, the scientific agents, are physical systems in essence and composition no different than much of the rest of the world. But if we do hold firmly to that—in a way that I do not see the Everettist as holding to it—we have to recognize that what were doing in the game of science is swimming in the thick middle of things."

Of course, once again, I am delighted by the Pascalian metaphor of "swimming in the thick middle of things". But you seem to take it as more than a metaphor: a definite belief about (not to say a faithful description of) the world and our position in it. You write seriously that "we, the scientific agents, are physical systems in essence and composition no different than much of the rest of the world". Isn't this a way of extrapolating one of our pragmatic - adaptative concepts, one of the concepts we need to swim with some success in the midst of the "glub" (here, the concept of a "physical system"), in order to describe everything including us as if it were seen from outside? I hear you saying something like "the world as I see it from my cosmic exile is made of physical systems and each one of us is one of these physical systems". But if we are "swimming" in the deep ocean of whatever we call "reality", we have absolutely no context-independent concept at our disposal, not even the very general meta-concept of "physical system". We must say that we ignore everything of the "thing in itself", including whether it is organized or not in a plurality of "physical systems". And we must therefore content ourselves with stating the formal conditions of our cognitive

aptitudes (within it). This latter attitude is typical of the Kantian and neo-Kantian lineage of philosophy (when one gets rid of the foundational aspect of Kant himself and hold a pragmatic variety of Kantianism, as I do). I hope you'll recognize it as a radical variety of your view... I wonder whether you'll become a member of our radical club or rather decide to stick to your position as it stands.

19-12-03 *Time to Think about Time* (to G. L. Comer)

Comerism 7: *What's that line of Wheeler's about the past? Is it something like "...the past exists insomuch as at is consistent with the present"? I've had this crazy idea about closed timelike curves, information theory, no-cloning theorems, and time travel. The craziness is this, can I perform a whole suite of information-gathering exercises today, that would in effect change the past so that it looks like I actually travelled back in time and effected the changes directly? Can nature be so cockeyed as to allow something like that? I mean, can I squeeze and prod nature in such a way that I can extract a present that is consistent with some other past? In effect, could I then "travel" back to the "past", and "kill" my grandparents? Surely nature is not this screwy.*

It was never quite clear what Wheeler was saying on this subject, i.e., whether we can change the past with our quantum measurements, or rather just change what we can say about the past. I think he flip-flopped from time to time. My own present take is that it is neither. A quantum measurement here and now cannot actually change the past there and then (whatever that might mean). That is just as a quantum measurement here cannot actually change anything physical there (i.e., a spacelike separation away). Rather with regard to time, it is all and only about the present. I would say this is inspired by Wheeler's take on the subject, but not quite the same. (Though who can say, maybe it is). The closer alliance is to the thought of the pragmatist philosopher George Mead. I'll attach a note that hopefully makes some sense of this. [See note to Savitt, dated 23 September 2003.] Reading back over it, I still like my quantum discussion, but there's no doubt it needs fleshing out. Tell me if my discussion or the Mead passage provokes any thoughts.

23-12-03 *Little Comments* (to R. Schack)

I've read your paper now. I like it; it's fine. [...]

By the way, I'd definitely like to swipe some material from this for the Matteo Paris thing. Your discussion on subjectivity has me thinking about the proper definition again. I wonder if I would now go so far as your first sentence in your section 2? Mind you, I just don't know. An alternative I'm playing with at least might go something like this: "To say that a quantum state for a system is subjective means that the state is not determined by any objective facts about the system. The quantum state, instead, takes its ground in the agent, whose beliefs the state is a compact summary of." Or something like that. It is a little more careful in that it does not say that the state is determined by no facts, but rather if it is, those facts are not determined by the actual system itself.

Here is a lovely passage drawn from near the conclusion of *The Metaphysical Club*:

> Academic freedom and the freedom of speech are quintessentially modern principles. Since the defining characteristic of modern life is social change—not onward and upward, but forward, and toward a future always in the making—the problem of legitimacy continually arises. In a premodern society, legitimacy rests with hereditary authority

> and tradition; in a modernizing society ... legitimacy tends to be transferred from leaders and customs to nature. Agassiz and the senior Holmes and Benjamin Peirce all assumed that social arrangements are justified if they correspond with the design of the natural world ... But in societies bent on transforming the past, and on treating nature itself as a process of ceaseless transformation, how do we trust the claim that a particular state of affairs is legitimate?
>
> The solution has been to shift the totem of legitimacy from premises to procedures. We know an outcome is right not because it was derived from immutable principles, but because it was reached by following the correct procedures.

And our own point is that Dutch-book coherence—and more generally, though yet to be worked out fully, quantum 'coherence'—is the correct procedure.

20-01-04 *Caltech and Peirce* (to A. Shimony)

Maybe in partial payment I should tell you that in the last couple of evenings I've been reading a fun piece by Susan Haack called "'We Pragmatists ...': Peirce and Rorty in Conversation". It's a fictitious conversation between the two men built out of quotes from their various works. It makes it quite clear how sly Rorty is being when he continually invokes the phrase 'We pragmatists ...'. It'd be nice if someone were to put together a similar dialogue between Rorty and Dewey. I sometimes wonder if the two of them are as close to each other as Rorty portrays.

23-01-04 *Flying to Texas* (to N. D. Mermin)

I'm flying to Texas as I write this to you. I'm popping in for the weekend for my Mom's 75th birthday. I start heading back Monday morning. The following Sunday I'm off for a visit to the Niels Bohr Institute.

Mostly I'm working on a proposal in the background, but I'll stop for a minute to have some conversation with you.

Merminition 131: *Somewhere in Jaynes's book (but I haven't been able to find it again) he says that to assign something probability zero is, by Bayes's theorem, to commit yourself to never updating the assignment of zero on the basis of any new information. This, of course, would be bad news for your position.*

I don't understand why you are thinking it would be 'bad news'. In fact for our understanding it is actually good news, for it tells you how opinions actually change: By Darwinism. As de Finetti points out pretty strongly in his Probabilismo, applications of Bayes' rule are not changes of opinion at all, but rather following one's initial opinion through to its consequences.

In terms of a Dutch-book scenario, if one has certainty for some event, then one is willing to bet one's life savings on it. If that event then does not occur, one has lost everything: The whims of Darwinian evolution have taken one out. And one's beliefs then do not propagate.

Merminition 132: *But I believe what he has in mind is this:*

$$p(a|bx) = \frac{p(ab|x)}{p(b|x)}$$

So if $p(a|x) = 0$*, then* $p(a|bx) = 0$ *too, for any* b*.*

What he appears to overlook is that this is only so if $p(b|x) \neq 0$. The correct statement should to be that if you assign something probability zero, then only the occurrence of something else to which you assign probability zero can lead you to update the first probability to a non-zero value (assuming you are behaving rationally — i.e. following the laws of thought.)

I was reminded of this when I read in paragraph 69 of "On Certainty":

> *. . . If I am wrong about this, I have no guarantee that anything I say is true . . .*

In the context of the other paragraphs surrounding Wittgenstein's 69, Wittgenstein's point granted. But I don't understand your 'passing thought'. You can try again, but I think my point above already negates it: two deaths don't make a life.

Merminition 133 : [Regarding Spekkens.] *Doesn't the fact that* [his toy model] *has both epistemic AND ontic states make it uninteresting?*

I think you're missing something big here. It is interesting precisely because he has ontic states. What his work helps argue is that so many of these effects we find interesting in quantum information—like teleportation, superdense coding, 'nonlocality without entanglement', the no-broadcasting theorem, entanglement monogamy, etc., etc.—come about *solely* from the epistemic nature of quantum states (including the less than full use of the probability simplex, i.e., his epistemic restriction, or knowledge-balance principle). And in that light they are not nearly as interesting as we once thought they were.

It's a question of refocusing yet again. What his work shows us is that the interesting questions lie elsewhere. In particular, it teaches us that the crucial question must be, 'Information About What?' A quantum state is epistemic alright—all these examples are meant to help you believe that—but epistemic about what? That is the great unanswered question. Of course, I myself say, epistemic about the outcomes of other interventions. But what is left to do is to make that sentence precise enough that, from it, we can rederive the structure of quantum theory. It is about formalizing THE Paulian Idea—that is where he has taught us to refocus. (Though he would likely disavow much that I've said, being one who deeply longs for nonlocal hidden variables or some such.)

Here's another way to put it. Does THE Paulian Idea (i.e., that the agent cannot be detached from the phenomena he helps bring about) find any crucial expression in the phenomena of teleportation, superdense coding, 'nonlocality without entanglement', the no-broadcasting theorem, entanglement monogamy, etc., etc.? Probably not. All those phenomena seem to have more to do with the epistemic nature of quantum states.

Instead, the Paulian Idea may well be localized solely in Kochen-Specker phenomena. (Which is something itself longing to be reexpressed in Bayesian terms, but I need to write you a separate note about that one day.)

I'll paste some related thoughts from a letter to Oliver Cohen below. [See ??]

Does any of this help you see why I am excited by Rob's work?

28-01-04 *Merminizing* (to N. D. Mermin)

Just wondering how you're doing? Did you get my note in reply to your 'passing thoughts'? I seem to have had some email problems, with people not getting my messages again lately. I hope you weren't one of them.

My own favorite paragraph from *On Certainty* comes from #30:

> Certainty is as it were a tone of voice in which one declares how things are, but one does not infer from the tone of voice that one is justified.

31-01-04 *Just the Opposite* (to N. D. Mermin)

Merminition 134: *I read in today's NY Times:*

> *Mr. Bush no longer declares, as he once did, that he is certain that sooner or later unconventional weapons will be found in Iraq.*

Can Ludwig get him honorably off the hook?

It's just the opposite! That was the part of the point of my last long note to you. For the Bayesian, the only way to back away from certainty is death. I.e., it's Darwin to the rescue!

01-02-04 *Wilczek, Einstein, and Bohr* (to G. Brassard)

I was once at a conference where someone asked Frank Wilczek what it was like to live in Einstein's old house in Princeton. The fellow asked, "Have you ever felt Einstein's ghost?" Wilczek answered, "No, but every morning I wake up a little more dubious of quantum mechanics."

I'm going to bed now. I'll tell you if Bohr enters my dreams.

03-02-04 *The Land of Bohr* (to G. L. Comer)

I'm writing to you from Kastrup airport, just as I'm leaving the land of Bohr. Yesterday I walked into Bohr's old office and had a look at his desk. Thinking back on the moment, how I wish I had sat at it, even if only briefly. I could have; there was nothing to stop me. But it didn't dawn on me at the moment that there was an experience passing me by. It reminds me of a couple of lines in The Pogues' song, "Fairy Tale of New York." The man laments "I could have been someone," to which the woman cries out in reply, "Well so could anyone!"

When I visited you in Meudon that year [1992], I went into the Cathedral of Notre Dame and tried to imagine Napoleon taking the crown away from the Pope at the last minute of his coronation. "I crown myself Emperor," he said.

I hope I won't forget the lesson so quickly this time around.

05-02-04 *Facts, Values and Quanta* (to D. M. Appleby)

Wow! Thank you for the color-full and value-laden paper. I'm just printing it out now. I'll try to respond to you as soon as possible.

Will you be at the LSE meeting Monday February 16?

Most of the stuff in my talk you will have heard before (over and over and over), but there is one point that is new (and I think noteworthy). Plus there is a line-up of several other good-looking talks. It'd be great to see you there.

BTW, regardless of content, I love your title!

06-02-04 *Replies to Your Comments* (to N. D. Mermin)

Here goes something of a reply.

Merminition 135 :

> [CAF wrote:] This is where quantum information (including the collateral fields of quantum cryptography, computing, and communication theory) has a unique role to play. Its tasks and protocols naturally isolate the parts of quantum theory that should be given the most foundational scrutiny. "Is such and such effect due simply to a quantum state being a state of information rather than a state of nature, or is it due to the deeper issue of what the information is about?"

Is superconductivity (a famous quantum effect) a state of nature or a state of information or is it due to . . . . I'm not trying to be nasty. Just suggesting thoughts that may occur to some readers.

That is precisely the sort of thing that I am asking that we examine—though I don't quite know where superconductivity fits into the quantum-informational classification scheme yet. How can I be faulted for asking the same question that your straw man is?

Merminition 136 :

> [CAF wrote:] Recent investigations by several workers are starting to show that a plethora . . .

Have even you succumbed to the use of "plethora" (which means excess, too much, etc.) simply to mean lots and lots without any negative connotations? Then the battle has been lost.

You forget that I did not know English before I started writing email in 1991, and even then I did not get a real boost in my grammar until I started corresponding with you in 1996. It is an ongoing process with me, and you have taught me the meaning of plethora. It's not a lost battle; thanks.

Merminition 137 :

> [CAF wrote:] On the other hand, other phenomena, such as the potential computational speed-up of quantum computing, seem to come from a more physical source: In particular, the answer to the question, "Information about what?"

Quantum computational speedup comes from the answer to the question, "Information about what?"? Do you really mean that?

Yep, I mean that. What is the answer to "Information about what?"? I like to say, "the consequences of our interventions into nature." But that needs to be tightened up into a more formal statement. It has it's base in Kochen-Specker, but I don't know how to carry the thought any further than that at the moment. Let me reiterate the point I made to you about Spekkens the other day: [See note titled "Flying to Texas," dated 23 January 2004.]

Merminition 138 :

> [CAF wrote:] When we finally delineate an answer to this physics will reach a profound juncture. We will for the first time see the exact nature of 'quantum reality' . . .

By putting the term in quotes I assume you mean we will finally understand what the term means. But it can also be read as saying that there is a quantum reality that we will understand. Not sure what you want the reader to take from this.

Maybe both ideas.

Merminition 139 :

> [CAF wrote:] Trickle-down effects could be the solution to the black-hole information paradox and even the meshing together of quantum theory and gravitational physics—some of this can already be seen in broad outline.

Really!? Would you care to expand on this?

I was a little more careful in my formulation this time around. The 'broad outline' part really only attaches to the black-hole information paradox business. Who would have thought that it is a QM-foundational paradox on the order of all the usual ones, but I think that's all it is. In non-Bayesian approaches to quantum mechanics, when one has a mixed state, one usually gets all fuddled up either, a) trying to identify the REAL pure state underneath the mixed state, or b) trying to seek a purification of the state that is the REAL state of some bipartite system. From the Bayesian point of view that is a fruitless exercise: There is no demand that a mixed state be derivable from any REAL pure state. All those ancillas, environments, and purifications are generally factitious.

Similarly for the Bayesian point of view of quantum operations, a la Fuchs and Schack (but maybe not yet Caves). There is no demand that a proper trace-preserving quantum operation be derivable from some REAL unitary operation on a larger system. And that is all I think is going on with the black-hole information paradox. They can't find any natural bipartite system to pin a unitary dynamics on. But so what? It, like the EPR paradox, is a pseudo-problem. It can be good for clarifying concepts, but it is not a feature of nature.

Merminition 140 :

> [CAF wrote:] Since the beginning of quantum theory, much of what the enthusiasts have called 'foundational work' has been pseudoscience pure and simple. But the field can be made as respectable as quantum theory itself, if done right. Quantum information is the technique for the task.

Why get on the defensive? You're the one who disparaged foundational work. Why should these guys share this negative view?

Just pounding on the idea that my program is quantum foundations with that little something extra (i.e., legitimacy).

Anyway, why draw attention to it? Look at one question I got in Copenhagen the other day. A fellow said, "This is all very nice for 'Sunday physics,' but what do you do for the rest of the week?" I looked him straight in the eye and told him that this was the same kind of physics that went into my calculations for Kimble's teleportation experiment. Apparently he needed to be told that.

People have to get it in their heads that this is serious physics, as serious as anything else in the now vast edifice of quantum information and computing. They're drawing an imaginary line.

Merminition 141 :

> [CAF wrote:] With regard to quantum mechanics then, the Bayesian view of probability combined with Gleason's theorem on Hilbert-space measures leads ineluctably to the idea that a quantum state is a collection of gambling commitments and nothing more.

Ouch! You've been trying for 5 years to bring me around to this point of view and I'm still not there. Try to tell a solid state physicist that the BCS ground state of a superconductor is a set of gambling commitments. She'll kick you out of her office. Try to tell a chemist that a chemical bond is a gambling commitment. Who is your audience?

Position statement. But your point is well taken. Unfortunately, I can't help it that a probability distribution is not a solid object (as we've all been led to believe from our early educations), but I can't lie about it either. That point is the core of my research program. What one sentence could I write that would either a) soften the blow, or b) make it all seem more reasonable? One sentence alone?

Merminition 142 :

> [CAF wrote:] What is already clear enough, nonetheless, is that from a Bayesian approach the formal structure of quantum theory represents not so much physical reality itself, but rather a behavior change from standard Bayesianism for gambling agents *immersed* within a quantum world. The trace of a 'quantum reality' (which we would so dearly love to formalize) must be found in the difference.

I've missed the punch line. The difference between what and what? Between behavior as described by standard Bayesianism and behavior of gambling agents immersed in a quantum world? It sounds like you're saying that there is no reality for standard Bayesians but a little bit emerges when quantum phenomena enter the story. Is this really what you mean?

Something like that. Standard Bayesianism makes no reference to anything about reality. Quantum mechanics seems to: It is a layer on top of Bayesianism that has to do with setting priors, and modifying the update rule when there is physical contact between the agent and the system he is stimulating (in old language, measuring).

Merminition 143 :

> [CAF wrote:] Quantum mechanics holds the promise of drastically changing our world view.

That happened a long time ago. Before even I was born, if you can believe it.

I don't think so. It changed physical practice long ago, but it has yet to change our world view in a widespread way. That's why there is still so much effort to make Everett, decoherence (einselection), consistent histories, Bohmian mechanics, modal interpretations, etc., etc., work.

None of those wimps have had the nerve to embrace the Paulian idea (whatever it is).

OK, that's my response to all your comments.

Now, all that said, I am a little disappointed in what I've been able to muster for this proposal. I tried to be inspiring and sober at the same time; I suspect I failed.

Writing is not an easy profession, is it?

25-02-04 *Promised Message* (to N. D. Mermin)

Merminition 144: *The following true story will appeal to nobody but you and maybe not to you either, but here it is.*

Do you remember my funny way of solving Bernstein-Vazirani (that I spoke on at the Bennett symposium)? You want to determine an n-bit number a, and are given a U that acts on a n-bit input register containing x to shift the one bit output register by the bit-wise modulo-2 inner product of a and x. My solution starts by replacing U by a bunch of cNOT gates — one for each non-zero bit of a controlled by the corresponding bit of x, all of them targeted on the output register.

Anyway I showed this to my class of physicists and computer scientists, and somebody remarked that it was pretty clumsy having to reconfigure the hardware every time you wanted to do it for a different value of a.

Instantly one of the CS students said no, that wasn't necessary. All you needed was an additional n-bit register into which you put a. The hardware was then fixed, consisting of n doubly controlled NOTS (i.e. Toffoli gates) all targeted on the output register and controlled by pairs of corresponding bits of x and a.

Thinking back on this a few days later it struck me that the way I presented it the choice of Hamiltonian (in the form of U) associated with the different possible a's was objective — different arrangements of the classical hardware. But the way the sharp CS student suggested doing it changed that choice of Hamiltonian to a specification of the state of the additional register. The selection of the Hamiltonian from among all possible Hamiltonians in his scheme was on exactly the same footing as (and in fact was identical to) the specification of a state vector.

That's all. Sounds less entertaining now that I've written it out.

Nope, a very deep point I would say ... Sounds worthy of a slogan. How about, "A quantum operation is just a quantum state in disguise." ??

01-03-04 *Thinking Out Loud, As Usual* (to H. Halvorson)

Thanks for the note. I guess we *have* never met; I've certainly seen you in the distance.

Halvorsonism 1: *In some of my current work, I'm picking up a question that has been close to your heart over the past few years — viz., how much of QM is just "laws of thought"? I'm going to have occasion to talk to some general philosophical audiences about the topic, and I'd like to give them an accurate representation of your position. So, I was wondering if I could ask you a favor: Could you point me to what you take to be the two or three (or more, if you have time!) most significant passages or results in your corpus that discuss, or bear on, this topic? In other words, where should one look first if one wants to quickly learn the correct answer to the question?*

It's a phrase I stole from Boole, you know. (See pages 527–529 and 351 in *Notes on a Paulian Idea*, Växjö U Press edition. I did send a copy of that to you, didn't I?)

It would be hard to tell you the "correct" answer because all of these thoughts are in constant transition. It's just been something I've been groping for, for the last eight or nine years. (I think I invented the phrase at a bar in Albuquerque, Jack's Liquor and Lounge, in the Fall of 1995.)

Be warned that by the phrase I don't mean something like a Kantian a priori category, i.e., a position like von Weizsacker's in his book *The Unity of Nature*. I don't mean something like, "an understanding using the terms of quantum mechanics is the precondition for possible experience." Rather I have started to toy rather strongly with a Darwinian kind of idea: Using the rules of

quantum mechanics for manipulating and updating our expectations (i.e., as a "law of thought") is the presently best known means for survival, given that we are immersed in the particular world we are. That is, I want to view quantum theory as a branch of decision theory that is contingent upon properties of the world we live in ... and it is something we locked into only in our most recent turn in evolutionary development.

Also, I should try to make it clear that, in this light, I view quantum mechanics as a normative theory in a sense akin to the one Bernardo and Smith use to describe Bayesian probability theory:

> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way.

That's the short of it.

The best reference for the long story at the moment is my samizdat *Quantum States: What the Hell Are They?*,—unfortunately the whole of it—posted at my webpage in pdf format. But that's too much material (and too loosely organized) for you. So within that, let me point you more specifically to: pages 49–50 "Note on Terminology," pages 83–85 "Replies on Practical Art," pages 144–147 "Psychology 101," and pages 150–155 "A Wonderful Life." Maybe also pages 35–38, "Identity Crisis." That might do for first pass.

Ultimately, I'd like to synthesize (and "consistify") these 235 pages of email into a single paper of 20 or 30 pages, but unfortunately that hasn't happened yet. If anything looks like sheer nonsense, or the writing is detractingly ambiguous, let me know, and I'll try to clarify for you.

Good luck. If the stuff provokes any thoughts in you, I'd love to hear them. Also, I'd love to know the sorts of things you're already thinking (that your note above alluded to).

04-03-04 *Curl Activator* (to N. D. Mermin)

Merminition 145 : *Don't know if you wanted comments on the opening serenade of your paper with Rüdiger, but here are a few:*

- (a) The Bayesian view of quantum states is that it is not: The quantum state is not something the system itself possesses.
- (b) What distinguishes this view from a more traditional "Copenhagen-interpretation style" view is that there is no pretense that a quantum state represents a physical fact.

"this view" in (b) seems to refer to (a). But it's a non-trivial jump from not being something the system itself possesses, to not representing a physical fact. E.g. the state of a quantum computer represents the initialize procedure (measure all the qubits and apply NOT to those that register 1) and the sequence of gates that have been applied. While this history is not "possessed by" the qubits (since it can't be recovered from them) it's a big step to say that the history (and the quantum state it gives rise to) is not a physical fact. (Namely your denial that gates [hamiltonians] are objective, or that what has been measured [as opposed to the measurement outcome] is objective.)

> It is the outcomes of quantum measurements that represent physical facts within quantum theory, not the quantum states.

Repeating myself, shouldn't you acknowledge here that although the outcomes of measurements represent physical facts, what it is that has been measured is not, in your view, a physical fact.

> In particular, there is no fact of nature to prohibit two different agents from using distinct pure states for a single system. [Footnote: Contrast this to the treatment of Refs. Mermin2001 and Brun2001b.]

Please cite Mermin2002 (J. Math. Phys., **43***, 4560-66) where the argument is (in response to you guys) at least explicitly made contingent on the assumption that probability 0 means objective impossibility (and, I believe, no other assumption).*

So, my words did curl your hair! Makes me proud.

Thanks for the comments. Rüdiger and I had a nice time on the phone discussing them this evening. The main thing it helped us realize is that we've gotta, gotta, gotta get that "On Quantum Certainty" paper written for you.

Anyway, the first main comment didn't cause us to make a change in the draft: The introduction wasn't the place to defend the view; we just wanted to state the view.

About Mermin2002, I've included it in the citations. I've never actually read the paper though (the dangers of not putting something on quant-ph). Could you send me the file?

Concerning this one:

Merminition 146 :

> In any case, this does not imply that a single agent can believe willy-nilly in anything he wishes. To quote D. M. Appleby, "You know, it is *really* hard to believe something you don't actually believe." Difficult though this may be to accept for someone trained in the traditional presentation of quantum mechanics, the only thing it demonstrates is a careful distinction between the terms *belief* and *fact*.

It's not clear what "this" refers to. From the syntax alone it would appear to be Appleby's remarks. This is reinforced by the "believe" (Appleby) and "belief" (you). But I assume you have in mind some or all of what you have to say before Appleby appeared on the scene.

I think you got confused by not noticing that the "Difficult though this . . . " sentence, was outside of the footnote. If you ignore the footnote it works fine. Still though I did adjust the words slightly, just in case the reader loses track after a diversion to the footnote.

Just in case you're curious, in the next email, I'll send you the completed paper. I'll have to send it as a pdf file though, so that you get the figures. As I told you before, it's pretty much a throw-away paper, using as it does nothing but old technical material.

Still, you might enjoy the Introduction, the Concluding Remarks, and Section VI on "Subjectivity of Quantum Operations." They package the story in a way you may not have seen before.

Of use to me would be to know how the "stimulus-response" imagery at the end strikes you.

> Is there something in nature even when there are no observers or agents about? At the practical level, it would seem hard to deny this, and neither of the authors wish to be viewed as doing so. The world persists without the observer—there is no doubt in either of our minds about that. But then, does that require that two of the most celebrated elements (namely, quantum states and operations) in quantum theory—our

best, most all-encompassing scientific theory to date—must be viewed as objective, agent-independent constructs? There is no reason to do so, we say. In fact, we think there is everything to be gained from carefully delineating which part of the structure of quantum theory is about the world and which part is about the agent's interface with the world.

From this perspective, much—*but not all*—of quantum mechanics is about disciplined uncertainty accounting, just as is Bayesian probability theory in general. Bernardo and Smith write this of Bayesian theory,

> What is the nature and scope of Bayesian Statistics . . . ?
>
> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way.

In fact, one might go further and say of quantum theory, that in those cases where it is not just Bayesian probability theory full stop, it is a theory of stimulation and response. The agent, through the process of quantum measurement stimulates the world external to himself. The world, in return, stimulates a response in the agent that is quantified by a change in his beliefs—i.e., by a change from a prior to a posterior quantum state. Somewhere in the structure of those belief changes lies quantum theory's most direct statement about what we believe of the world as it is without agents.

I think it's a particularly crisp way of expressing the program. Anyway, it's all perfectly tame as written down, but the phrases arose out of a discussion during my recent visit to the Oxford philosophy department, where I was extolling the virtues of the "sexual interpretation of quantum mechanics." Caught up in the moment, I said something like: "Mermin's got this thing about 'correlation without correlata', but what I'm looking for in my quantum foundations program is 'stimulation without stimulata'!" It was nonsense and didn't fit, but we had a good laugh . . . and the word 'stimulate' stuck with me, waiting to rise again on a more legitimate occasion.

07-03-04 *All Kinds of Veils* (to A. Peres)

Asherism 7: *I am absolutely elated by "my" discovery that there are no quantum states (and therefore no "problem of time" in quantum gravity, etc.). Heisenberg et al. used only algebras of operators to compute observable quantities. Then Schrodinger came and "stole the show"—and completely messed it up. He did that after Einstein called his attention to de Broglie's thesis, and wrote "he has partly lifted the great veil" or something like that. You are an inexhaustible source of references. Where is that "great veil" mentioned?*

I will look for the "great veil" reference tomorrow at the office, where I have a copy of Jammer's *The Philosophy of Quantum Mechanics*. It is a very thorough historical reference. Another place

where you are likely to find it, and other interesting things, is a little book titled *Letters on Wave Mechanics*, edited by Karl Przibram, with letters by Einstein, Schrödinger, Lorentz, and maybe a couple of others. Your library might have it. I remember finding one letter particularly amazing within it. It was a letter written by Einstein to Schrödinger, in great excitement, after Einstein's first quick reading of Schrödinger's paper on the time independent Schrödinger equation. Einstein says something like "what a great insight!" but then he quotes Schrödinger's equation *incorrectly* and complains something like, "You will note that this equation has this and this and this undesirable property. On the other hand, if you had considered this equation [where Einstein now writes the correct Schrödinger equation as if he had never seen it], all of these problems will be fixed." But then the really lovely thing that Einstein says next is something like, "However, for the life of me, I can't think of a physical interpretation for the wave function that appears in this equation."

Unfortunately, my copy of the book was burned up in the fire, and I haven't replaced it since. It's a nice little book.

08-03-04 *The Great Veil* (to A. Peres)

Apparently the quote is "lifted a corner of the great veil." However, I haven't been able to pin down the exact origin of it yet. You can at least read it here: ...

Jammer's book wasn't as much help as I thought it would be. For instance, I'm discovering how bad the index is. My method of looking was to cross-reference a) Einstein and de Broglie, and b) Einstein and Schrödinger. There a quite a few places where I found the names within the text, but no cataloguing within the index.

Actually, I just found this story at `http://www.cosmicgravity.com/part1.pdf`:

> Working on his doctorate in 1909, a young aristocrat, Prince Louis-Victor de Broglie, discovered a mathematical relationship between Planck's Constant and a yet to be observed wavelike property of moving masses. His examiners were of a mind to reject the paper, and wanting an outside opinion, sent a copy to Einstein who replied: "He has lifted the corner of a great veil." The dissertation was accepted–fifteen years later it earned de Broglie a Nobel Prize, the first ever awarded for an academic thesis.

15-03-04 *Via Alaska Airlines* (to D. M. Appleby)

I apologize for the long delay, but I've finally read your paper on probability. It happened on my flight from Vancouver to Los Angeles the last few hours. Now I'm in the Aer Lingus lounge in LAX, waiting to make the rest of my way home to Dublin.

It's a good paper! What parts were you afraid I would disagree with? I think you did the community a great service with the paper, and I'll try to advertise it as best I can. I particularly enjoyed your argumentation in Section 6 on retrodictive inferences.

A few very minor remarks.

1. Of the paragraph in Section 1 starting, "Hume famously argued that one ...", I wrote, "Good way of saying it."

2. Of the sentence in the same section, "On the face of it, taking an epistemic view of the state vector amounts to giving up on the idea of physical reality altogether," I wrote, "Yuck." Rather than "on the face of it" you might have written "ostensibly" or "at first pass". I guess "on the face of it" conveys the idea somewhat, but I fear the reader may not immediately see that you're being rhetorical. You do, in fact, recover with:

3. "However, I feel it may be consistent with a much more subtle and interesting kind of realism . . . " to which I wrote "Better."

4. In Section 4, where you say, "For instance half-lives are typically tabulated next to masses, as if they were just one more physical property," I wrote "Interesting!" That is a good point. However,

5. from that point onward, all the way to the end of the section, I could not understand what you were getting at.

6. At section 9, I wrote "Utilities!!" I think you could do a lot better in general in that discussion.

7. Finally Section 12 was far too abrupt! You had me licking my lips and then left me hanging. (It looks a little like you were exhausted by the time you got to the last section and, so, wrapped it up pretty quickly.) I was tantalized by this comparison of probabilities to qualia—I had never seen that before—and I wanted to see how you developed the point. Also, despite the mention of Bohr in the last two paragraphs, you never did come back to the point of a "much more subtle and interesting kind of realism."

All very minor points.

In an earlier note, you wrote me,

Applebyism 5 : *It is all about classical probability—coins, and the like. I do appreciate that quantum probabilities are fascinatingly and intriguingly different. And that is what I am going to start ¿hinking about now.*

I don't think you'll find any consideration in your paper that is changed in the quantum setting. ("Where did you make use of the difference between classical and quantum?," I ask rhetorically.) At least as long as you are talking about actually performed trials—rather than counterfactually performed trials (as in my bureau of standards)—I think everything will be OK. In particular if we look at things in the right way, I suspect the only difference between classical and quantum will be in their notions of event, not in their notions of probability. In the quantum case, the events are direct consequences of the agent himself (via his conjugal relations with the external world)—stimulandum, stimulation, stimulata. (Correlation without correlata? No! Rather, no stimulation without stimulata! No stimulation in the agent without a corresponding stimulation to the external world.) Somehow, taking that into account in the agent's reasoning leads to an apparent modification of the probability calculus with respect to counterfactual measurements (i.e., how he updates his beliefs about the bureau of standards).

By the way, concerning,

Applebyism 6 : *Matthew thinks that he cannot pick up a glass of wine. Only an American pragmatist could be so simple-minded—so horribly unsophisticated—as to imagine that it is possible to genuinely do anything.*

thank you! It's been a long time since I've been proud to be an American!

All the best, and it really is a fine paper. Thanks for writing it.

PS. Let me attach the latest concoction Schack and I brewed; it's something we haven't posted yet. There's not much new in it—it was a quickly pasted together article for a review volume on quantum state estimation—however you might still enjoy reading the introduction, conclusion, and Section 6 on the subjectivity of quantum operations. In particular, I'm kind of proud of the language choices I made in the first three paragraphs or so, and also in the concluding section. I

really want to rearrange how we think of quantum theory—from a big theory of everything to a little theory from the inside. A theory of stimulation and response, and maybe a little creation in the process. It's the latter part of that sentence that gives me the will to keep working at this.

26-03-04 *So Slow Chris* (to A. Peres)

I apologize for being so slow to reply. I have been hurriedly trying to finish my paper for the Holevo festschrift, and I pretty much dropped all else to get it done. I will attach the LaTeX file to the present note: Perhaps the introduction and conclusion sections will amuse you. (I haven't posted it yet on `quant-ph`; I'll probably do so Monday before departing for Germany myself.)

Asherism 8: *I have read again your* `quant-ph/0205039`*, and figures 1 and 2 reminded me of my paper "Convex probability domain ..." with Danny Terno, JPA* **31** *(1998) L671 [*`quant-ph/9806024`*]. Is there any relation?*

Yes, there absolutely is. It was quite an oversight on my part not to cite your paper there. (In fact, I thought I had until I received this from you.) It was because of your paper with Danny that I decided to make the "bureau of standards" measurement have precisely D^2 outcomes. If the measurement had any more than D^2 outcomes, say N, then the allowed region would have zero volume in the N-simplex, as you and Danny show. I have been meaning to repost that paper with various fix-ups. This will surely be one of them. (There, I just fixed it actually.)

What I think is the next important step is to say more about the geometric properties of these convex sets. For instance, with regard to qubits, the convex region is actually always an ellipsoid (no matter what the bureau-of-standards measurement). The ellipsoid varies from measurement to measurement, but it is always an ellipsoid. One question is, what is the proper characterization of the region in higher dimensions? What are the invariant geometrical features? And I can think of several questions beyond that. Would you be interested in collaborating on this?

I am glad to hear you survived your voyage back to Germany and did not come back with a renewed bitter taste. I very much enjoyed reading the (partial) autobiography you sent me. You have had a stirring and very impressive life.

26-03-04 *Nonuniqueness from Nonexistence* (to A. Peres)

By the way, I've been meaning to tell you how pleased I am of your embracing the point that "quantum states do not exist" in your quantum gravitational research.

To that end, let me attach another paper of mine—this one with Schack. There is not a lot new in it by way of technicality. It was effectively pasted together from old publications for the purpose of a review volume on "quantum state estimation," however, we reworked the language in the old articles significantly to take into account the more consistent view that we now have. The main parts that will be relevant to you are the introduction, the conclusions, and Section 6 on "The Subjectivity of Quantum Operations."

If quantum states do not exist (even pure states), then they surely cannot be unique (even pure states).

You will note how I used your 1984 paper "What Is a State Vector?" as a point of contrast for our present view. I hope you will not think that I was picking on you. The reason I singled out your paper is because it is the very clearest statement on the subject. If you think, however, that I should change any of the language before posting it, please let me know. For instance, I did

not attempt to contrast your 2004 view with your 1984 view—they may be significantly different!! (And I would guess they are.) Unfortunately, it may be too late for the version appearing in the volume: I was in such a rush sending off the draft—and returning the proofs—that the possible sensitivity of my statement didn't dawn on me at the time. But it will not be too late for `quant-ph`; please let me know.

19-04-04 *Part 2.1* (to D. M. Appleby)

I'm sorry; you've caught me at one of those times when I am in "bad correspondent" mode. (At the moment it looks like this is going to continue for a while—at least until I get my post-sabbatical plans settled.)

Presently, I guess I don't have much to say about your note except that I still don't like propensity, even as a view from somewhere. For it would mean—as far as I understand the term—that from my (or your or his or her, etc.) point of view nature has a certain tendency to this or that (quantifiable by the probability distribution each of us happens to use). I just think that terminology is misleading, attempting as it does to once again materialize probabilities, rather than let them stand for sheer ignorance or opinion. The only direction I see forward is for all of us to recognize starkly that the world owes us nothing. Regardless of our probability assignments, the world owes us nothing.

Beyond that, I also don't think I'm happy with the conception of physics as a *view* from somewhere. The term I protest is "view." Presently, I think it's all about survival, period, with the last bit of representationalism banished. I agree with the "somewhere-centeredness" you are striving for—though in my terms, for each of us, it is about MY survival (remember the note I sent you titled "Me, Me, Me"?)—but I would be hesitant to call that a "view." Do have a read of Rorty's "philosophical papers," Volume 1—I think he does a pretty good job of what I'm shooting for (what I'm shooting for in QM particularly, where I think the evidence is greater than what he's got to work with). But that's about all I can say at the moment.

Or maybe I can swipe a few (relevant) words from a recent note that Michel Bitbol wrote me. I'll place them below. Maybe they add a little gloss to what I'd like to say myself (if it were a better world with fewer time constraints).

Anyway, really, thanks for your long note. I hope I'm not offending with this short reply, but you're not alone: I've only been replying to small fraction (the most urgent) of my emails lately.

21-04-04 *From Glub to Snowflakes, Creation, and Construction* (to M. Bitbol)

Thanks for your thoughtful letter. I have very much enjoyed reading it (maybe five times now).

Bitbolism 1: *At this stage, I have to state my major point of disagreement with you: I suspect the remark you made about the "many-worlds interpretation" applies quite well to some of what you say. Let me explain this.*

You write: "What I find egocentric about the Everett point of view is the way it purports to be a means for us little finite beings to get outside the universe and imagine what it is doing as a whole." I deeply agree with you.

But, then, you begin to do essentially the same as the naive Everettian. You try to describe our situation in the world from a sort of vantage point. Here is the sentence in your paper that made me suspicious: "I think the solution is in nothing other than holding firmly—absolutely firmly—-to

the belief that we, the scientific agents, are physical systems in essence and composition no different than much of the rest of the world. But if we do hold firmly to that—in a way that I do not see the Everettist as holding to it—we have to recognize that what were doing in the game of science is swimming in the thick middle of things."

Of course, once again, I am delighted by the Pascalian metaphor of "swimming in the thick middle of things". But you seem to take it as more than a metaphor: a definite belief about (not to say a faithful description of) the world and our position in it. You write seriously that "we, the scientific agents, are *physical systems in essence and composition no different than much of the rest of the world". Isn't this a way of extrapolating one of our pragmatic-adaptative concepts, one of the concepts we need to swim with some success in the midst of the "glub" (here, the concept of a "physical system"), in order to describe everything including us as if it were seen from outside? I hear you saying something like "the world as I see it from my cosmic exile is made of physical systems and each one of us is one of these physical systems". But if we are "swimming" in the deep ocean of whatever we call "reality", we have absolutely no context-independent concept at our disposal, not even the very general meta-concept of "physical system". We must say that we ignore* everything *of the "thing in itself", including whether it is organized or not in a plurality of "physical systems". And we must therefore content ourselves with stating the formal conditions of our cognitive aptitudes (within it). This latter attitude is typical of the Kantian and neo-Kantian lineage of philosophy (when one gets rid of the foundational aspect of Kant himself and holds a pragmatic variety of Kantianism, as I do). I hope you'll recognize it as a radical variety of your view... I wonder whether you'll become a member of our radical club or rather decide to stick to your position as it stands.*

I don't know that I have an answer for you at the moment. (I hope you understand that all my efforts, all my writings, are of the groping variety—I have no final answers to anything. Nor do I hold any pretense of being consistent from one email or paper to the next. The only thing I can promise is that I do strive for consistency, and I welcome exercises like the one you've presented me.)

To make a start of an answer though, I think my usage of the term "physical system" is considerably more nuanced than you probably guess. (Though, it may not have been so nuanced at the time of my writing the anti-Växjö paper. Alternatively, I may have simply lapsed while I was writing those notes.) I say that it is nuanced, because I often toy with the idea that "physical systems" are agent-defined. You will find this idea probably first appearing in my writings in the chart on page 292 of *Notes on a Paulian Idea*. There I ask, "What is a quantum system?" And I reply to myself, "A line drawn in the sand." (It goes back to a 1995 letter to Greg Comer.)

Maybe I can do a better job of what I am thinking, though, by attaching a couple of other emails that I've been promoting recently. They're attached to this letter in a file titled ForMarcus.pdf. In the letter "Me, Me, Me" to Mermin and Schack within that collection, I give a definition of what I mean by "system." I hope that definition will make you think twice about the characterization of my views you gave above.

In general lately, I'm not even sure what I can make of the idea of "thing in itself"—it now sounds too static for what I'm trying to get at. To that end, let me attach another little compilation—this one titled ForSlusher.pdf. In particular, I hope the note within that titled "The World Is Under Construction" will help you better see what I am talking about.

Finally, let me paste below still another note to help muddy the waters. It is a string of earlier notes culminating in a few remarks to Jeff Bub; so in reading it linearly you will be travelling backward in time. Its relevance here is that I think you think when I utter the words "physical system" I am doing it in the sense that William James describes below as a case of "nothing but."

But I sincerely hope that is not what I am doing.

Anyway, all of this is probably not the sort of thing you were expecting as a reply: It is quite roundabout. But I am trying to put three lines of thought (as expressed in the three collections mentioned above) into a consistent whole. If I can do that, I think it will count as something of a direct answer to your query.

Does any of this make sense to you? Or does it all look more like the ramblings of a crazy man?

21-04-04 *Essential Incompleteness* (to W. G. Demopoulos)

Thanks for sending me your paper "Some Remarks on Elementary Propositions and Partial Boolean Algebras." I've taken a shot at understanding it—i.e., I've read it by some measure—but I probably didn't fare as well as I should have.

As you argued in your earlier letter to me (one from last year sometime), our views—or maybe just our languages—may not be so incompatible as one might think. However, I am left with the feeling that this is only a contingent feature of the particular stages of the game we happen to be at, at the moment. In particular, from my own view, I think it is quite important that we strive to stop thinking of quantum states as states of knowledge about the TRUTH VALUE of this or that proposition (even if truth value is not invariant with respect to 'experimental arrangement'—the idea you are toying with). My feeling is that the imagery of measurement outcomes mapping to truth values (in this context anyway) will only cloud our vision for how to take the next big step.

What is the next big step? I think it is a deeper understanding of how—very literally—the world "is in the making" (to use a Jamesian phrase). To try to make that idea at least graspable (if not either clear or consistent yet), and to try to show you quantum theory's role in all this, let me attach four letters I've written recently. They're contained within the attached files `ForMarcus.pdf` and `ForSlusher.pdf`. I think they are my best statements to date of what I am shooting for; and I think that goal fundamentally conflicts with the idea of "measurement" propositions having truth values in the conventional sense.

That is not to say, however, that I am yet ready to give up on the idea of physical systems having autonomous properties. The question is, what can still be pinned down as a property in the conventional sense?

In the letter you wrote me way back you said,

Demopoulosism 1: *What I've tried to address in my paper is the question whether there is anything in the quantum theory's conceptual framework that plays a role analogous to that played by a classical state. My suggestion is that the closest analog of this is given by the elementary physical propositions that are true of the system. They, rather than the quantum state, expresses the underlying reality that the theory purports to describe.*

As I said above, that's where I don't want to go. Instead, at least at the moment, the only thing I am willing to think of in quantum theory that "plays a role analogous to that played by a classical state" is a system's dimensionality. [Question: What is this system's ontic state? Answer: D, just D.] Here's the way I put it in the paper "On the Quantumness of a Hilbert Space" which will be appearing on quant-ph tomorrow:

> In this paper, I present some results that take their *motivation* (though not necessarily their interpretation) in a different point of view about the meaning of a system's dimensionality. From this view, dimensionality may be the raw, irreducible concept—the single *property* of a quantum system—from which other consequences are derived

> (for instance, the maximum number of distinguishable preparations which can be imparted to a system in a communication setting). The best I can put my finger on it is that dimensionality should have something to do with a quantum system's "sensitivity to the touch," its ability to be modified with respect to the external world due to the interventions of that world upon its natural course. Thus, for instance, in quantum computing each little push or computational step has the chance of counting for more than in the classical world.

This language is definitely not completely consistent with the vision I outline in the attached letters but it is the best I can do at the moment. ("Modification without modificata?!?," I hear you asking. "What could it mean?")

In any case, I have looked at your new paper, and I have re-looked at the paper you gave me last year, and I will be bringing both of them with me to Maryland. It would be a pleasure to talk further about all this—how we differ, or even how I missed something and the gaps may be erasable after all. (The latter would be very nice if it is the case.)

22-04-04 *Language Games* (to N. D. Mermin)

Correlation without Correlata ... Modification without Modificata ... No Stimulation without Stimulata!

I actually have found a way to make sense of the last one, "no stimulation without stimulata," within my budding point of view about quantum mechanics. Have I told you? No stimulation to the agent (in old language "the observer"), without simultaneously recognizing that the world must be stimulated in return. No stimulation to the agent without the external world being stimulata—i.e., that which is stimulated—in its own right.

Language games.

06-05-04 *Section 4.1* (to W. G. Demopoulos)

I'm on the plane now. I guess, at this stage, I don't have much to add to what I wrote you earlier after all. The "snowflakes" bit captures most of what I had wanted to say. The only other thing I can recommend to have a look at is Section 4.1 (trying to justify noncontextuality for our probability assignments in QM) in `quant-ph/0205039` (i.e., "QM as QI (and only a little more)"). I'll be curious to know how that argument meshes with your own.

What I would like to go into further eventually is the similarity and differences between 1) your point of view of the underlying events in QM as being preexistent (but maybe unique) realities, and 2) my point of view that they are best supposed as (unique) creations. I also get the feeling that your view collides with another one of my favorite doctrines: Namely, that we should take POVMs as just as basic as standard quantum measurements (because they so prettily match Bayes rule). But I'll have to think about that.

17-05-04 *Big Bang 1910* (to G. L. Comer)

The following passage comes from William James's posthumous book, *Some Problems of Philosophy*—started in March 1909 and worked upon until his death in August 1910.

> It is a common belief that all particular beings have one origin and source, either in God, or in atoms all equally old. There is no real novelty, it is believed, in the universe,

the new things that appear having either been eternally prefigured in the absolute, or being results of the same *primordia rerum*, atoms, or monads, getting into new mixtures. But the question of being is so obscure anyhow, that whether realities have burst into existence all at once, by a single 'bang,' as it were; or whether they came piecemeal, and have different ages (so that real novelties may be leaking into our universe all the time), may here be left an open question, though it is undoubtedly intellectually economical to suppose that all things are equally old, and that no novelties leak in.

18-05-04 *Agents, Interventions, and Surgical Removal* (to J. Woodward)

(I'll drop the "Professor Woodward" bit and act like I know you, now that this is a second contact.)

I had time over the weekend to better digest your Stanford Encyclopedia article, "Causation and Manipulability." Per my first guess, we do indeed use a lot of words in common—intervention, manipulation, agent—though your concern is causation and my concern is the elicitation of "quantum measurement outcomes." As evidence of this, see for instance Section 4 of my paper "Quantum Mechanics as Quantum Information (and only a little more)" posted at my website (link below).

But there are also more than words in common. There is the common concern of exorcising the (anthropomorphic) agent if possible from each of our domains. Can it be done? My suspicion about quantum mechanics is that there is a level at which it can be done and a level of which it can't. Both levels are needed, however, for an explication of what the theory is about.

What I liked in learning from your paper is the similarity of our strategies. (Rüdiger Schack tells me I'm a cherry-picker when it comes to extracting content from papers; sorry if I'm doing that here.) Anyway, the way I see it is we both start from rather firm starting points—though they necessarily involve the notion of an agent—and only then, make a step toward de-anthropomorphizing the picture. In your case, it's to start with a manipulationist account of causation and then to move to an interventionist one. The ultimate goal is to talk of causes in a way that makes no reference to human action.

In my case, I try first to make sense of a quantum measurement outcome as a birthy sort of event that happens when two pieces of the world metaphorically bump into each other *in the case that* one of the pieces is an active agent (who is equipped with subjective probabilities, etc., etc.). Of course, after that I'd like to go the next step—to imbed these anthropocentric "births" in a larger account of a world in continuous creation, in analogy to your strategy of imbedding humanly manipulations within a larger (de-anthropocentrized) interventionist account. At present, I only have a glimpse of how to do that. About the only thing I feel confident of is that, if it's going to be done, the project is going to have to be carried out in a relatively oblique manner—namely, one that stops talking about the births themselves and transfers attention to systems' capacities to take part in these kinds of mini-creations. (This is the stuff I call "Zing!" in footnote 10 of the paper mentioned above.)

Why must the strategy be oblique? Your paper gave me a nice metaphor for that:

> Human beings cannot at present alter the attractive force exerted by the moon on the tides (e.g., by altering its orbit). More interestingly, it may well be that there is no physically possible process that will meet the conditions for an intervention on the moon's position with respect to the tides – all possible processes that would alter the gravitational force exerted by the moon may be insufficiently "surgical". For example,

> it may very well be that any possible process that alters the position of the moon by altering the position of some other massive object will have an independent impact on the tides in violation of condition (M2) for an intervention.

You see, as opposed to classical gravitational phenomena, I believe quantum mechanics puts us in a new situation. The world appears to be wired together so intricately that every attempt to extricate or "surgically remove" the agent from the quantum measurement process leaves a conceptually empty structure (I'm thinking of Everettian and Bohmian quantum mechanics, etc., here). What quantum mechanics is mostly about is making-do in light of that.

That's on the negative side of things. On the positive side of things, knowing that the world is made of a "stuff" that causes us to "make-do" in this way is something I find tremendously exciting. I.e., it's not the making-do that's exciting, but the stuff that gives rise to it that is. This is because I think it leads to a far more interesting and creative world than we might have imagined otherwise. In the next note, I'll attach a historical/philosophical document I've been working on that tries to convey some of this. The title is, "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature," and it's fairly large (922K) ... so don't get frightened that something has gone wrong with your email. The thing is still far from complete, but I hope it conveys a flavor of the kinds of thoughts I'm up to in my most philosophical moments.

How do I live with such a crucial piece of quantum theory (and the next stage of physics it hints at) being so anthropocentric? Here's the way I put it in that document: "Observers, a necessary part of reality? No. But do they tend to change things once they are on the scene? Yes. If quantum mechanics can tell us something deep about nature, I think it is that." And it is that idea that I'm working my tail off to formalize.

Anyway, a long letter just to say I got a lot out of reading your paper and to reiterate that I think we have plenty of potential for fruitful interaction. [For instance—on a different subject—my radically Bayesian nose smells the agent remaining, despite your efforts, in your account of nondeterministic causation (right after equation CD). That is because, for such a Bayesian, the probabilities in the difference you consider still make crucial reference to an agent for their very existence.]

17-06-04 *Preamble* (to H. Mabuchi)

I think I would like you to also post the little text file below along with my other suggested readings for my "Intro to QM" lecture. You can give it the title "Preamble". It was something I sketched out on my flight over here, and reading over it again, I kind of like it.

> A lecturer faces a dilemma when teaching a course at a farsighted summer school like this one. This is because, when it comes to research, there is often a fine line between what one thinks and what is demonstrable fact. More than that, conveying to the students what one thinks—in other words, one's hopes, one's desires, the potentest of one's premises—can be just as empowering to the students' research lives (even if the ideas are not quite right) as the bare tabulation of any amount of demonstrable fact. So I want to use one percent of this lecture to tell you what I think—the potentest of all my premises—and use the remaining ninety-nine to tell you about the mathematical structure from which that premise arises.
>
> I think the greatest lesson quantum theory holds for us is that when two pieces of the world come together, they give birth. [Bring two fists together and then open them to imply an explosion.] They give birth to FACTS in a way not so unlike the

romantic notion of parenthood: that a child is more than the sum total of her parents, an entity unto herself with untold potential for reshaping the world. Add a new piece to a puzzle—not to its beginning or end or edges, but somewhere deep in its middle—and all the extant pieces must be rejiggled or recut to make a new, but different, whole. That is the great lesson.

But quantum mechanics is only a glimpse into this profound feature of nature; it is only a part of the story. For its focus is exclusively upon a very special case of this phenomenon: The case where one piece of the world is a highly-developed decision-making agent—an experimentalist—and the other piece is some fraction of the world that captures his attention or interest.

When an experimentalist reaches out and touches a quantum system—the process usually called quantum 'measurement'—that process gives rise to a birth. It gives rise to a little act of creation. And it is how those births or acts of creation impact the agent's *expectations* for other such births that is the subject matter of quantum theory. That is to say, quantum theory is a calculus for aiding us in our decisions and adjusting our expectations in a QUANTUM WORLD. Ultimately, as physicists, it is the quantum world for which we would like to say as much as we can, but that is not our starting point. Quantum theory rests at a level higher than that.

To put it starkly, quantum theory is just the start of our adventure. The quantum world is still ahead of us. So let us learn about quantum theory.

07-07-04 B (to G. Musser)

Musserism 1: *"It is a theory about a very small part of the world... a theory that is trying to tell us that there is much, much more to the world than it can say." How is this not hidden variables?*

Sure, they may not be hidden variables in the pre-existing sense – i.e. in the sense that a properly designed experiment can come asymptotically close to ascertaining their pre-experiment value. But does not "more to the world" imply something hidden?

Take a break from me for a moment and ask yourself how the Everett interpretation is not a hidden-variable theory? (It almost seems you would have asked the Everettian the same thing you asked me.) A hidden-variable theory is a very specific thing: If one were to know the value (even if only hypothetically and not operationally) of all the variables (including possibly the ones on the inside of the observer), then one can predict the outcome of all measurements with certainty. It is a fancy way of saying measurement outcomes pre-exist, even if nothing one would ever call a measurement is actually performed.

The determination or setting of specific measurement outcomes (in any quantum mechanical experiment) has always been outside of the quantum mechanical formalism. There is nothing in the formalism that determines whether one will get this click or whether one will get that click in some measurement device. But that does not make it a hidden-variable theory. What is hidden?

Here is the way Pauli put it:

> Like an ultimate fact without any cause, the individual outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case ...
>
> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular

factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.

Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.

(The conjunction of these thoughts is what I call "the Paulian idea"—hence the name of my book.) "Like an ultimate fact without any cause, the individual outcome of a measurement is not comprehended by laws."

The way I see it, quantum measurement outcomes are ultimate facts without specific call for further explanation. And indeed the quantum formalism supplies none. Thus there is more to the world than the quantum formalism can supply. Nothing to do with hidden variables.

But more specifically, regarding your point:

Musserism 2: *"It is a theory about a very small part of the world... a theory that is trying to tell us that there is much, much more to the world than it can say." How is this not hidden variables?*

How does the theory tell us that there is much more to the world than it can say? It tells us that *facts* can be made to come into existence, and not just at some time in the remote past called the "big bang" but here and now, all the time, whenever an observer sets out to perform (in antiquated language) a quantum measurement. I find that fantastic! And it hints that facts are being created all the time all around us. But that now steps out of the domain of what the quantum formalism is about, and so is the subject of future research. At the present—as a first step—I want rather to make the interpretation of the quantum formalism along these lines absolutely airtight. And then from there we'll better know how to go further.

Doesn't that just make you tingle? That (metaphorically, or maybe not so metaphorically) the big bang is, in part, right here all around us? And that the actions we take are *part* of that creation! At least for me, it makes my life count in a way that I didn't dare dream before I stumbled upon Wheeler, Pauli, and Bell-Kochen-Specker.

But let me get away from this speculation and rope myself back in on your particular question: How is this not some hidden variables account? Simple: If there are any extra facts being created around us, they nevertheless do not impinge on the individual quantum measurement outcome.

When I say that QM is a theory about a very small part of the world, you should literally think of a map of the United States in relation to the rest of the globe. The map of the US is certainly incomplete in the sense that it is obviously not a map of the whole globe. But on the other hand it is as complete as it can be (by definition) as a representation of the US. There are no hidden variables that one can add to the US map that will magically turn into a map of the whole globe after all. The US map is what it is and need be nothing more.

Does that help any?

I think a good bit of the problem comes from something that was beat into most of us at an early age. It is this idea: Whatever else it is, quantum theory should be construed as a theory of the world. The formalism and the terms within the formalism somehow reflect what is out there in the world. Thus, if there is more to the world than quantum theory holds out for, the theory must be incomplete. And we should seek to find what will complete it.

But my tack has been to say that that is a false image or a false expectation. Quantum theory from my view is not so much a law of nature (as the usual view takes), but rather a law of thought. In a slogan: Quantum mechanics is a law of thought. It is a way of plagiarizing George Boole who called probability theory a law of thought. (Look at the first couple of entries in the Ruediger

Schack chapter of *Notes on a Paulian Idea*.) Try to think of it in these terms, and let's see if this helps.

Let us take a simple term from probability theory, namely a probability distribution over some hypothesis $P(h)$. This function represents a gambling agent's expectations about which value of h will obtain in an observation or experiment. Suppose now the agent gathers a separate piece of data d from some other observation or experiment and uses it to conditionalize his expectations for h; i.e., he readjusts his expectations for h to some new function $P(h|d)$ by using Bayes' rule. Now here's a question for you. Is there anything within abstract probability theory that will allow the agent to predict precisely which value of d he will find when he gathers his data? Of course not. It's almost silly to pose the question. Abstract probability theory has nothing to do with the actual facts of the world. But then, doesn't that mean that probability theory is an incomplete theory? It can't, for instance, explain its own transitions $P(h) \longrightarrow P(h|d)$ since probability theory alone can't tell us why this d rather than that d. Moreover if probability is incomplete in this way, shouldn't we be striving to complete it? Both silly questions, and I hope for obvious reasons.

So:

1. There is no particular mystery in the transition $P(h) \longrightarrow P(h|d)$.

2. We would never expect probability theory to provide a mechanism to determine which value of d is found or produced in the experiment. The value d represents a fact of the world, and probability theory is *only* a theory about how to manipulate expectations once facts are given.

3. But also no one would be compelled to call probability theory incomplete because of this.

4. In particular, admitting this does not amount to having a hidden-variable explanation of probability theory.

So I say with quantum mechanics. The story is almost one-to-one the same: You just replace probability distributions with quantum states. ... But then you reply, "But there's a difference; quantum theory is a theory of physics, it is not simply a calculus of thought." And I say, "That's where you err." Quantum theory retains a trace of something about the real, physical world but predominantly it is a law of thought that agents should use when navigating in the (real, physical) world. In particular, just like with probability theory, we should not think of quantum theory as incomplete in the usual sense. If it is incomplete in any way, it is only incomplete in the way that the US map is incomplete with respect to the globe: There's a lot more land and ocean out there.

Teasing out (your words) the trace of the physical world in the formalism—i.e., the part of the theory that compels the rest of it as a useful law of thought—is the only way I see to get a solid handle on what quantum mechanics is trying to tell us about nature itself.

With this let me now go back to the US map for one final analogy. I said that there is a sense in which the US map is as complete as it can be. However there is also a sense in which it tells us something about the wider world: If we tabulate the distances between cities, we can't help but notice that the map is probably best drawn on the surface of a globe. I.e., the US already reveals a good guess on the curvature of the world as a whole—it hints that the world is not flat. And that's a great addition to our knowledge! For it tells a would-be Columbus that he can safely go out and explore new territories. Exploring those new territories won't make the US map any more complete, but it still means that there is a great adventure in front of him.

OK, with that, I'm going to call this discussion to an end. If you still think of me as a hidden variablist, I've either failed in my explanatory powers or my view is simply inconsistent. (I would bank on the former rather than the latter.) Either way I'm going to call the discussion to an end.

Good luck finalizing your article (if you haven't already finalized it).

12-10-04 *Blowing in the Wind* (to G. L. Comer)

I thought about both of us as I was reading the article below yesterday. I particularly loved the line, "Half the people you knew believed that if only they could figure out what Bob Dylan was saying, the secrets of the universe would be revealed." I've certainly done that: With John Lennon, then Paul Simon, and then Bob Dylan, and before and after and in between with John Wheeler and John Coltrane, and now mostly with William James and Richard Rorty. I keep waiting for the secrets of the universe to be revealed, and it's usually in poetry and music and stirring prose. Or at least I used to. Now mostly I worry about getting a new roof on the house and getting the place rigged with central air and heat ... and how much that's going to cost my pocket book.

25-10-04 *Stages of Development* (to N. D. Mermin)

I went through hernia operation in the winter of '94 and didn't much enjoy it. (Though, Carl Caves used to pride himself that it was his kids that likely caused the hernia—when I was playing with them one evening—and, by way of that, he could trace the cause of my meeting Kiki back to himself. You see, because of the operation, I actually stayed home for a while and stopped looking for a girl friend in the coffee shops and bars. And, lo and behold, in my boredom looking out the window, I first noticed this beautiful neighbor walking her dog. The rest history ... and two kids.)

Which reminds me of something that I've been wanting to record. Lately when Katie (who is getting close to three years old) misplaces something, if I ask her where it is, she will reply, "anywhere." She doesn't say, "I don't know." Or "I can't remember," or indeed any other phrase that would put the blame on her incapacities. She just replies with "anywhere." What has struck me is how much this reminds me of some of the early expositions of the uncertainty principle that I was exposed to. Maybe in both cases, it's all about stages in our development.

25-10-04 *The Quantum and the Politic* (to N. D. Mermin)

By the way, as you can guess, I've been watching the presidential race with greater than usual interest this time around. Of course there's the issue of how our nation is in a quagmire now ... and my profound confusion about how so many Americans can still support Bush. But there's been an undercurrent that has interested me at the intellectual level: All the talk about belief, reality, and certainty. It's everywhere!

In the first presidential debate, I couldn't have gotten a better slogan for our own discussions of the last couple of years, than from a remark Kerry made to Bush: "But this issue of certainty: it's one thing to be certain, but you can be certain and be wrong."

Even in quantum mechanics.

10-11-04 *Even Better Than Your Talk Title* (to H. Halvorson)

Halvorsonism 2: *By the way, John Conway is giving a talk "Free Will, Elementary Particles, & the Kochen-Specker Paradox" on November 19th at 4pm. The talk is based on a recent theorem by Conway and Kochen. They tell me that they have decisively proven, once and for all, that QM is indeterministic. Hmm!*

Frankly, I think I'd almost give up all of PSA to see this one! My own feeling is that nothing in quantum mechanics gets closer to a (trial) ontological statement (or ontological relative to the theory) in quantum mechanics than the Kochen-Specker theorem. Here's the way I put it tongue-in-cheek in a couple of talk abstracts recently (for the summer school that will follow our Konstanz meeting):

> Title: Some of the Phenomena of Quantum Information Theory
>
> Abstract: The no-cloning theorem. Quantum entanglement. Quantum teleportation. Quantum key distribution. Positive-operator-valued measures (POVMs). Quantum nonlocality without entanglement. The monogamy of quantum entanglement. Bell inequalities. The Kochen- Specker theorem.
>
> Title: Bayesian-Like Ways to Think of Those Phenomena
>
> Abstract: A Bayesian understanding of no-cloning. A Bayesian description of quantum teleportation. The connection between Bayesian conditionalization and POVMs. ... And so on down the line. Until, finally, the Kochen-Specker theorem: This one may actually be a statement about ontology rather than a statement about degrees of belief! And without some ontology, Bayesians would have very little reason to be.

Anyway, I suspect Conway will be talking about another version of Kochen-Specker (or something similar). I'd like to see the mechanics of that.

Hope to hear a positive response from you soon on the BBQW meeting! I think this is going to be the best (most productive, most exciting, most memorable) meeting I've ever been involved in organizing!

14-12-04 *Thoughts on Non-nonlocality* (to H. Price)

Finally coming back to your request for what to read within my writings on how a Bayesian conception of quantum states takes care of (or should take care of) issues to do with nonlocality in QM. Let me suggest you read Section 3, "Why Information?", and the beginning part of Section 4, "Information About What?", in my paper "QM as Quantum Information (and only a little more)", `quant-ph/0205039`. In that regard, let me also paste in below some correspondence I had with David Mermin and Jerry Finkelstein on those sections—I think they clarify things a bit further—and let me also attach an abridged version of the same paper that I never posted on the archive. In that version, I changed the "Why Information?" section somewhat to better reflect what I told Mermin below.

The key issue is, what do I mean when I say that nothing physically changes on Bob's side when Alice performs a measurement on her half of an entangled pair? In that regard, I think you might also get something out of my discussions on pages 175–176 and 184–189 of my samizdat *Quantum States: What the Hell Are They?*. These discussions were motivated by some standard dictionary (can't remember which) wanting to include a definition of quantum teleportation in its latest revision, and Bennett supporting a definition that said something like, "The transference of one particle's properties onto another particle via the assistance of ..." The thing I objected to strenuously was the idea that any properties at all were transferred. Also, let me point you to some discussion on the same point in *Notes on a Paulian Idea*. See pages 465–466, starting at the words "Quote from Draft," and see pages 467–471 in the note titled, "Detailed Commentary." Getting a description of quantum teleportation that Asher and I could both agree on for the paper we were

writing was a point of serious contention for us. And I went at great lengths to try to get my point across to him. It's exactly the same point that's relevant to our own discussion (i.e., Fuchs and Price).

I guess you could say the overarching idea is "no surprises, no reality." That is an idea I flesh out in pretty good detail in my paper "The Anti-Växjö Interpretation of Quantum Mechanics." See sections 4 and 5. (They are completely independent of the rest of the paper; so don't read any of the rest of the paper.) I think (I hope) actually, that you'll find the discussion there interesting independently of any issues to do with our discussion of nonlocality. In particular, if you read those sections, I don't think you could fail to give me the credentials of a "subject naturalist." Am I right?

Let's see, anything more? One last thing: Let me also suggest Chapter 8 of Chris Timpson's PhD thesis *Quantum Information Theory and the Foundations of Quantum Mechanics*, `quant-ph/0412063`, and also the "Envoi" at the very end. He does quite a nice job of explaining how the "nonfactivity" of our quantum states buys us the ability to stop talking about nonlocality.

There, that's one thing marked off my to-do list for you. (Certainly there's no rush to respond; I know your travelling. Just archive this note until you have the time ... even if that's six months from now.)

14-12-04 *Directions, Etc* (to J. Bub)

Bubism 3: *Unfortunately, the only copy of Bernardo and Smith is out of the library, so probably no one will have read that. A free roaming discussion is fine, I think, but you should probably be prepared to talk a bit in general first (or perhaps second) about what specific foundational issues in quantum mechanics push one towards Bayesianism, and perhaps also about information in physics, since this is the topic of the seminar. Does that sound OK?*

I'll try.

But most importantly, you gotta realize that, concerning

> what specific foundational issues in quantum mechanics push one towards Bayesianism,

it's the other way around. It's clear thinking about probability and the use of it that leads to Bayesianism—nothing to do with quantum mechanics. *Then*, recognizing that a quantum state is (mathematically and conceptually) *nothing more than* a compendium of probabilities, one gets to the Bayesian conception of quantum states. In other words, it is Bayesianism that carries a hope of clarifying quantum foundations—not that something in quantum mechanics specifically pushes toward Bayesianism.

I'll try to see if I can get Bernardo and Smith scanned into my computer and turned into a text file. There's some chance that that might happen tonight.

15-12-04 *Favorite Bernardo and Smith Quotes* (to J. Bub)

Well, try as I might, I couldn't get my scanner connected up last night. It always takes much more time to install these things than one expects! Very frustrating. It just confirms what I've been thinking: I never want to go through another move again. *I never ever want to move again!* Moves just disrupt everything in life, and never for the better.

In light of last night's failure, let me do this as a stopgap. Here are two of my favorite Bernardo and Smith quotes that I already have in my computer. Chew upon them, because I think they especially express the flavor of the reconception of quantum mechanics I'm shooting for: That quantum mechanics is a *normative* theory of personal behavior (or uncertainty accounting) IN LIGHT OF the peculiar, particular world in which we are immersed. Different world, different normative theory for agents immersed within it. Moreover, because of the last point, though quantum theory is a theory about agents (rather than of objective reality per se), one can still hope to glean some hypotheses about objective reality itself from it. One just has to do so obliquely. But that's our task, and that's what we're setting ourselves up to do.

I'll try to emphasize these things in a preamble to our discussion tomorrow night. But if you don't mind, distribute this note to your students first so that it'll have had at least a little time to percolate in their minds.

31-01-05 *Quote of the Day* (to N. D. Mermin)

I just ran across the following quote of Nietzsche in Jauch's book on quantum foundations, and it reminded me of you:

> That things have a quality in themselves quite apart from interpretation and subjectivity, is an idle hypothesis: It would presuppose that to interpret and to be a subject are not essential, that a thing detached from all relations is still a thing.
>
> — from *The Will to Power*

I don't know that you'll like the first part; but I'll bet that the second part makes the phrase "correlation without correlata" pop into your head (for whatever reason).

08-03-05 *Spelled Wrocław, Pronounced Vrotswav, but Germans Still Say Breslau* (to D. B. L. Baker)

"It's been a long time since I've written you from a far-away place. I guess that's because far-away places have become little more than a pain for me. Trip after trip seems to drone on, and I've lost so much purpose. But tonight I feel like writing something. I'm captured by the charm of what I see around me. The snow coming down, the muffled sounds and the romance in this city square; a kiss I spied in a dark corner of the cathedral."

I wrote those lines in my head last night, thinking that I would go back to my hotel room, have a beer or two, and write you a long note. It didn't happen. I had the beer, you see, but I got wrapped up in music and the newspaper. Hans Bethe died yesterday at the age of 98, and I got sucked into stories about him. He was a great physicist and a great man. The last time I saw him, about 6 years ago, he was still reporting his research! In slow motion, but he was still reporting topical research! Amazing. Get on Google News if you can, and read about him. He was an important part of America and of all time; he brought the cores of stars into the hands of man. You read about someone like that, and I challenge you to think that man is an insignificant force in nature.

21-03-05 *Changing the World* (to C. H. Bennett)

Here's a little story I've got to tell you and Theo. Ever since you wrote me these lines soon after Katie's birth,

Bennettism 15: *Theo enjoyed your birth announcement for Katie. When I read her that Katie was beautiful in every way and would shape the world by her presence and will, she said she wasn't surprised. That is just what she would expect from another baby that came from the same parents as Emma. Theo told me that her grandfather, when told of a the birth of a baby from someone he admired, would typically say "That baby will change the world." (After the world changes it a few hundred times, I couldn't help thinking.) Congratulations to all four of you from the two of us.*

I have from time to time told her that one day she will change the world—that that's her destiny. Last night I told her that as I was tucking her into bed and asked, "Do you know it?" How did she reply? "Yeah. Today I changed the batteries in my toy."

I loved it and had to record it.

30-03-05 *Civil Disobedience* (to G. L. Comer)

Thanks for your notes bringing me up-to-date on your tripartite career: poetry, music, and physics. And I applaud your efforts to find linearity in each of them! (I.e., the place of the observer/actor/agent in each of them.) [...]

I look forward to reading your discussion of linearity in GR. Definitely, send it by me as it comes along.

For myself, I've been obsessed by a basic linear algebra for two months now, and I've thrown myself into it like I was a graduate student again. That is one of the great things about quantum information: One starts to realize how many very basic questions in finite-dimensional linear algebra have simply never been explored. And sometimes, they're really tough questions despite the finite dimensions. Take the linear vector space of $d \times d$ complex matrices. That's a d^2 dimensional vector space, and you can equip it with an inner product, say the Frobenius one $\mathrm{tr}(A^\dagger B)$ where $\dagger$ denotes Hermitian conjugation. Question: Can one always construct a complete orthonormal basis for this vector space consisting solely of unitary matrices? It's not obvious, but the answer is yes—it took Schwinger to note it. New question: Can one always construct a complete orthonormal basis consisting of Hermitian matrices? Much easier, and the answer is yes. New question: Can one always construct a complete orthonormal basis consisting of positive semidefinite Hermitian matrices? Much harder! But the answer is no. In fact, it looks like the closest one can come is to get a normalized basis with pairwise inner products of value $1/(d+1)$. But proving it, that has turned out to be really hard. Even constructing a set of (positive semidefinite Hermitian) matrices with those inner products has turned out to be elusive. Constructions exist for $d = 2, ..., 8$ and 19, but there's only numerical evidence for the other dimensions up to 45 (and after 45 there's not even numerical evidence, the problem becomes too big for easily accessible computers). But dammit, it's an important question. For these are precisely the things we should have been using all along to express quantum mechanics, rather than expansions in terms of orthonormal bases.

But such is the life! Interpreting and reinterpreting the scripture.

12-05-05 *Smelling Correlation without Correlata* (to N. D. Mermin)

I was listening to a talk of Robert Raussendorf yesterday, who is visiting us here at Bell Labs, on cluster-state computation, and I'm almost ashamed to admit it, but I just about blurted out "Correlation without Correlata!" right in the middle of the talk! There was something in all he said that made the idea come to the surface in a way it hadn't (for me) in years. Particularly, I was moved by his point (actually, the particular way he presented it because I had already known it)

that in a direct simulation of a quantum circuit in this model, one could perform the measurement that represents the readout in the first step, in the last step, or anywhere in between. It's only the correlation, not the correlata (not the particular measurement outcomes themselves), that matter in this model of quantum computation.

Anyway, I found myself wondering if this model of quantum computation might not be a better home for your "Copenhagen Computation" ideas than the usual unitary model. Have you thought about that?

17-05-05 *Spontaneous Syntax Breaking* (to G. L. Comer)

I feel your pain ...

On a related subject (to which your note brings me back, as did a couple of sentences in this talk by Healey that I heard a couple of weeks ago: `http://carnap.umd.edu/philphysics/healey.htm`), back in my early years at UNC I went through an obsession for a while of looking up formal developments of the idea of a differentiable manifold. The thing that I was troubled by at the time was that in all the developments I could find, the idea of a set *plus* a set of coordinate charts (along with a requirement that it be possible to paste them together) was *primary*. I could find no definition of differentiable manifold that didn't rely on a more primitive definition of coordinate chart. The reason that troubled me—these are all vague memories now, so please don't press me to try to defend any of the ideas anymore or even try to make them precise—was that when it came time to describe what general relativity was about *but in words*—and all books of course strived to do that—the differential manifold was taken as a *primary* construct with coordinate charts being something of a secondary nature. See the circularity in that? You can't get the idea of a differentiable manifold off the ground without the idea of a coordinate chart, but then you're told that the differentiable manifold is the primary idea and coordinate systems are only gratuitous additions.

Do you remember in discussions of ours along these lines (circa 1991/1992)? And if so, can you dig up any of the old mails/emails?

Friday I leave for beautiful Vienna. But my romanticism for the place is marred by the sad fact that I have no idea yet what I'll say in my talk! There'll be a lot of sleeplessness between now and then.

18-05-05 *Coordinate Charts* (to G. L. Comer)

The point I didn't emphasize in my last note—where at least it looks like I was successful in emphasizing that, in all the formal developments I've seen, "coordinate chart" is a more basic notion than "differentiable manifold" (otherwise the definition of manifold would not make use of coordinate chart in any way)—is that this may mesh very well with what you wrote me:

Comerism 8: *General covariance is the process through which results in one coordinate system can be translated into results for another coordinate system.*

From that point of view, coordinate chart should indeed be primary, with differentiable manifold only secondary: "Differentiable manifold" is only the formal object that falls out of the *physical* statement that all coordinate systems should be smoothly translatable into one another (locally).

A little bit of these thoughts were on my mind when I wrote that old draft/notes "an information theoretic hole in general covariance" (or with such title), circa '91. You probably don't remember it because you probably thought I was a quack then (maybe you still do). Anyway, the wacky idea on my mind at the time was that *I hoped* that if one restricted oneself to *only* the coordinate charts

that were computably (in the Turing sense) generatable from one another, one might not be able to distill a full-fledged manifold from the process but a different, more loosely connected structure for spacetime. In this way, I hoped to make sense of EPR phenomena by explaining that the two particles in it are not so far from each other after all.

With the years and the thoughts that have intervened, I know that those ideas were indeed very misplaced: They were indeed quackery. But it laid the groundwork for me to better appreciate your point: Coordinate systems are a more basic mathematical structure than manifolds for a rather deep reason. They are proxies for potential observers (agents, in my favored language), and the key issue is translatability.

19-06-05 *Philosopher's Stone* (to G. L. Comer)

[. . .] The week turned out to be quite productive, and ideas started flowing.

The main thing on my mind has been viewing the quantum measurement process as a kind of modern version of alchemy. I guess I've been talking about this on and off for several years now, having been exposed to the analogy through Pauli's letters to Jung and Fierz, but the transformation in Sweden was that the stuff really got into my way of thinking, and I could start to see how to formalize it. I'm toying with the idea of writing a paper titled, "The Consequences of Our Interventions," to try to redo my paper with Asher "Quantum Theory Needs No Interpretation" from this new perspective. In that paper, we had written,

> The thread common to all the nonstandard "interpretations" is the desire to create a new theory with features that correspond to some reality independent of our potential experiments. But, trying to fulfill a classical worldview by encumbering quantum mechanics with hidden variables, multiple worlds, consistency rules, or spontaneous collapse, without any improvement in its predictive power, only gives the illusion of a better understanding. Contrary to those desires, quantum theory does *not* describe physical reality. What it does is provide an algorithm for computing *probabilities* for the macroscopic events ("detector clicks") that are the consequences of our experimental interventions. This strict definition of the scope of quantum theory is the only interpretation ever needed, whether by experimenters or theorists.

First off, I wish I had never said, "quantum theory does not describe physical reality"—I really only meant "the wave function does not describe reality" and should have stuck with that formulation. But more importantly, what precisely are these "consequences of our interventions"? From the wording we used, one surely gets the impression that, whatever they are—we said "detector clicks," but what a glib phrase!—they somehow live outside of the agent performing the experiment. And I guess that's what I thought at the time.

Now I'm quite internal about it all; those "consequences" really live in the agent himself, not in the quantum system or even in some "detached" measurement device. And I think I now have a clear enough formulation of the idea that I ought to go public with it.

Quantum THEORY (I emphasize theory to draw a distinction between quantum theory and the quantum world, i.e., that stuff outside of us which is independent of the agent) is predominantly about the changes we bring about within ourselves by interacting with the external world. Very little of quantum theory is about the external world itself. Particularly, the clicks are not "out there," but rather "in here."

I've been scouring the Pauli quotes I have in my computer to see exactly where I got these ideas from, and so far I haven't read nearly as forceful of a version of it as I seem to remember—so I'm

pretty sure I still haven't quite found the right passage. The only thing that comes close, so far, is this passage from Pauli's article on Kepler:

> Now, there is a basic difference between the observers, or instruments of observation, which must be taken into consideration by modern microphysics, and the detached observer of classical physics. By the latter I mean one who is not necessarily without effect on the system observed but whose influence can always be eliminated by determinable corrections. In microphysics, however, the natural laws are of such a kind that every bit of knowledge gained from a measurement must be paid for by the loss of other, complementary items of knowledge. Every observation, therefore, interferes on an indeterminable scale both with the instruments of observation and with the system observed and interrupts the causal connection of the phenomena preceding it with those following it. This uncontrollable interaction between observer and system observed, taking place in every process of measurement, invalidates the deterministic conception of the phenomena assumed in classical physics: the series of events taking place according to pre-determined rules is interrupted, after a free choice has been made by the beholder between mutually exclusive experimental arrangements, by the selective observation which, as an essentially non-automatic occurrence (Geschehen), may be compared to a creation in the microcosm or even to a transmutation (Wandlung) the results of which are, however, unpredictable and beyond human control.
>
> In this way the role of the observer in modern physics is satisfactorily accounted for. The reaction of the knowledge gained on the gainer of that knowledge (Erkennenden) gives rise, however, to a situation transcending natural science, since it is necessary for the sake of the completeness of the experience connected therewith that it should have an obligatory force for the researcher (für den Erkennenden verbindlich). We have seen how not only alchemy but the heliocentric idea furnishes an instructive example of the problem as to how the process of knowing is connected with the religious experience of transmutation undergone by him who acquires knowledge (Wandlungserlebnis des Erkennenden). This connection can only be comprehended through symbols which both imaginatively express the emotional aspect of the experience and stand in vital relationship to the sum total of contemporary knowledge and the actual process of cognition. Just because in our times the possibility of such symbolism has become an alien idea, it may be considered especially interesting to examine another age to which the concepts of what is now called classical scientific mechanics were foreign but which permits us to prove the existence of a symbol that had, simultaneously, a religious and a scientific function.

Or maybe I picked it up from this passage in Heisenberg's article, "Wolfgang Pauli's Philosophical Outlook,"

> The elaboration of Plato's thought had led, in neo-Platonism and Christianity, to a position where matter was characterized as void of Ideas. Hence, since the intelligible was identical with the good, matter was identified as evil. But in the new science the world-soul was finally replaced by the abstract mathematical law of nature. Against this one-sidedly spiritualizing tendency the alchemistical philosophy, championed here by Fludd, represents a certain counterpoise. In the alchemistic view "there dwells in matter a spirit awaiting release. The alchemist in his laboratory is constantly involved in nature's course, in such wise that the real or supposed chemical reactions in the retort are mystically identified with the psychic processes in himself, and are called by

> the same names. The release of the substance by the man who transmutes it, which culminates in the production of the philosopher's stone, is seen by the alchemist, in light of the mystical correspondence of macrocosmos and microcosmos, as identical with the saving transformation of the man by the work, which succeeds only 'Deo concedente.'" The governing symbol for this magical view of nature is the quaternary number, the so-called "tetractys" of the Pythagoreans, which is put together out of two polarities. The division is correlated with the dark side of the world (matter, the Devil), and the magical view of nature also embraces this dark region.

Or finally, maybe from this letter from Pauli to Fierz:

> All of this then led me onto further, somewhat more phantastic [*sic*] paths of thought. It might very well be that we do not treat matter, for example viewed in the sense of *life*, "properly" if we observe it as we do in quantum mechanics, *specifically when doing so in complete ignorance of the inner state of the "observer."*
>
> It appears to me to be the case that the "after-effects" of observation which were ignored would *still* enter into the picture (as atomic bombs, general anxiety, "the Oppenheimer case" e.g. etc.), but in an *unwanted form.* The well-known "incompleteness" of quantum mechanics (Einstein) is certainly an existent fact somehow-somewhere, but certainly cannot be removed by reverting to classical field physics (that is only a "neurotic misunderstanding" of Einstein), it has much more to do with *integral relationships between "inside" and "outside" which the natural science of today does not contain* (but which alchemy had suspected and which can also be detected in the symbolics of my dreams, about which I believe them specifically to be characteristic for a contemporary physicist).
>
> With these vague courses of thought I have reached the border of that which is recognizable today, and I have even approached "magic." (From this standpoint observation in quantum mechanics might even appear to someone as a "black mass" after which the "ill-treated" matter manipulates its counter-effect against the "observer," thereby "taking its revenge," as a "shot being released from behind"). On this point I realize well that this amounts to the threatening danger of a regression into the most primitive superstition, that this would be much worse than Einstein's regressive remaining tied to classical field physics and that everything is a matter of holding onto the positive results and values of the *ratio.*

Still I have the nagging feeling that there is something much better if I could just dig it up.

Anyway, the way I view quantum measurement now is this. When one performs a "measurement" on a system, all one is really doing is taking an ACTION on that system. From this view, time evolutions or unitary operations etc., are not actions that one can take on a system; only "measurements" are. Thus the word measurement is really a misnomer—it is only an action. In contradistinction to the old idea that a measurement is a query of nature, or a way of gathering information or knowledge about nature, from this view it is just an action on something external—it is a kick of sorts. The "measurement device" should be thought of as being like a prosthetic hand for the agent—it is merely an extension of him; in this context, it should not be thought of as an independent entity beyond the agent. What quantum theory tells us is that the formal structure of all our possible actions (perhaps via the help of these prosthetic hands) is captured by the idea of a Positive-Operator-Valued Measure (or POVM, or so-called "generalized measurement"). We take our actions upon a system, and in return, the system gives rise to a reaction—in older terms, that is the "measurement outcome"—but the reaction is in the agent himself. The role of the quantum

system is thus more like that of the philosopher's stone; it is the catalyst that brings about a transformation (or transmutation) of the agent.

Reciprocally, there should be a transmutation of the system external to the agent. But the great trouble in quantum interpretation—I now think—is that we have been too inclined to jump the gun all these years: We have been misidentifying where the transmutation indicated by quantum mechanics (i.e., the one which quantum theory actually talks about, the "measurement outcome") takes place. It should be the case that there are also transmutations in the external world (transmutations in the system) in each quantum "measurement", BUT that is not what quantum theory is about. It is only a hint of that more interesting transmutation. And, as you know, somehow out of all this I think of the agent and the observer as being, together, involved in a little act of creation that ultimately has an autonomy of its own—that's the sexual interpretation of quantum mechanics. (However the ideas in that last sentence are a little murkier than what I'm trying to get at now.)

Does "measurement" in this new sense explicitly require consciousness (whatever that is)? I don't think so. But it does require some kind of nonreductive element—some kind of higher-level description that cannot be reduced to a lower-level one. Here's the way I've been putting it in my last three lectures, when talking more particularly about quantum mechanics from the Bayesian perspective. I point out that a Bayesian is, roughly speaking, someone who believes that without gamblers, there cannot be probabilities. Probabilities are not external to gamblers. Then someone always asks, must you have consciousness to have probabilities? And I say, "No." Take as an example my laptop computer loaded with a Bayesian spam filter. It is a perfectly good gambler in the Bayesian sense, but I think most people would be hard-pressed to call it conscious. Similarly, I think we're going to ultimately learn an analogous lesson about all this transformation/transmutation/ creation/measurement business.

On the other hand, I do find myself being tickled toward a more Whiteheadian-like view that, whatever this higher-level description is, every piece of nature has more or less of it, from people all the way to stones and atoms. It's just that you have to be on the inside of the philosopher's stone to see it.

25-07-05 *Bad Jokes* (to N. D. Mermin)

Regarding:

Merminition 147: *Please use the full title I sent you:*

> Does being Bayesian illuminate the quantum world?
> Or is the quantum world an embarrassment to the Bayesian?
>
> Abstract: The speaker will meditate on what he likes and what he dislikes about taking a Bayesian view of quantum theoretic probabilities. The talk will be very informal if only because, as we all know, there is no quantum world.

As you should know, I think there is ONLY a quantum world. Did you forget about me when you wrote your abstract?

Merminition 148: *Just an example of the kind of bad jokes I hoped I'd be able to string together. I had in mind the Bohr quote. Nothing more. But I did think it was a Law of Thought for you. Not the same as a world.*

I knew you had the Bohr quote in mind; I was trying to make my own bad joke.

Concerning, "But I did think it was a Law of Thought for you." ... you never cease to shock me .... And you never cease to cause me to strive to try to convey the very simple little idea more effectively! I wonder when I'm gonna finally hit the sweet spot? Quantum THEORY, a law of thought: Yes. Resoundingly yes. But the quantum WORLD—i.e., that situation, that world, that reality, which conditions us to choose THIS law of thought rather than THAT law of thought (in other words some alternative or imaginary law of thought)—is something else entirely. It's the stuff that's here whether there are any law-of-thoughters around or not. That's what I really want to get at; that's what I've always really wanted to get at.

25-10-05 *GOBs, Steering and Teleportation* (to H. Halvorson & B. C. van Fraassen)

... and if you're patient there's even a little bit about perspectivalism at the end.

Thanks again for letting me attend your seminar last Wednesday. I had a good time, and I hope I will push myself to continue coming. (If it only weren't for that darned long drive: 45–65 minutes, depending on traffic.) The main thing I get out of the deal, of course, is not so much the quantum-information material, but that the lectures and discussion points give me a window into how philosophers are starting to think about this subject. That's valuable for me.

Anyway, spurred by what I heard at the last seminar, I put together a few notes, and they're pasted below. Feel free to not read any further than this sentence if you're getting tired of my emails: These exercises of sentence construction are always useful for me, even if for no one else; so certainly I won't be hurt if you send this email to the recycle bin and don't reply. However, since you were the discussion leaders, I figure I might as well share the notes with you, even if only out of courtesy—I only hope they won't cause you to give me a failing grade for the semester!

Maudlin

First off, let me tackle something Maudlin said near the end of the seminar. As I recall, he basically asserted that the phenomenon of "steering" is a very real effect (I'm tempted to put the word "ontic" into his mouth here), that Bell had taught us such, and that the phenomenon has very useful consequences—for instance he tried to make the last point dramatic by putting a gun to Bob's head. As I recall, the point that started all this drama was Hans' statement that he was reluctant to accept the idea that a localized measurement could change the global *quantum* state for a bipartite system.

The main point I want to make here is that Maudlin's drama really carries no force as far as saying something *unique* about the quantum world. Let me give a contrived example that is a) not quantum mechanical, but b) serves the role Tim sought to fulfill via quantum entanglement (and quantum entanglement only).

Suppose Bob has in front of him four buckets, labelled 1, 2, 3, 4, and that there is a ball in precisely one of them—the rest of them are empty—but Bob has no clue which one. Furthermore unfortunately for him, these buckets along with the one ball are all themselves hermetically sealed in a GREAT OBFUSCATING BOX, or GOB. The definition of a great obfuscating box is that one can query it to ask a yes-no question of the ball's position within the collection of buckets, but it will never let the questioner know the position more precisely than two buckets' worth. For instance, one might ask "1v2?", meaning "is the ball in bucket 1 or 2?", to which the GOB will answer yes or no. If it answers "yes", then one is assured that the ball is indeed in bucket 1 or

bucket 2; if it answers "no", then the ball must be in either bucket 3 or 4. Similarly one could ask the GOB "2v3?" A "yes" answer narrows the ball's position down to 2 or 3, and a "no" answer narrows it to 1 or 4. And similarly still, one could ask the GOB "1v3?" to get an appropriate answer for that question. The nasty trick is that only one of these three questions can be asked and never another one.

So, with a GOB in place of a qubit, let's now go back to the rest of Maudlin's scenario. Charlie holds a gun to Bob's head and says, "I'm going to ask the GOB this question "XvY?", and if you correctly predict the outcome before I get the result from the GOB itself, then I'll let you go free; but if you get it wrong, then BAM!, a bullet in the head! By the specification of the scenario, without any further help, there's very little Bob can do but make a guess and say his prayers.

But suppose Bob's friend Alice happens to overhear what's going on and can surreptitiously talk to him. If it turns out that she knows she has an identically manufactured GOB—assured by the factory to have a ball in precisely the same bucket as Bob's—then everything changes. She can help her friend by asking her GOB the question Charlie is about to ask his GOB and then slipping the answer back to Bob. Bob's GOB, we know, will give the same answer if she asks the same question. Alice, of course, still won't know precisely what bucket her ball is in (and consequently which bucket Bob's is in), but the information she gained is enough to keep Bob from getting shot.

So there: All the drama of Maudlin's example, all the same conclusions operationally, but where's the quantum mechanics? Where's the nonlocality? There is none. The example is powered solely by Bob's having a friend who is capable of relieving him of some of his uncertainty. Alice passes off her own incomplete information to Bob, and that saves the day. In the context of quantum mechanics, Schrödinger called that first action of Alice's (before passing off the information) "steering," but that seems like such a weird term for an example like this: Alice isn't steering anything at all; she's just trimming her uncertainty one way or another by making the choice to ask one question or another.

Having a hidden variable—i.e., the actual position of the ball in the buckets—serves only as a dramatic device in this example: For it makes it absolutely clear that Alice's action of learning at her site changes nothing about the reality at Bob's site. The reason the situation *seems* to look different in quantum mechanics is because we have all become convinced (through Bell-like arguments) that there can be no local hidden-variable theories underlying quantum mechanics. But from my own perspective, that's an over-hasty conclusion. In fact, it's a *non sequitur*. One can have uncertainties about many things, from localized variables to relations between very distant systems to the consequences of one's own actions. And in all of those cases, one can invent "passing off privileged information" scenarios like the one above to make dramatic a kind of "steering phenomena." Ruling out localized hidden variables (which I definitely believe has been done in quantum mechanics) in no way pushes the further consequence that "steering" is an ontic phenomenon rather an epistemic one.

In fact, I have always hated the word "steering" as a name for this phenomenon in the quantum context because it already loads the dice toward an ontic interpretation of quantum states. Indeed, one can see that from the very beginning in Schrödinger's early 1935 papers and his correspondence with Einstein. Schrödinger *starts* with the assumption that quantum states are ontic states (rather than epistemic states), and is therefore led to introducing some ad hoc rules for entanglement-decay so as to get out of a kind of action at a distance. On the other hand, Einstein (as exhibited in correspondence with Schrödinger reprinted in Fine's book *The Shaky Game*) starts with the rejection of action at a distance, and concludes that "steering" must be an epistemic phenomenon.

I side with Einstein, of course. Thus, to load the dice toward my own interpretation, I much prefer the words "conditioning" or "conditionalizing" (as from simple probability theory) or even "updating" over "steering." These words much more adequately convey the passive nature of what

is going on in updating the quantum state of a far away system.

By the way, now that I've said all that, let me report that I'm doing nothing with my GOBs that Rob Spekkens isn't already doing with his toy model: `quant-ph/0401052`. I think that is conceptually the most important paper written concerning the interpretation of quantum information written in long while, and well worth everyone's understanding.

Teleportation

But isn't quantum teleportation the indication of something much deeper going on with quantum steering than with epistemic updating? No, it is just about the same thing fancied up into a more complicated situation.

Let me try to make that clear by writing down an example that I told Hans about in conversation at our second-to-last meeting. In usual teleportation, the cast of characters includes an Alice and a Bob who share two systems in a maximally entangled state, and implicitly, a Charlie who prepares a third system in the state of his choice and then hands it off to Alice. Alice then performs a measurement on the two systems in her possession and announces the result of the measurement to Bob. The teleportation process is completed with Bob performing an operation on his system conditioned upon his newly acquired information.

In what sense is it completed? Only in this: If Charlie has the promise that Alice and Bob went through all the actions described above, then he can safely ascribe the same quantum state to Bob's system that he had originally ascribed to the system he handed off to Alice.

Here's a corresponding classical example. In place of entanglement, let us equip Alice and Bob each with a coin (oriented heads or tails) encased in a magical opaque box. These magical opaque boxes have the following properties: 1) though one cannot see how a coin is oriented within it, one can nevertheless reach inside a box and turn the coin over if one wishes, and 2) if one touches two of these boxes together, they will glow green if the coins within them have the same orientations, and they will glow red if they have opposite orientations—the glowing reveals nothing about the actual orientation of either coin, only about their relationship. Finally let us stipulate the following for Alice and Bob: That their opaque boxes contain identically oriented coins, but Alice and Bob (or anyone else for that matter) know nothing more about the coins beyond that. In other words, Alice and Bob possess HH or TT, but they do not know which.

Now, as in quantum teleportation let us introduce a third character, Charlie. Charlie has an opaque box of his own. But let us give him some partial certainty about the orientation of his coin. Particularly, let us suppose he ascribes a probability p for his coin to be heads. This is a real number between 0 and 1, and in principle it might take an infinite number of bits to specify.

Here's the protocol. Charlie hands off his coin (encased in a magical opaque box) to Alice. Alice touches her newly acquired box to her old box. The two glow red or green, and she communicates the result to Bob. If the result was green, Bob leaves his coin alone. If the result was red, he reaches into his opaque box and turns the coin over. At that point the "teleportation" process is completed.

In what sense is it completed? Only in this: If Charlie has the promise that Alice and Bob went through all the actions described above, then he can safely ascribe the same probability p to the coin in Bob's box (i.e., p that it will be heads) that he had originally ascribed to the coin in his own box. In other words, Charlie has everything it takes to update his epistemic state about the orientation of the coin in Bob's box to what he had originally thought of the coin in his own box.

Is this wildly exciting? The stuff that would make headlines in papers all around the world and be called "teleportation"? At the material cost of transferring a single bit from Alice and Bob, an infinite number of bits (in the form of the real number p) has been transferred between the two sites

instantaneously? Not at all! The only thing that was materially transported from one site to the other was a single bit (that the boxes glowed red or green). The rest was just "conditionalizing" or "updating". And there is no mystery whatsoever in that. Dumb and dull, it would never make a headline.

And so too, I would say with quantum teleportation, even though it has built my career. (Our Caltech experiment of it has over 600 citations now.)

At the end of this note, I'll place some of the correspondence I had with Asher Peres as we were writing our ill-fated paper "Quantum Theory Needs No 'Interpretation'." (Of course the whole paper is about a particular interpretation, but most critics never read beyond the title.) In it, we had a paragraph on quantum teleportation and how it helps to illustrate the epistemic nature of quantum states. And boy did we fight about how to write that paragraph, and even—at that time—about the meaning of teleportation. Seeing some of our own wrangling may be of use to you, if you care to explore this idea further. The main points of the part attached below are 1) how there is no THE quantum state for a system (there as many as there are potential observers) and how teleportation makes use of that idea, and 2) when Alice performs her actions, nothing physical changes on Bob's side.

Finally, let me comment on:

Hans' Seemingly Black-and-White-ism or All-or-Nothing-ism

Two or three times now, I have heard Hans equate the idea of interpreting quantum states epistemically (i.e., as knowledge or like a Bayesian degree of belief or whatever) with giving up on the project of thinking that *physics* has something to say about the real world (i.e., presumably the world as it is without any knowers or even Bayesians). That is altogether too glib of a position, and I want to do whatever I can to ease him out of thinking it.

The stuff that Caves and Schack and Spekkens and Appleby and Timpson (and sometimes Mermin and Unruh and Milburn and whoever else) have been talking about is much more subtle than that, and it should be recognized as such—this field of thought is at a respectable enough level now that it shouldn't be caricatured. I for one, for instance, pretty much characterize myself as a realist in the time honored sense: That there is a real world out there beyond our whim and fancy, and it is the task of science to hypothesize about its attributes and properties. I am not a philosophical idealist and certainly not a solipsist. And I don't think I am a positivist or an empiricist. Predominantly, I think I lean toward a kind of materialism (though tempered by a lot of pragmatic subtleties).

The main way I think Hans errs is that when he hears a phrase like "quantum states are states of knowledge," he thinks that that throws *everything* away to knowledge (or Bayesian degree of belief)—that nothing is actually hypothesized about the world. But quantum theory contains so much more than simply quantum states: There are Hilbert spaces; there are Hilbert-space dimensions; there are Hamiltonians; there are eigenvalues; there are notions of *separate systems* whose joint description is worked up through a tensor product. The quantum state is only one lonely piece of quantum theory.

When it comes to quantum states, our point is simply scientific open-eyedness and conceptual clarity: 1) Open-eyedness. We see so very many analogies between quantum states and incomplete knowledge—just look at Spekkens paper for more than 20 such—that it would be scientifically foolish to not take those analogies absolutely seriously and explore them for all they're worth. But 2) Clarity. We think we get even more in return, via easy solutions to some of quantum foundations' main supposed conundrums. From the epistemic view, there is no measurement problem, there is no nonlocality.

Loads of questions still remain, but one shouldn't be frightened of the methodology ... which is, as Spekkens put it,

> The diversity and quality of these analogies provides compelling evidence for the view that quantum states are states of knowledge rather than states of reality, and that maximal knowledge is incomplete knowledge. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

For instance, if you would ask Carl Caves, he would classify quantum states as epistemic, but he thinks that Hamiltonians represent a decent chance of remaining ontic terms within the theory (i.e., as representing an agent-independent reality). If you ask Spekkens, his own gut feeling is that quantum states represent incomplete knowledge of *relations without relata* (whatever that would mean), but particularly those relations are to be viewed ontically. As for myself, I tend to think that the ontology lies somewhere in a quantum system's receptivity or sensitivity to external interventions (whatever that would mean), and that receptivity is to be viewed ontically.

So, there is a range of ways of staying hard-headed about the idea that *quantum states are states of mind* without becoming a postmodern or a deconstructionist—without taking a walk with Derrida. And that is what our research program is about. Here's the way I put it in my quant-ph/0205039:

> This, I see as the line of attack we should pursue with relentless consistency: The quantum system represents something real and independent of us; the quantum state represents a collection of subjective degrees of belief about *something* to do with that system (even if only in connection with our experimental kicks to it). The structure called quantum mechanics is about the interplay of these two things—the subjective and the objective. The task before us is to separate the wheat from the chaff. If the quantum state represents subjective information, then how much of its mathematical support structure might be of that same character? Some of it, maybe most of it, but surely not all of it.
>
> Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as subjective information will we be in a position to make real progress in quantum foundations. The raw distillate left behind—minuscule though it may be with respect to the full-blown theory—will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.

Now, look in that, and tell me where is this view of yours that we drop the idea that "physics should be about reality"? I'll just pose that as a challenge to you.

Hopefully I'll see you both again tomorrow.

30-10-05 *Correlation without Correlata in the Strangest Places* (to N. D. Mermin)

I read these lines in Charles Krauthammer's column in the Washington Post this morning,

> This coldbloodedness is a trademark of this nation's most doctrinaire foreign policy "realist." Realism is the billiard ball theory of foreign policy: The only thing that counts is how countries interact, not what's happening inside. You care not a whit about who is running a country. Whether it is Mother Teresa or the Assad family gangsters in Syria, you care only about their external actions, not how they treat their own people.

and thought, "Is that really the billiard ball theory or rather correlation without correlata?"

31-10-05 *Cover to Cover* (to N. D. Mermin)

By the way, do you know this quote of Bohr's? I've been meaning to send it to you for a while (but kept forgetting), in case it complements the one I usually hear you quote (i.e., the one about "track[ing] down ... relations between the manifold aspects of our experience").

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

02-11-05 *Zeilinger, Me, and von Baeyer Makes Three?* (to H. C. von Baeyer)

I'm back, though "lunch" turned out to be effectively a two-day affair! To business now:

von Baeyerism 1: *I have been dipping into your marvellous book all weekend, and have a question (or seventeen). ...*

In January I will give an invited talk at the 75th anniversary of the American Association of Physics Teachers, in Anchorage, Alaska. The occasion is my receiving the Gemant Award of the AIP. My title is: "How I learned to stop worrying about Schroedinger's cat." I intend to chronicle my gradual adoption of your point of view about QM. Now the question:

It seems to me that the search is on for the irreducible kernel of QM, you call it the Zing, Maxwell, as a boy, would have called it "The GO of it." You and Zeilinger approach it from opposite ends: You by stripping away all "merely" information theoretic baggage, and he by guessing the answer. You have not yet succeeded in getting down to the bottom, and he has a long way to go before he builds up to the full theory. Analysis v. synthesis, top-down v. bottom-up.

Is this a fair way to characterize your approach?

Thanks by this; I'm certainly flattered. And I hope you'll ask me all seventeen eventually. The way to do good physics is to write good physics, and in my case, I need good questions before I can even hope for the latter.

And, by the way, speaking of good books, I really did enjoy reading yours last week. I think, whether it was intended to be or not, it is a great service to the quantum Bayesian community—sort of a mortar to soften up the popular beach—and I'm grateful for that. For the heck of it, below, I've decided to type in the little notes I made on my bookmark as I read it—they record my agreements and disagreements and a little of my study of the English language. Particularly, you'll note a remark I made about something on page 38. It refers mostly to your sentence,

> If the wave function is nothing but a storehouse of information needed to make correct predictions, then the stuff of the world is really, at bottom, information.

I hope my talk at W&M emphasized that I would be in disagreement with that.

And in fact, to start to answer your question, I think that disagreement probably captures the greatest gulf between the views Anton and I are each striving to develop. It's not a difference of technique (analysis vs. synthesis) per se, but of our *guesses* of an ontology—I think they are opposed to each other.

If you can give me a few days, I plan to come back to you with a much more complete answer. But first I want to re-read all of Zeilinger and Brukner's stuff. This will be a good opportunity for me to flesh out the similarities and differences of what we're hoping to get at. In any case, I'll get back to you long, long before your January talk.

By the way, congratulations on the teaching award. But I hope Anchorage isn't too cold in January!

— — — — — — — — — — — — — —

Notes on H. C. von Baeyer, *Information: The New Language of Science*

vB asks "why information not like energy" – 9, 10, 33
special relativity as inserting the observer into physics – 12
Wheeler, subjectivity – 13
split between objective and subjective – 13, 14
Wheeler quote – 15
relations without relata – 23, 24, 52-53
(Schroedinger) information as surprise – 25
Gell-Mann sobriety – 33
*bad inference – 38
non-Bayesian view of probability? – 57, 58, 72
questionable interpretation of Bohr – 64, 65
probability as informed guess – 75
not "avoid" but "masks" – 95
quantum random numbers – 107
hearing loss – 125, 126
Wheeler grafitto from Pecan St. – 127, 128
as it were – 132, 134, 155, 166, 201, 231
Nobel prize meat – 140, 141
probability does not exist – 172
bad language for superposition – 180, 181
interventions – 183
measurement as creation – 184
qubit as state rather than system – 187, 188
the godfather of soul – 191
fey neurasthaenic – 193
analogy, maybe more accurate than you think – 195, 196
quantum crypto distinct idea from quantum computing – 197
physics as comparison – 203
Boltzmann titles – 223
correct phrase on teleportation! – 225
Zeilinger or Rovelli first? – 226
information about what? whose information? – 227

entanglement not necessarily the key to quantum crypto – 230

02-11-05 *And Holladay* (to H. C. von Baeyer)

I just can't get my email right today; I keep remembering things that I forgot to reply to.

von Baeyerism 2: *Full disclosure: Wendell Holladay was my thesis adviser, but he died last year. De mortuis nihil nisi bene. (Of the dead, say nothing but good things.)*

I am sorry to hear about that. Were you close? Probably. In reviewing what I wrote about him in *Notes on a Paulian Idea*, at least I notice that a reply to his letter seemed to be the hardest part of the paper for me to construct—i.e., apparently he gave me a run for the money. He was probably a great guy. Again, I'm sorry.

02-11-05 *Carry Cameras, not Guns* (to C. H. Bennett)

I really enjoyed thumbing through your talk. Particularly, I enjoyed this quote

> Could it be that every major past phenomenon, say Sapphos other poems, or Jimmy Hoffa's murder, can be recovered from physical evidence in principle, if not in practice?
>
> To believe otherwise is venturing dangerously close to the deconstructionist view, abhorred by most scientists, that history is not what "actually" happened, only what we think happened.

as food for thought. It put the importance of the black-hole information question in a new light for me.

But also it reminded me of some stuff I once read about the interesting things that can happen when one tries to mesh the idea of "physical indeterminism" together with the ideas of "history" and "archeology" as autonomous sciences. As it happens, I have an old PDF file which includes a description of George Herbert Mead's thoughts on the subject. I'll attach it for your fun. (Ignore the stuff I wrote at the beginning of it, and just go straight to the copied description of Mead himself on page 2.) By the way, Mead—one of the founders of American pragmatism—was born in South Hadley, MA in 1863. So I suspect his ghost is practically a neighbor to you guys in Wendell on the weekends.

07-11-05 *The Nauseating Terminus* (to G. L. Comer)

I thought your note was on the mark. What it shows is that even in our state of relative ignorance about the goings on of the universe, we should still expect life to arise somewhere/somehow simply due to the vast size of this universe.

But also keep in mind that, from the Bayesian view, probabilities (and the surprise or lack of surprise they entail when one sees the outcome of a trial) represent nothing other than one's degrees of beliefs. Probabilities are not part of the furniture of the universe. In particular, I suspect if we understood chemistry a lot better, then—conditioned on that knowledge—life wouldn't look so surprising at all: That is, *with respect to* a better state of knowledge, one wouldn't have to shore up one's argument for the inevitability of life by statements of the vastness of the universe. It'd be a simple consequence of chemistry as such (and not rare initial conditions).

So that in the end, as scientists we would say: Because our fundamental fields have these Hamiltonians, life is effectively inevitable. The religious-minded at that point should say, "So who

ordered those Hamiltonians? You yourself contemplate that they could have been otherwise. Hence, at just that point, there's the concrete expression of an intelligent design. From it, as you have just proved to me dear scientist, everything inevitably flows." And how do you combat that? We each have our terminus of inquiry: ours is an equation, and theirs is an old man behind the equation. By why should either of us terminate? We should both have a horrible sense of dissatisfaction.

07-11-05 *The Second Terminus* (to G. L. Comer)

Comerism 9 : *I'm also not sure that I've assumed that probabilities are real. I readily admit that I introduced them via the question asked.*

I didn't mean to imply that you weren't being a good Bayesian. I was just changing the subject a bit ... so that I could make the point that the religious can always find a place for their old man in the sky. For they can always pick up wherever we (the scientists) say THIS is the fundamental equation or THAT is the necessary class of initial conditions. On the other hand, they think they're getting somewhere deeper by positing their old man in the sky than we are with our fundamental equation or initial condition (or both). To that I ask, why have they gotten any deeper: It looks to me to be of effectively the same level. For I can ask of their god, who ordered that? In fact, one can turn their own argument upon themselves. If they ever say that the beauty or the complexity of the world around us cannot be explained without a creator, then I say that the beauty or complexity of that creator could not be explained without an even more powerful and harmonious creator. And so on ad infinitum. The only leg I've ever see them stand on is to reply that, "But God needs no explanation" ... end of argument. And then I retort, "But then why do you demand it of my equations." And of course it just goes round and round.

Anyway, sorry, I wasn't disagreeing with you ... just changing the topic a little. But also, I wasn't even sticking to one topic really. The point about my personal *belief* that life is largely independent of initial conditions (just like planet formation seems to be, for instance), was really a side point.

Or to say it another way, mostly in my last note I was just ranting because the religious right have pissed me off too.

08-11-05 *Quibbles, Actions, and Reading* (to H. C. von Baeyer)

von Baeyerism 3 : *Re Bayesian: I have a couple of the volumes of the Pauli letters here on my desk. If you look up Bayes in the index, you come to a letter from P. to F. dated 12 July 1952. It is distinctive in including the dream-drawing of quantum mechanics and deeper reality that you are going to paste into your* `PauliProject.pdf`*. Where did you see it? It also appears in a letter to Jung a year later. Is Pauli's letter available in English?*

Fierz wrote at least two detailed drafts of a reply. One bit that you would love is this: Pauli brings up people who believe – or disbelieve – a theory so much that they remain unconvinced by statistical evidence. They stubbornly continue to wait for a measurement that disagrees significantly with the theory. Pauli suggests that the state, or society, could then declare that these people belong in an insane asylum. But he himself doesn't dare to make such rules.

Fierz replies:

> I am totally against the putative opponents, in the name of the state or of society, to hold their views. Rather, one should challenge them to a *bet*. If they accept it, they

> will *lose their money*, which proves that they have not lost their reason, but everything else, in particular, money.
>
> On the other hand, if they reject the bet, it is demonstrated that their conviction is without practical consequences, in other words, that they they don't really believe what they claim.
>
> I agree with your claim, and have always shared it, that purely logically nothing follows from the laws of Bayes and Bernoulli about a concrete single event. This is the answer to your question 2.
>
> However, the maxims of our practical actions can never be justified logically. If that were the case, empirical observations could be reduced to logic. Every action requires a decision, and that is an act of will.

Betting good money, that's where it's at!!!

I like it! What a great quote. Find me a thousand more like that, and I'll be forever in your debt.

Concerning your earlier Zeilinger question, I did take the time to read the Zeilinger and Brukner-Zeilinger papers. Also I reread Hall's and Timpson's commentary on them. (I suspect Mana's commentary is also good, but I didn't discover that one until this morning; so I can't say that I read it, though I know him and think he is a very promising student.) Timpson's comment in particular, I think is very relevant for what you were wanting to know. So I would suggest you have a look at it, if you haven't already. Here are the coordinates: `quant-ph/0112178`. The paper is written in too much of an argumentative style for my tastes, but I do agree with the substantive points.

Particularly, I agree that there's nothing to Zeilinger's principle *as presently sketched* without already invoking quantum mechanics itself. See Timpson's Appendix A. This is not to say that we may not gain some insight from these ruminations. As I said before, I think Spekkens' toy model is a great example in that regard. So Zeilinger's ideas are certainly important ideas, even if ultimately flawed. Indeed, I think what Spekkens' paper really shows is that the heart of quantum mechanics is something beyond the Zeilinger principle. For one might say that Spekkens' toy model already effectively embodies the Z principle—formally through its "knowledge balance principle"—and yet the model is *not* quantum mechanics. Rather it is a local hidden variable theory. As Rob says in his abstract, "A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with this research program."

For my own money, I now think the real heart of the theory is in the Kochen-Specker theorems; and the Z principle just doesn't capture that. In particular, I think the KS theorems are likely the most direct statements we yet have that the "observer cannot be detached" in an ultimate account of reality. Particularly, that's where I think our research needs to focus most: Finding a way to justify a Kochen-Specker-like theorem directly from the idea of engaged/activating observers, and bypassing the details of quantum mechanics. But I just don't see how to go that route yet.

Finally, let me go back to what I view as the more fundamental disagreement in outlooks for Zeilinger and me at the moment. (The disagreement may not be a permanent thing, but it is definitely here for the moment.) The point is made by Timpson pretty clearly:

> Before stating the Foundational Principle, it is helpful to identify two philosophical assumptions that Zeilinger's position incorporates. The first is a form of phenomenalism: physical objects are taken not to exist in and of themselves, but to be mere constructs relating sense impressions [Footnote: Here I take phenomenalism to be the doctrine that the subject matter of all conceivable propositions are one's own actual or possible

experiences, or the actual and possible experiences of another.]; the second assumption is an explicit instrumentalism about the quantum state:

> The initial state ... represents all our information as obtained by earlier observation ... [the time evolved] state is just a short-hand way of representing the outcomes of all possible future observations.

Zeilinger and I are certainly in agreement on the second assumption, but it is the first that drives a wedge between us presently. In fact, just last week, I wrote Bas van Fraassen and Hans Halvorson these words:

> I for one, for instance, pretty much characterize myself as a realist in the time honored sense: That there is a real world out there beyond our whim and fancy, and it is the task of science to hypothesize about its attributes and properties. I am not a philosophical idealist and certainly not a solipsist. And I don't think I am a positivist or an empiricist. Predominantly, I think I lean toward a kind of materialism (though tempered by a lot of pragmatic subtleties).

That's the key point: As far as I can tell, I think I almost lean toward a kind of materialism. Metaphorically: Outside of the alchemist is the philosopher's stone. Without it, there is nothing to enact the transmutation of the baser metals or of the alchemist himself.

If you decide after reading a good portion of the stuff I've already sent you that I'm not completely crazy, and you're interested, then I'll go on a limb and tell you more about what I meant in the last two lines above. And I'll tell you why I'm really so interested in this Pauli-Fierz correspondence (and Fierz's own writings). But if you're already thinking I'm a little crazy, I don't want to fuel the fire.

08-11-05 *A Few More Things* (to H. C. von Baeyer)

At your house, you asked why I called my compendium "The Activating Observer" rather than "The Participating Observer" or some such. Looking in the LaTeX file, I see that I marked out at least this many other tentative titles:

The Undetached Observer:
The Catalyzing Observer:
The Malleable Reality:
A Malleable Reality:
The Malleable Substrate:

von Baeyerism 4: *While I am happy that you are reading my book carefully, I won't spend much effort on defending it. I have learned a lot since I wrote it, and am much more of a Bayesian now.*

If you want to go to the real extreme—the place where Caves, Schack, and I now sit in the spectrum of Bayesianisms—then let me recommend you read these things:

1. Bruno de Finetti, "Probabilism: A Critical Essay on the Theory of Probability and on the Value of Science," Erkenntnis **31**, 169–223 (1989).

2. Bruno de Finetti, *Theory of Probability* (Wiley, New York, 1990), volume 1.

3. Richard Jeffrey, *Subjective Probability (The Real Thing)* (Cambridge University Press, Cambridge, UK, 2004).

4. Richard Jeffrey, *Probability and the Art of Judgment* (Cambridge University Press, Cambridge, UK, 1992).

The two de Finetti pieces are phenomenal, and they were life-changing for me (particularly the first reference, *after i read it the fourth time*!). (It's this life change that is recorded in my samizdat, *Quantum States: What the Hell Are They?*) My only warnings are: 1) to take his hint of solipsism in Section 2 of the first reference above with a grain of salt—he backs off on it by the time he gets to his book—, and 2) and to disregard his support for Mussolini and fascism in the last section of the same paper. Those things are independent of the rest of the development, and one should not get distracted by them—the rest of the paper really is phenomenal.

von Baeyerism 5: *If you look up Bayes in the index, you come to a letter from P. to F. dated 12 July 1952. It is distinctive in including the dream-drawing of quantum mechanics and deeper reality that you are going to paste into your* `PauliProject.pdf`*. Where did you see it? It also appears in a letter to Jung a year later. Is Pauli's letter available in English?*

Those figures that I'm going to paste in refer to two letters from Pauli to Jung: One from 27 May 1953 and one from 31 March 1953. The Pauli-Jung correspondence has been translated into English; that's where I got those long quotes from. They start up on pages 146 and 144 of `PauliProject.pdf`, respectively.

There. Now I think I've answered everything you've ever asked me.

10-11-05 *Delirium Quantum* (to B. C. van Fraassen)

I've got to say, I really, really enjoyed today's discussion. Now, I'm getting my money's worth! I thought you did a great job of giving a kind of (perhaps momentary) suspension of disbelief about Rovelli's ideas—i.e., making an honest attempt to get your head around them—and I thought Maudlin did a great job as skeptical counterpoint. I was very pleased with the food for thought it all gave me, and it made the drive back to Cranford go like a flash.

Particularly I'm struck that there really is more of a similarity between Rovelli's outlook and our quantum-Bayesian outlook than I had thought. Of course, there are plenty of differences too (and likely big ones). But I think it is much worthwhile for me to rethink all the various issues surrounding his view.

Anyway, let me attach a file that I hope will amuse you; it's titled `DeliriumQuantum.pdf`. The parts I'm interested in your reading connect mostly to your remarks today about Rovelli's view of the *domain* of quantum theory. In particular, I shoot for some explication of how I can at once say "quantum mechanics is incomplete"—I do that a lot now—and still emphatically deny that it would be worthwhile to try to complete it with a hidden-variable theory. Particularly, I like the analogy to do with a map vs. a globe that you'll see at the end of this file. In total, the only parts that I really care for you to see are Section 4 "Me, Me, Me," Section 6 "Preamble," and Section 7 "B." Of course, you can look at all the rest if you got nothin' better to do, but I don't think the other parts are connected to your discussion today.

I apologize in advance that the pieces I'm recommending were written in a more overtly William Jamesian style than usual (even by my own standards). Sadly, I find more clarity for myself when I do that, but I suspect not all my readers do! I remember I once gave some lectures at Case Western Reserve U., and one of the professors wrote to John Preskill, "During Fuchs's lectures everyone became convinced that quantum information theory was the most important thing ever. But when he left, no one could remember why!" You may have a not-unconnected reaction to these writings.

I'm taking the family on a little "road trip" vacation to New Hampshire for the next four days—starting in about six hours if I ever get to sleep—but I suspect I'll be dropping in on you electronically over the next few days. I'm quite excited about this, and I will probably spout things from time to time as they come to mind. I hope you don't mind: As always, feel free to ignore these notes and chalk them up to being my diary entries if you wish.

10-11-05 *"Action" instead of "Measurement"* (to B. C. van Fraassen)

Let me also elaborate a little on the spiel I gave today condoning the word "action" more than the words "measurement" or "question" in the context of "quantum measurement."

The following explanation comes from an email to another friend last Spring. I do think the very word "measurement" is fraught with trouble, particularly for what Rovelli and us quantum-Bayesians are trying to get after.

> Anyway, the way I view quantum measurement now is this. When one performs a "measurement" on a system, all one is really doing is taking an ACTION on that system. From this view, time evolutions or unitary operations etc., are not actions that one can take on a system; only "measurements" are. Thus the word measurement is really a misnomer—it is only an action. In contradistinction to the old idea that a measurement is a query of nature, or a way of gathering information or knowledge about nature, from this view it is just an action on something external—it is a kick of sorts. The "measurement device" should be thought of as being like a prosthetic hand for the agent—it is merely an extension of him; in this context, it should not be thought of as an independent entity beyond the agent. What quantum theory tells us is that the formal structure of all our possible actions (perhaps via the help of these prosthetic hands) is captured by the idea of a Positive-Operator-Valued Measure (or POVM, or so-called "generalized measurement"). We take our actions upon a system, and in return, the system gives rise to a reaction—in older terms, that is the "measurement outcome"—but the reaction is in the agent himself. The role of the quantum system is thus more like that of the philosopher's stone; it is the catalyst that brings about a transformation (or transmutation) of the agent.
>
> Reciprocally, there should be a transmutation of the system external to the agent. But the great trouble in quantum interpretation—I now think—is that we have been too inclined to jump the gun all these years: We have been misidentifying where the transmutation indicated by quantum mechanics (i.e., the one which quantum theory actually talks about, the "measurement outcome") takes place. It should be the case that there are also transmutations in the external world (transmutations in the system) in each quantum "measurement", BUT that is not what quantum theory is about. It is only a hint of that more interesting transmutation. And, as you know, somehow out of all this I think of the agent and the observer as being, together, involved in a little act of creation that ultimately has an autonomy of its own—that's the sexual interpretation of quantum mechanics. (However the ideas in that last sentence are a little murkier than what I'm trying to get at now.)
>
> Does "measurement" in this new sense explicitly require consciousness (whatever that is)? I don't think so. But it does require some kind of nonreductive element—some kind of higher-level description that cannot be reduced to a lower-level one. Here's the way I've been putting it in my last three lectures, when talking more particularly about quantum mechanics from the Bayesian perspective. I point out that a Bayesian

is, roughly speaking, someone who believes that without gamblers, there cannot be probabilities. Probabilities are not external to gamblers. Then someone always asks, must you have consciousness to have probabilities? And I say, "No." Take as an example my laptop computer loaded with a Bayesian spam filter. It is a perfectly good gambler in the Bayesian sense, but I think most people would be hard-pressed to call it conscious. Similarly, I think we're going to ultimately learn an analogous lesson about all this transformation/transmutation/ creation/measurement business.

On the other hand, I do find myself being tickled toward a more Whiteheadian-like view that, whatever this higher-level description is, every piece of nature has more or less of it, from people all the way to stones and atoms. It's just that you have to be on the inside of the philosopher's stone to see it.

10-11-05 *Wheeler's 20 Questions and Nordheim* (to B. C. van Fraassen)

Below is Wheeler's account of the original incidence. It comes from

> J. A. Wheeler, "Bohr, Einstein, and the Strange Lesson of the Quantum," in *Mind in Nature: Nobel Conference XVII, Gustavus Adolphus College, St. Peter, Minnesota*, edited by R. Q. Elvee (Harper & Row, San Francisco, CA, 1982), pp. 1–23, discussion pp. 23–30, 88–89, 112–113, and 148–149.

I'll work on digging up that Nordheim / von Neumann reference I told you about. I don't think it is the Hilbert / Nordheim / von Neumann paper that one easily finds on the web, but something later. But maybe I'm wrong (and in more than one way).

> What is the difference between a "participatory" reality and a reality that exists "out there" independent of the community of perceivers? An example may illustrate a little of the difference. Edward Teller and I, and a dozen other guests, were sitting in the living room of Lothar Nordheim in Durham after dinner. From general conversation we moved on to the game of twenty questions. One, chosen as victim, was sent out of the room. The rest of us agreed on some implausible word like "brontosaurus." Then the victim was let back into the room. To win, he had to discover the word with no more than twenty yes/no questions. Otherwise, he lost. After we had played several rounds, my turn came and I was sent out. . . .

14-11-05 *Questions, Actions, Answers, and Consequences* (to B. C. van Fraassen)

> But physicists are, at bottom, a naive breed, forever trying to come to terms with the 'world out there' by methods which, however imaginative and refined, involve in essence the same element of contact as a well-placed kick.
>
> — Bryce DeWitt and Neill Graham, 1971

> I think that the sickliest notion of physics, even if a student gets it, is that it is 'the science of masses, molecules, and the ether.' And I think that the healthiest notion, even if a student does not wholly get it, is that physics is the science of the ways of taking hold of bodies and pushing them!
>
> — W. S. Franklin, 1903

Thanks for the note. In my own case, I didn't get nearly as much reading and thinking done over the weekend as I had hoped to—indeed, the time turned out to be a great vacation for the kids, but not so much for dad.

van Fraassenism 3 : *I thought I would after all not follow you in replacing the term "measurement", despite all the bad effects old connotations have had in various discussions.*

Fine. That's your business. I just offered my two cents because Maudlin made one of his impassioned spiels precisely on the point of what "measurement" could mean in the Rovelli context—it seemed that some clarification was in order, and particularly worthwhile reiterating the point that if better language were used, quantum "measurement" might stop looking so unfamiliar.

van Fraassenism 4 : *We need to bracket the old connotations such as that a measurement result reveals a pre-existing value for the measured observable. But I think we can do that because:*

Not trying to sway you anymore, but let me comment on a couple of these.

van Fraassenism 5 : *there is a certain kind of retrodictive inference possible also on the basis of qm measurements. For a long time the paradigm was a source preparing a stream of particles in a certain state – measurements on samples taken from the stream give a good basis for conclusions about just what state the source was preparing, and these conclusions can then be used to predict the outcomes of further measurements made on later samples of the stream*

This is what we in quantum information call quantum-state tomography. One can indeed think of a quantum measurement outcome as *giving information* in the old standard sense in that case and not simply being the "unpredictable consequence of one's action." But then "giving information" is quantified by Shannon's "mutual information," $I(X, Y)$ and not simply by his entropy function $H(Y)$. That is, one has two random variables in the game—one treated classically, namely the "unknown preparation" X, and the other one purely quantum mechanical, the result Y of the measurement interaction. Those two variables have quite different roles, and one indeed would not want to think of X as the "consequence of one's interaction." On the other hand, without making explicit mention of X one has no means for thinking of the elicitation of Y as giving information about anything at all. Before seeing the value of Y, one can expect to be *surprised* to the extent quantified by $H(Y)$, but that's where the story stops.

[For a more detailed Bayesian-like development of this point, you might have a look at our paper "Unknown Quantum States and Operations, a Bayesian View," `quant-ph/0404156` and some of the references therein. Particularly the Introduction and Concluding section might be of some interest to you with regard to the present discussion.]

The only point I want to make to you with regard to your remark above is that, for these reasons, I would say it has no bearing on the issue at debate: I.e., whether it is better to think of a "quantum measurement" as simply an action with an unforeseeable consequence, or rather as a kind of "question-asking" or "information-gathering." It is tangential.

On a completely different subject,

van Fraassenism 6 : *Writers on the subject have emphasized that the main form of measurement in quantum mechanics has as result the value of the observable at the end of the measurement – and that this observable may not even have had a definite value, let alone the same one, before.*

your phrase "MAY NOT even have a definite value" floated to my attention. I guess this floated to my attention because I had recently read the following in one of the Brukner/Zeilinger papers,

> Only in the exceptional case of the qubit in an eigenstate of the measurement apparatus the bit value observed reveals a property already carried by the qubit. Yet in general the value obtained by the measurement has an element of irreducible randomness and therefore cannot be assumed to reveal the bit value or even a hidden property of the system existing before the measurement is performed.

I wondered if your "may not" referred to effectively the same thing as their disclaimer at the beginning of this quote. Maybe it doesn't. Anyway, the Brukner/Zeilinger disclaimer is a point that Caves, Schack, and I now definitely reject: From our view all measurements are generative of a NON-preexisting property regardless of the quantum state. I.e., measurements never reveal "a property already carried by the qubit." For this, of course, we have to adopt a Richard Jeffrey-like analysis of the notion of "certainty"—i.e., that it too, like any probability assignment, is a state of mind—or one along (my reading of) Wittgenstein's—i.e., that "certainty is a tone of voice"—to make it all make sense, but so be it.

I'm curious to understand whether Rovelli's writings already specify an opinion on the issue.

14-11-05 *Nordheim Again* (to B. C. van Fraassen)

Following up. The paper Alex Wilce talked about was apparently,

> Hilbert, D., von Neumann, J., and Nordheim, L., "Uber die Grundlagen der Quantenmechanik," Math. Annalen **98** (1927) 1–30.

It looks now, however, like I was wrong about these guys talking about the possibility of a quantum logical approach. Instead what this paper seems to be famous for is simply giving some guidelines for what it would mean to axiomatize quantum theory. Von Neumann, as best I can tell, only first talked about lattices (in the quantum context) in a 1935 letter to Birkhoff and first in print in a 1936 paper (by himself, not the one with Birkhoff).

Sorry for feeding you some misinformation.

15-11-05 *Canned Answers* (to B. C. van Fraassen)

> One does not infer how things are from one's own certainty.
>
> Certainty is as it were a tone of voice in which one declares how things are, but one does not infer from the tone of voice that one is justified.
>
> — L. Wittgenstein, *On Certainty*, par. 30

> But de Finetti's philosophical view does see determinism as a state of mind masquerading as a state of nature, and sees causality as a fancied magical projection into nature of our own patterns of expectation. Beneath the mask of determinism is a state of mind—certainty—that is intelligible enough ...
>
> Certainties are not the only states of mind that are made to masquerade as states of nature. Probabilistic previsions are also magically projected into nature, to produce "real" probabilities, i.e. "objective chances" or probabilistic "propensities", which de Finetti rejects as firmly as he does deterministic causality.
>
> — R. Jeffrey, "De Finetti's Radical Probabilism"

Addressing your questions:

van Fraassenism 7: *Suppose that an observer assigns eigenstate $|a\rangle$ of A to a system on the basis of a measurement, then predicts with certainty that an immediate further measurement of A will yield value a, and then makes that second measurement and finds a. Don't you even want to say that the second measurement just showed to this observer, as was expected, the value that A already had? He does not need to change his subjective probabilities at all in response to the 2nd measurement outcome, does he?*

It is not going to be easy, because this in fact is what Schack and I are actually writing a whole paper about at the moment—this point has been the most controversial thing (with the Mermin, Unruh, Wootters, Spekkens, etc., crowd) that we've said in a while, and it seems that it's going to require a whole paper to do the point justice. But I'll still try to give you the skinny of it:

- Q) He does not need to change his subjective probabilities at all in response to the 2nd measurement outcome, does he?
- A) No he doesn't.
- Q) Don't you even want to say that the second measurement just showed to this observer, as was expected, the value that A already had?
- A) No I don't.

The problem is one of the very consistency of the subjective point of view of quantum states. The task we set before ourselves is to completely sever any supposed connections between quantum states and the actual, existent physical properties of the quantum system. It is only from this—if it can be done, and of course we try to argue it can be done—that we get any "interpretive traction" (as Chris Timpson likes to say) for the various problems that plague QM. (In that regard, you might look at Timpson's explanation of the point in the Envoi at the end of his thesis, starting page 223, in connection with his discussion of the problem of the "factivity of knowledge" on pages 176-182, particularly footnote 4.)

This may boil down to a difference between the Rovellian and the Bayesian/Paulian approach; I'm not clear on that yet. I'm looking at the first box on page 3 of your last week's handout at the moment. Rovelli relativizes the states to the observer, even the pure states, and with that—through the eigenstate-eigenvalue link—, YOU SAY, the values of the observables. I'm not completely sure what that means in Rovelli-world yet, however.

I, on the other hand, do know that I would say that a measurement intervention is always generative of a new fact in the world, whatever the measurer's quantum state for the system. If the measurer's state for the system HAPPENS to be an eigenstate of the Hermitian operator describing the measurement intervention, then the measurer will be confident, CERTAIN even, of the consequence of the measurement intervention he is about to perform. But that CERTAINTY is in the sense of Jeffrey and Wittgenstein above—it is a "tone of voice" of utter confidence. The world could still, as a point of principle, smite the measurer down by giving him a consequence that he predicted to be impossible. In a traditional development—with ties to a correspondence theory of truth—we would then say, "Well, that proves the measurer was wrong with his quantum state assignment. He was wrong before he ever went through the motions of the measurement." But as you've gathered, I'm not about traditional developments. Instead I would say, "Even from my view there is a sense in which the measurer's quantum state is WRONG. But it is MADE WRONG by the ACTUAL consequence of the intervention—it is made wrong on the fly; it's wrongness was not determined beforehand." And that seems to be the main point of contention.

I think I say some of this better, and give better argumentation for it, in the attached document, but I'll let you be the judge of that. Particularly, I hope the long de Finetti quote helps here. The file is Certainty.pdf.

I think I have more to say in a positive vein on Rovelli, but I'll come back to that after lunch.

17-11-05 *Cash Value* (to H. Halvorson)

I probably laid too many cards on the table yesterday when you attempted to elicit my opinion on Bohmian mechanics. For, one of the arguments I gave was essentially one I had made in a referee report some time ago. Still it's always hard to keep these kinds of things secret—they generally so infect one's way of life that surely everyone can already guess the perpetrator.

Anyway, in principle, I am not a priori against the introduction of extra, unobservable entities into a theory (over and above, say, its rawest statement). But to make me care at all about them, they have to have a "cash value." This is why I gave the example of electromagnetic potentials. In classical electromagnetism, they too are unobservable; only the fields themselves are observable. But often it is quite useful to explicitly introduce the potentials and work in terms of them when solving a problem (if one wishes, one can even believe they're as real as real can be)—their facilitating my problem solving is their cash value and my reason for taking them seriously.

My argument is in the same vein when it comes to Bohmian mechanics.

Who knows, the Bohmians might one day indeed win out with me by giving me a good dollop

of cash value. But I'm not banking on it. And life is too short to pursue every wacky theory *simply* because it is consistent. One has to follow one's instincts, and my instinct is that our biggest confusions with quantum theory come about because the only way we know how to word it presently is through the use of *too many* structures, not too few. That is, the number of distinct concepts in the axioms needs trimming, not fattening.

But I don't say anything that I haven't already said a thousand times. And in your case, I'm preaching to the choir anyway!

01-12-05 *The Swedish Bayesian Team* (to D. M. Appleby & Others)

I'm writing this note to see if you might be available to come to a meeting in Växjö, Sweden, June 4-9, 2006? The general meeting is *Foundations of Probability and Physics – 4* organized predominantly by Andrei Khrennikov along the lines of his previous meetings, but he has given me a small budget to contribute toward trying to attract a group of quantum-Bayesians. If this meeting goes like the ones I've been associated with before, it will turn out to be a pleasant and productive time for the group I hope to attract there. (It is a nice village with a couple of bars and a nice lake to walk around and discuss things.)

Ideally, I'd like to have you, Itamar Pitowsky, Richard Gill, Matt Leifer, Ariel Caticha, and Rüdiger Schack there to contribute toward a discussion. [. . .]

The sort of idea I have in mind for a focused discussion can be read in some of the words I used in a recent abstract I wrote for my talk at the APS March meeting. I'll place those words below to give you a kind of feeling.

> Opening from Fuchs's APS abstract:
>
> In the neo-Bayesian view of quantum mechanics that Appleby, Caves, Pitowsky, Schack, the author, and others are developing, quantum states are taken to be compendia of partial beliefs about potential measurement outcomes, rather than objective properties of quantum systems. Different observers may validly have different quantum states for a single system, and the ultimate origin of each individual state assignment is taken to be unanalyzable within physical theory—its origin, instead, ultimately comes from probability assignments made at stages of physical investigation or laboratory practice previous to quantum theory. The objective content of quantum mechanics (i.e., the part making no reference to observers) thus resides somewhere else than in the quantum state, and various ideas for where that "somewhere else" is are presently under debate—there are adherents to the idea that it is purely in the "measurement clicks," there are adherents to the idea that it is in intrinsic, observer-independent Hamiltonians, there are adherents to the idea that it is in the normative rules quantum theory supplies for updating quantum states, and so on. This part of the program is an active area of investigation; what is overwhelmingly agreed upon is only the opening statement. Still, quantum states are not simply Bayesian probability assignments themselves, and different representations of the theory (in terms of state vectors or Wigner functions or C^*-algebras and the like) can take one further from or closer to a Bayesian point of view. It is thus worthwhile spending some time thinking about which representation might be the most propitious for the point of view and might, in turn, carry us the most quickly toward solutions of some of the open problems.

01-12-05 *Finite Sets (then limits) Policy* (to H. Halvorson)

Also, here's the quote from Ed Jaynes that I almost read aloud in your seminar yesterday. I suspect nothing deeper than this is going on with Redei's criticism of *whatever he is imagining is* the Bayesian approach. (I.e., there are many different approaches that one might think of as more or less Bayesian, and whichever one Redei is attacking, I doubt his problems are of any origin other than what Jaynes expresses below.)

> It is very important to note that [the theorems we have just proven] have been established only for probabilities assigned on finite sets of propositions. In principle, every problem must start with such finite sets of probabilities; extension to infinite sets is permitted only when this is the result of a welldefined and wellbehaved limiting process from a finite set. More generally, in any mathematical operations involving infinite sets the safe procedure is the finite sets policy:
>
> > Apply the ordinary processes of arithmetic and analysis only to expressions with a finite number of terms. Then after the calculation is done, observe how the resulting finite expressions behave as the number of terms increases indefinitely.
>
> In laying down this rule of conduct, we are only following the policy that mathematicians from Archimedes to Gauss have considered clearly necessary for nonsense avoidance in all of mathematics. But more recently, the popularity of infinite set theory and measure theory have led some to disregard it and seek shortcuts which purport to use measure theory directly. Note, however, that this rule of conduct is consistent with the original Lebesgue definition of measure, and when a wellbehaved limit exists it leads us automatically to correct "measure theoretic" results. Indeed, this is how Lebesgue found his first results.
>
> The danger is that the present measure theory notation presupposes the infinite limit already accomplished, but contains no symbol indicating which limiting process was used. Yet as noted in our Preface, different limiting processes—equally wellbehaved—lead in general to different results. When there is no wellbehaved limit, any attempt to go directly to the limit can result in nonsense, the cause of which cannot be seen as long as one looks only at the limit, and not at the limiting process.

27-12-05 *Regarding the Detached Observer Attached* (to H. C. von Baeyer)

von Baeyerism 6 : *Dear Chris, the detached observer is attached. I thought this footnote might interest you not so much for its text, which is less than limpid, as for its citations of the phrase.*

> In Wolfgang Pauli, *Scientific Correspondence with Bohr, Einstein, Heisenberg, a.o.* (Springer, 1996) Volume IV, Part I: 1950-1952, page 340 there is a three-page editorial comment on letter number [1263], from David Bohm to Pauli. The penultimate paragraph of this comment, in German, begins:
>
> > Pauli, on the other hand, under the influence of his psychological point of view, had progressively moved away from the classical-Cartesian assumption of the detached observer (cf. letters [1313] and [13 14])[14] who assumes a *fixed or pre-arranged game* behind the stage *(abgekartetes Spiel)* [letter 1388]:

> (in English) 'For me, however, it is much more satisfactory if the laws of nature themselves exclude in principle the possibility even to conceive the disturbances in the obervers (sic) own body and own brain connected with his own observations. . . .'

I believe the editor may be mixing up two different notions here, that of the detached observer, and that of the view of nature as a pre-arranged game, but I wanted to give the context of footnote 14. HvB

> [14]This term first appears in this context in Pauli's letter [1197] of 31 January 1951 to M.-L. von Franz and then again in the letter from M. Fierz [1288] of 10 October 1951. From now on Pauli used it more frequently in his lectures, publications, and letters. In a letter of 5 May 1953 to C.F. von Weizsäcker he mentioned this question in connection with his Kepler study: "As I hinted in my essay, it seems to me that Fludd was much closer to the symbolic formulation of the unity of existence, which in turn so paradoxically divides into 'observer' and 'external world' ('the cut'), than was Kepler, with his 'detached observer' of classical physics. The 'archaic' Fludd had the stronger feeling for the proposition that the 'position of the cut' is arbitrary (Heisenberg)." Similarly Pauli wrote on 15 May 1953 to M.-L. von Franz: "Holding on to these assumptions requires one to restrict oneself to statistical laws and to 'sacrifice' the individual case. Einstein, on the other hand, would like both to 'have his cake and eat it.' He yells 'incomplete', regresses to the detached observer of classical physics, and places 'world formulae' into a blue fog (which does not contain the observer.)

Thanks so much for the quote! I've just incorporated it into my "activating observer" compendium (and given you credit for the translation). But now you'll start to see my insatiable appetite! For I'd dearly love to see the FULL letters 1197, 1288, 1313, 1314, 1388, as well as the 5 May 1953 letter to von Weizsacker and 15 May 1953 letter to von Franz (for some reason there were no numbers listed next to the latter two).

In my existing collection, a search on the year 1951 only gives this quote from a Primas paper,

> Faced with the wholeness of the reality, scientists have been slow to accept the challenge of discussing the premises of Baconian science. They have even been reluctant to consider the simplest modification of a mechanistic world view, namely the inclusion of teleological considerations as an essential part of their discipline – a relatively simple problem for which in the framework of modern quantum mechanics all necessary tools are available presently. But Pauli was looking for an incomparably deeper approach which goes far beyond the limits of current quantum theory, and which includes *physis* and *psyche* as complementary aspects of the same reality. A reality containing both rational and irrational elements, and alchemic *conjunctio* of spirit and matter. In psychology as well as in physics, quaternity is taken to be an expression of all concepts of unbroken totality. In a letter Pauli wrote: "Ich bin ja auf Kepler als Trinitarier und Fludd als Quaternarier gestossen – und fühlte bei mir selbst, mit deren Polemik, einen inneren Konflikt mitschwingen. Ich habe gewisse Züge von beiden, sollte aber jetzt in der zweiten Lebenshälfte zur quarternären Einstellung übergehen. Das Problem ist, dass dabei die positiven Werte der trinitarischen Einstellung nicht geopfert werden dürfen." (Pauli to Fierz on October 3, 1951, quoted in Enz, p. 509). Pauli could not solve his dilemma of three and four which plays a great role in alchemy as "the axiom of Maria

> Prophetissa" ("Out of the Third comes the One as the Fourth"), and this shows that we are at the bare beginning to understand reality. But we again reached a turning point, a way of thinking is developing which is very different from that which has been dominant in the past decades, and which recognizes the repression of the irrational as incongruous.

(of which I don't know what the German part is saying) and this point of interest from a Lindorff paper

> This statement, taken together with the way Pauli associated the radioactive atom with the lapis, moved Jung to question whether 'the archetype and its effects and the effect of the active atom on its environment is not more than a metaphor.'
>
> With these thoughts in mind Jung said he would amplify his essay, 'Synchronicity: an acausal connecting principle'. He presented the material to the Psychological Club in two parts on 20 January and 2 March 1951. The essay, together with Pauli's work on Kepler, was subsequently published in book form.

The only materials I have from 1953 are a few letters from Pauli to Jung; nothing from Pauli or von Weizsacker or von Franz or Fierz.

At the moment, I'm particularly interested in better understanding the thing Pauli wrote to von Franz in what you sent me: "For me, however, it is much more satisfactory if the laws of nature themselves exclude in principle the possibility even to conceive the disturbances in the observers' own body and own brain connected with his own observations . . . " That really intrigues me! What exactly does he mean by that?

In that regard, let me send you a little compendium I sent to David Mermin, in fact just today. It's pasted below. As you'll see, I'm starting to lean very heavily on an alchemical analogy for building some imagery of what quantum measurement is all about. (I hinted a little about this to you before, but supplied no details.) And the quote you gave me above seems to touch on that in a way I hadn't seen before. This is something I really would love to explore.

Thanks so much again for all this! You're bringing me back to life in these lazy days between Christmas and the New Year.

I hope you and your wife are having a great holiday yourselves.

27-12-05 *Words of Yours I Liked and Didn't* (to N. D. Mermin)

Happy holidays! I've been missing you a lot lately, particularly as Rüdiger was visiting a couple of weeks back, and once again we picked up on our old thread of trying to develop a paper on "certainty"—you don't know how many times your image came to us in the conversations!

I've been meaning to write you, also, to thank you for having a copy of your book sent my way! It is beautiful. Per my promise, I will read it cover to cover; just give me a little time.

I have at least read the preface as of now, and I think I have discovered why you have been so nice to me all of these years (or at least tolerant of me)! Is it because my heart is in the right place, i.e., wanting to see quantum theory reduced to a statement that can be taught in an ordinary high school? I hadn't realized that you had made such an important point of this with Special Relativity. Wonderful!

Now, why am I writing you today in particular? It's because I had insomnia last night and I stumbled across Dennis Overbye's article in the *NY Times*. When I read this,

> In an essay in 1985, Dr. Mermin said that "if there is spooky action at a distance, then, like other spooks, it is absolutely useless except for its effect, benign or otherwise, on our state of mind."

I thought it was sheer genius, and I was ashamed that I didn't remember having read it before. I so wish I could write with your cleverness! Words I liked.

But when I read this,

> "I would say we have to be careful saying what's real," Dr. Mermin said. "Properties cannot be said to be there until they are revealed by an actual experiment."

I thought, "Tsk. Tsk." What trouble I continue to think that way of expressing things makes for our imaginations. Thinking of quantum measurement outcomes as "revealing properties" (whether they are pre-existent properties or not) is, I think, one of the biggest problems getting in our way of making a decent myth for the quantum mechanical world. Words of yours, this time, I didn't like.

By way of saying Happy New Year again, I'll send you three recent emails I wrote to Bas van Fraassen (all pasted below) where once again I'm groping to find the right words to say what quantum measurement "is" and what the clicks it gives correspond to. A quantum measurement outcome (considered on its own, without relation to other issues) is just a "surprise," full stop; it doesn't reveal anything. It's just a primitive reaction within the agent. Or at least that's the myth I'm groping for! (You'll see in the notes below that even I'm guilty of sometimes using the same words that I said "tsk, tsk" to you about. Once again—as always!—I learn from reading even the tiniest things of yours; somehow you cause me to introspect like few others do.)

Happy, happy, happy New Year!

28-12-05 *And a Cartoon* (to H. C. von Baeyer)

And I shouldn't forget to send you the cartoon that goes with the last note I sent you. It is attached as a `.jpg` file. Quantum theory is about all the stuff to the left side of the sparks—i.e., the actions and transformations of the agent. The stuff to the right of the sparks is the 'external reality,' the partial source of the sparks—it is the 'philosopher's stone,' without which the agent would never get transformed. The word 'catalyst' in the cartoon is a little misleading, as anyone with a little chemical training thinks of a catalyst as something that remains unchanged in the reaction; however, I'm thinking here of it in the broader sense of, say, the *American Heritage Dictionary* where a catalyst is simply "an agent that stimulates or precipitates a reaction, development, or change," full stop.

The reason I'm intrigued by the Pauli quote to von Franz is that he says, "For me ... it is much more satisfactory if the laws of nature themselves exclude in principle the possibility even to conceive the disturbances in the observer's own body and own brain connected with his own observations ..." That is, he seems to be talking about something going on to the left of the sparks too, not to the right of the sparks—i.e., just the part I claim quantum theory is about.

01-01-06 *Quantum Events and Propositions* (to W. G. Demopoulos)

Demopoulosism 2: *Maybe this is what separates us: I think an event's happening is strictly analogous to a proposition's being true. Just as events happen or fail to happen, propositions are true or false. So I'm not sure what to make of the idea that props are true or false but events just happen, seeing these as different ways of saying the same thing.*

Actually, I guess I fear that I agree with the ending part of this statement, i.e., that "I'm not sure what to make of the idea that props are true or false but events just happen, seeing these as different ways of saying the same thing." But, I think, the force of it goes in the opposite direction for me than it does for you. That is, I think I think [sic] one of the great lessons of quantum mechanics is that it is a call to arms to rethink what is meant by the truth value of a proposition. Here's the way I put it in one of the proposals I once made:

> Quantum Mechanics as a Powerful Hint. In my opinion, the most profound statement yet to come out of quantum theory is the Kochen-Specker theorem. For it licenses the slogan, "Unperformed measurements have no outcomes." This is just a beginning. If one canvasses the philosophic traditions for one that has significantly developed this slogan, one will find the now mostly-forgotten tradition of pragmatism fathered by William James and John Dewey. As a source of ideas for what quantum mechanics can more rigorously justify, no block of literature is more relevant: The connections between the two fields cry out for systematic study. Quantum mechanics holds the promise of drastically changing our worldview on the wide scale. It is time to let that happen.

Demopoulosism 3 : *On another matter, there is an idea in Pauli that your remark about quantum mechanics and classical probability theory reminded me of, but I can't recall it exactly. Doesn't he say somewhere that what QM does is to give a precise formulation of how the probabilities of propositions all stand with respect to one another without, however, specifying what actually occurs?*

(I've quoted this: "Just as in the theory of relativity a group of mathematical transformations connects all possible coordinate systems, so in quantum mechanics a group of mathematical transformations connects all possible experimental arrangements.")

I don't recall Pauli saying something like in your first version, only your second, but he might well have: I'm a horrible thief at times. Here's the way I put something similar in my anti-Växjö pseudo-paper `quant-ph/0204146`:

> In choosing one experiment over another, I choose one context over another. The experiment elicits the world to do something. To say that the world is indeterministic means simply that I cannot predict with certainty what it will do in response to my action. Instead, I say what I can in the form of a probability assignment. My probability assignment comes about from the information available to me (how the system reacted in other contexts, etc., etc.). Similarly for you, even though your information may not be the same as mine. The OBJECTIVE content of the probability assignment comes from the fact that *no one* can make *tighter* predictions for the outcomes of experiments than specified by the quantum mechanical laws. Or to say it still another way, it is the very existence of transformation *rules* from one context to another that expresses an objective content for the theory. Those rules apply to me as well as to you, even though our probability assignments *within* each context may be completely different (because they are subjective). But, if one of us follows the proper transformation rules—the quantum rules—for going to one context from another, while the other of us does not, then one of us will be able to take advantage of the other in a gambling match. The one of us that ignores the structure of the world will be bitten by it!

That is, part of the rational part of quantum mechanics is much like de Finettian or F. P. Ramseyan "coherence": If you gamble this way about this, and you gamble that way about that, etc., etc., then you'd better gamble such way about the other, or you're not being coherent with respect to your beliefs of the properties of the world.

02-01-06 *Final Installment* (to W. G. Demopoulos)

Now let me tackle this point of yours

Demopoulosism 4: *I also don't see why we should need something as fundamental as KS to sustain the notion that "unperformed measurements don't have outcomes." I'm being a devil's advocate here because I think what you really mean is that without a measurement of whether the cat is alive, the cat is neither alive nor not alive. But would you put it this baldly? If not, why not?*

in a more longwinded way. For that, I'll paste in an anthology of emails I've had recently with David Mermin and Bas van Fraassen. I think they refer to your point explicitly, but as always I'm still groping to try to get the right language. All the emails are connected, and you can read them linearly from top to bottom.

To answer your question in the best way I know how at the moment, I would say: The transformation that quantum mechanics speaks about, the transformation from a 'superposition' to 'aliveness' or 'deadness', is a transformation *within the agent*, and that transformation cannot take place without some interaction with the external physical system labelled by the word 'cat'. What happens to 'cat' itself (described in a way that makes no reference to the agent)? On that, I think quantum mechanics is silent. With a mantra: Quantum mechanics is a theory for ascribing (and intertwining) personal probabilities for the personal consequences of one's personal interactions with the external world.

Does all this (and particularly the stuff below) go some way toward answering your question?

02-01-06 *The Oblique Pauli* (to H. C. von Baeyer)

Look at this little gem I discovered today. I stumbled across it in the Schilpp volume on Einstein as I was researching for the talk I have to give in Utah Thursday: The topic I got roped into is whether I think Bohr gave an adequate reply to EPR. (My talk's title is, "Why I Never Understood Bohr's Reply to EPR, But Still Liked It.") The quote comes from page 683:

> It may appear as if all such considerations were just superfluous learned hairsplitting, which have nothing to do with physics proper. However, it depends precisely upon such considerations in which direction one believes one must look for the future conceptual basis of physics.
>
> I close these expositions, which have grown rather lengthy, concerning the interpretation of quantum theory with the reproduction of a brief conversation which I had with an important theoretical physicist. He: "I am inclined to believe in telepathy." I: "This has probably more to do with physics than with psychology." He: "Yes." —

Who else could that "important theoretical physicist" be but Pauli! They were certainly discussing these sorts of things at length at that time. Einstein wrote his remarks in 1949 (I think), while Pauli had been visiting Einstein in 1948 (recall how Pauli adjudicated the quarrel between Born and Einstein on quantum foundations during that time).

Also compare the similarity between what Einstein says above and these words of Pauli to Fierz, 10 August 1954:

> All of this then led me onto further, somewhat more phantastic [*sic*] paths of thought. It might very well be that we do not treat matter, for example viewed in the sense of *life*, "properly" if we observe it as we do in quantum mechanics, *specifically when doing so in complete ignorance of the inner state of the "observer."*

> It appears to me to be the case that the "after-effects" of observation which were ignored would *still* enter into the picture (as atomic bombs, general anxiety, "the Oppenheimer case" e.g. etc.), but in an *unwanted form.* The well-known "incompleteness" of quantum mechanics (Einstein) is certainly an existent fact somehow-somewhere, but certainly cannot be removed by reverting to classical field physics (that is only a "neurotic misunderstanding" of Einstein), it has much more to do with *integral relationships between "inside" and "outside" which the natural science of today does not contain* (but which alchemy had suspected and which can also be detected in the symbolics of my dreams, about which I believe them specifically to be characteristic for a contemporary physicist).
>
> With these vague courses of thought I have reached the border of that which is recognizable today, and I have even approached "magic." (From this standpoint observation in quantum mechanics might even appear to someone as a "black mass" after which the "ill-treated" matter manipulates its counter-effect against the "observer," thereby "taking its revenge," as a "shot being released from behind"). On this point I realize well that this amounts to the threatening danger of a regression into the most primitive superstition, that this would be much worse than Einstein's regressive remaining tied to classical field physics and that everything is a matter of holding onto the positive results and values of the *ratio.*

So, I think it's just got to be Pauli that Einstein is referring to!

On another matter, let me come back to something you wrote in your last letter:

von Baeyerism 7: *I had decided to start translating Fierz letters, but will start, instead, with the list you sent – ie with the selection criterion "detached observer" instead of "Fierz."*

I apologize for causing trouble. And the more even I think about it, a well defined theme for you is probably called for. How else would you be able to turn your work into a book? I fear a little, however, that "detached observer" may be too narrow, as I think Pauli and Fierz must have discussed all kinds of "mystical" things tangentially related to that topic, from the possibility of physical/psychical neutral language to synchronicity to archetypes, etc., etc. And I think all that stuff is worthwhile to get into the public eye. On the other hand, as my getting carried away has already demonstrated, some of what he wrote to von Franz and others is probably quite interesting too. So, where does that leave you? I hope not an infinite task! Maybe outside of little side ventures of gathering a little background material here and there, maybe indeed it is better to stick with your original plan of tracking the Pauli-Fierz conversation.

Tomorrow I leave for Snowbird and may be out of email contact for a little while.

Happy New Year again!

10-01-06 *Subject and Object* (to D. M. Appleby)

Thanks for the note, which I very much enjoyed reading tonight (while listening to Abbey Lincoln and Hank Jones in the background). It did my soul good, and I'll certainly be reading it again.

Let me, however, give you a quick first reaction to this:

Applebyism 7: *I know we agree that physics isn't any kind of mirror. Well: it seems to me that if you start trying to identify some parts of quantum mechanics as "objective" and other parts as "subjective" then you are going back on that.*

In my talk at Konstanz I mentioned the idea of Galileo and Descartes that properties like "redness" don't faithfully depict properties actually in the object, whereas properties like "cuboidal" do. They thought, in other words, that the property of "redness" is subjective whereas the property of being "cuboidal" is objective. And they thought that if one eliminates all the subjective features of the visual field, one will be left with an accurate reflection of things as they are in reality: a mirror of reality. Quite possibly I have misunderstood you. If I have please correct me. But when you talk about identifying the parts of quantum mechanics having objective content it sounds, to my ears, as though you are thinking in a similar way to Galileo and Descartes. Purifying the reflection. Polishing the mirror.

Might I ask you to go back to Sections 4 and 5 (but particularly Section 5) of my anti-Växjö paper `quant-ph/0204146` and tell me whether you think the words in there seem to alleviate any of my sins? I had been planning to use the "It's a Wonderful Life" allusion to open up my *Scientific American* article, so I'd like to know what you think of it in this context. (If you don't know the movie, find a way to rent it and give it a watch.)

Honestly, I'm getting confused on these issues even myself of late. I have been meaning to read Donald Davidson's essay titled "The Myth of the Subjective" and see if that helped me any, but I haven't had the gumption yet.

You're definitely making me think about these things.

11-01-06 *For the Record* (to N. D. Mermin)

Well, I'm back at home, safely again at Bell Labs, after having spent some time at a meeting in Snowbird, Utah. I am afraid I angered our friend, Prof. Plotnitsky, with my talk. My original title for the talk had been "Why I Never Understood Bohr's Reply to EPR, But Still Liked It"—but I wrote it on two overlapping transparencies so that, at the appropriate moment, I could strip off the part that said "But Still Liked It." (I hadn't originally intended to do that, but it was the only thing I could do with honesty after rereading Bohr.) And so the talk went. I explained how I didn't see much in Bohr's reply that EPR hadn't anticipated in their second- or third-to-last sentence, "... one would not arrive at our conclusion if one insisted that two or more physical quantities can be regarded as simultaneous elements of reality only when they can be simultaneously measured or predicted." (Which is a conception they pretty much simply dismiss.) Then I showed how the EPR criterion of reality can nevertheless be made to implode through a combination of perfect predictability (through entanglement) and KS noncolorable sets. Then I read some long passages from Einstein's autobiographical notes in the Schilpp volume, and claimed that his own logic was flawless: The conclusion being that a quantum state cannot correspond to a "real factual state of affairs"—when Einstein was right, he was really right! Finally I asked, so what is the uncertainty given by a quantum state about? And concluded with a picture that was meant to capture much of what I put in the emails to you a couple of weeks ago.

There's just not a lot you can do by considering only *two* noncommuting variables, and as far as I can tell, that's all Bohr ever really did.

But that's not why I'm writing you—i.e., to make my own record—but rather to get you to come down on the record. I ask because I don't know how many meetings in the last year or two where I've heard people praise the Ithaca interpretation or "correlation without correlata"—this meeting in Snowbird was one of them (Ivan Deutsch being the most recent admirer). And in all cases, I've said, "I don't think Mermin subscribes to those ideas anymore, or at least not fully." But when asked what your problems are with your earlier ideas, I don't know that I've had adequate answers of your own point of view. So, could I ask you again to what extent you now disavow the

II and exactly why? I'd like to get it on the record so I don't screw up when trying to represent you.

On another issue, have you ever looked at the section in Max Jammer's book *The Philosophy of Quantum Mechanics* on "relational conceptions of the quantum state"? It seems like there's probably a lot of material in there that would interest you. I myself don't recall ever having noticed that section before last week (though surely I read it before, as I read the whole book cover to cover in the summer of 1984 or 1985), and I found it quite interesting. I also found his section on "latency theories" at the end, where he reports Margenau's view, very useful: In particular, I've started to wonder if, DISREGARDING the part where Margenau thinks of the quantum state as objective, the rest of his view might correspond quite nicely with where Schack and I stand at the moment. Margenau, apparently, views quantum measurement outcomes as secondary qualities in Locke's hierarchy, not primary ones, much like the idea of blueness (which I think you call qualia). I found myself wondering if, in the end, my own view might not just boil down to that. (This, of course, not unconnected to some points you were trying to make in your "What Is QM Trying to Tell Us?" paper.)

See, I can write a paper on certainty, but I never can be certain myself!

Mermin's Reply

I wish I ran into people who liked the IIQM. Maybe I should go to more meetings. I probably should write some sort of update before the 10th anniversary of the AJP article, but from my perspective it seems vaguely auto-erotic, since I haven't detected anything like the amount of interest in the paper that you describe.

It seems to me that I touched on much of what bothers me (and bothered me even then) in the final section of the AJP paper, reproduced below for your convenience:

> XII. A FEW FINAL REMARKS
>
> At the risk of losing the interest of those who — like myself — read only the first and last sections before deciding whether the rest is worth perusing, I conclude with some brief comments about loose ends.
>
> As noted at the beginning, what I have been describing is more an attitude toward quantum mechanics than a systematic interpretation. The only proper subject of physics is how some parts of the world relate to other parts. Correlations constitute its entire content. The actual specific values of the correlated quantities in the actual specific world we know, are beyond the powers of physics to articulate. The answer to the question "What has physical reality?" depends on the nature of "what." The answer is "Everything!" if one is asking about correlations among subsystems, but "Nothing!" if one is asking about particular values for the subsystem correlata.
>
> This alters the terms of the traditional debates. Traditionally people have been asking what correlata have physical reality. The many different schools of thought differ by answering with many different versions of "Some" while the IIQM answers "None!" The question of what correlations have physical reality, which the IIQM answers with "All!" has not, to my knowledge, been asked in this context. While I maintain that abandoning the ability of physics to speak of correlata is a small price to pay for the recognition that it can speak simultaneously and consistently of all possible correlations, there

remains the question of how to tie this wonderful structure of relationships down to anything particular, if physics admits of nothing particular.

At this stage I am not prepared to offer an answer, beyond noting that this formulates the conceptual problem posed by quantum mechanics in a somewhat different way, and suggesting that there may be something to be learned by thinking about it along these lines. I suspect our unfathomable conscious perceptions will have to enter the picture, as a way of updating the correlations. To acknowledge this is not to acknowledge that "consciousness collapses the wavepacket." But it is to admit that quantum mechanics does not describe a world of eternally developing correlation, described by "the wave-function of the universe"!, but a phenomenology for investigating what kinds of correlations can coexist with each other, and for updating current correlations and extrapolating them into the future. This phenomenology applies to any system that can be well approximated as completely isolated.

A skeptic might object that the problem of how to update correlations is nothing more than the measurement problem, under a new name. Perhaps it is, but at least the problem is posed in a new context: How are we to understand the interplay between correlation as the only objective feature of physical reality and the absolute particularity of conscious reality? Is something missing from a description of nature whose purpose is not to disclose the real essence of the phenomena but only to track down relations between the manifold aspects of our experience? Is this a shortcoming of our description of nature or is it a deep problem about the nature of our experience?

... By acknowledging that in our description of nature the purpose is not to disclose the real essence of the phenomena, we free ourselves to construct from the manifold aspects of our experience formal representations of the systems we want to talk about. We have learned how to express their possible correlations by an appropriate state space, and the evolution of those correlations by an appropriate Hamiltonian. By setting aside "the real essence of the phenomena" we also acquire the ability to replace the befuddling spectre of an endlessly branching state of the universe–as disturbing in the self-styled down-to-earth Bohmian interpretation as it is in the wildest extravagances of the many worlds interpretation–with a quantum mechanics that simply tells us how we can expect some of the manifold aspects of our experience to be correlated with others. While this may sound anthropocentric, it is my expectation that anthropos can be kept out of everything but the initial and final conditions, and often—but not always —even out of those. But this remains to be explored.

What can I add to that, 8 years later?

1. You persuaded me quite soon that "objective probability" was problematic. Until I met you I had never taken the notion of subjective probability seriously, or even know very much about it. While I'm still not convinced (sorry) that you've got it right either, I'm much more aware that one of the pillars of the IIQM is much more fragile than I thought.

2. Not unrelated to 1, the notion of "correlation" is not well defined, beyond my assertion that it means nothing more than "joint distribution". But what does it mean to say that joint distributions are fundamental, while conditional distributions, which

can be constructed from joints, have no physical meaning? And what are these joint distributions describing? How are they tied down to experience? Which brings us to

3. Consciousness. Although I say that the problem of consciousness should be set aside, when I went around giving physics colloquia on the IIQM in the late 90's, during questions everything kept coming back to conscious perceptions. It ended up being too big a rug to sweep problems under. This is a point in your favor. If probability is subjective, then there is a subject (with conscious perceptions) built in at the beginning, and consciousness becomes the starting point, rather than a completely mysterious add-on.

4. Quantum computation (which I only started studying after writing the paper) made me much more sympathetic to Copenhagen and a purely instrumentalist (positivist?) view of the subject than I had ever been. (See "How I stopped worrying and learned to love Bohr.")

Doubtless there's more, and perhaps I should think about writing something more careful and considered, but since my views have yet to settle down, that still seems premature.

So if you want something to tell these people who you claim exist, I think its too strong to say that I disavow the IIQM. But I do regard it as at best incomplete (as I think I made clear in the 1998 paper). I guess I was hoping somebody would take up the cause and complete it.

12-01-06 *It's All Your Fault* (to H. C. von Baeyer)

von Baeyerism 8: *Back from the Bahamas, I find three books from Princeton U. P. in payment for a proposal I reviewed for them.* [One I requested] *on the strength of your blurb: Omnes's Converging Realities.*

Just a quick question: Should I really read it, or were you just being kind to him (them)?

I'm not tellin'. Your interests and taste will decide the first part of your question within the first couple of chapters.

As for Omnes' ability to stir the soul, here's an example of Omnes at his best:

> Perhaps the best way to see what it is all about is to consider what would happen if a theory were able to offer a detailed mechanism for actualization. This is, after all, what the advocates of hidden variables are asking for. It would mean that everything is deeply determined. The evolution of the universe would be nothing but a long reading of its initial state. Moreover, nothing would distinguish reality from theory, the latter being an exact copy of the former. More properly, nothing would distinguish reality from logos, the time-changing from the timeless. Time itself would be an illusion, just a convenient ordering index in the theory. ... Physics is not a complete explanation of reality, which would be its insane reduction to pure unchanging mathematics. It is a *representation* of reality that does not cross the threshold of actuality. ... It is wonderful how quantum mechanics succeeds in giving such a precise and, as of now, such an encompassing description of reality, while avoiding the risk of an overdeterministic insanity. It does it because it is probabilistic in an essential way. This is not an accident, nor a blemish to be cured, since probability was found to be an intrinsic building block of logic long before reappearing as an expression of ignorance, as empirical probabilities. Moreover, and this is peculiar to quantum mechanics, theory ceases to be identical with

reality at their ultimate encounter, precisely when potentiality becomes actuality. This is why one may legitimately consider that the inability of quantum mechanics to account for actuality is not a problem nor a flaw, but the best mark of its unprecedented success.

16-01-06 *Little Phrases* (to D. M. Appleby)

A subject and an object, from a physicalist perspective, are simply two objects. Suppose the subject has a belief about something to do with an object (even if only a belief about how some interaction with the object will lead to a certain sensation in the subject). To the extent that the belief is a possession of the subject, and the object need not abide by it, the belief is "subjective." I.e., it is a statement of a property of the subject. To the extent that the belief encodes a statement of the subject's history and composition (perhaps it's genetic makeup, the culture in which it was raised, the accidental things that happened to it throughout its life, how much alcohol it had to drink just before the belief, etc.), it is "objective." For, it is a settled and inalienable possession of the actual historical record of that small piece of the world.

Does any of this wordplay have anything to do with settling the issues you wrote me about last week?

17-01-06 *Diptera* (to D. M. Appleby)

I'm not sure what I did to do it, but it looks like I really angered you. All I guess is that you took very seriously my one (flippant?) use of the work "physicalist," which probably has a lot of connotations for you. I'm not completely sure what it means (other than the Webster definition), but I was trying to search for a word that conveyed the idea that there is no true divide between subject and object in a Kantian or Schopenhauerian sense. What do you call that?

Did you read the Davidson essay "The Myth of the Subjective"? What would you call that kind of idea? Maybe not physicalism, but then what?

You write, "You are probably thinking that the physicalist perspective you ask me to adopt . . . " But I didn't ask you to adopt anything. I ASKED, "Does any of this wordplay have anything to do with settling the issues you wrote me about last week?" It was my meager attempt to see if that was what was at issue; it was a (lame?) attempt to clarify things to my mind.

18-01-06 *What I Must Have Meant* (to D. M. Appleby)

I searched my memory on and off though my sleeping/sleepless hours last night to see if I could find a clue to why I had used the word "physicalist" in my note to you.

Well, I found it: It came to me through Rorty. "Nonreductive Physicalism" is what he calls his view and what he calls Davidson's in the "The Myth of the Subjective" and other places. So, I finally read Davidson's essay, and I RE-read Rorty's essay "Non-reductive Physicalism" in his book *Objectivity, Relativism, and Truth*. And I think that view approximates to some extent the direction I'd like to go.

Did you ever read Rorty's introduction to that volume, like I had once suggested to you? (I'll place my old note suggesting that below.) He states quite clearly there what he means by antirepresentationalism—I think it is a view that approximates our own two views (i.e., yours and mine). Yet, later in the book Rorty has this essay "Non-reductive Physicalism," and he certainly doesn't see it at cross-purposes with his introduction. So my question is, what do you think of it? Is it a meaning for the word "physicalism" that you could accept?

27-01-06 *Degrees of Freedom / Distinguishable States* (to H. Halvorson)

Also, I spent a little time this morning trying to search my archive of mumbles to see if I could find something that decently expressed my distaste for taking "the number of degrees of freedom" or "the maximum number of distinguishable states" as the foundation for Hilbert-space dimension. Unfortunately, I wasn't as successful as I had hoped I would be: Maybe it means my memory is failing, and I haven't really yet written a clear statement of what I'm thinking.

So, let me give you the pointers that I could find in the meantime:

- "On the Quantumness of a Hilbert Space," `quant-ph/0404122`
- pages 159–160 of *Quantum States: W.H.A.T.?*
  `http://netlib.bell-labs.com/who/cafuchs/PhaseTransition.pdf`

I want Hilbert-space dimension to denote not (a priori at least) number of distinguishable states, but rather something to do with a quantum system's sensitivity to the touch or its creative power. And that's partially what I am trying to get at with ideas like "quantumness".

01-02-06 *Measurement Based Quantum Computation* (to H. Halvorson)

While I'm thinking of it (because of the grant proposal I happen to be working on), let me give you references to my three favorite papers on measurement-based quantum computation:

- Measurement-based quantum computation with cluster states
  Authors: R. Raussendorf, D.E. Browne, H.J. Briegel
  `http://www.arxiv.org/abs/quant-ph/0301052`
- Cluster-state quantum computation
  Author: Michael A. Nielsen
  `http://www.arxiv.org/abs/quant-ph/0504097`
- An introduction to measurement based quantum computation
  Author: Richard Jozsa
  `http://www.arxiv.org/abs/quant-ph/0508124`

That computational model strikes me as a veritable goldmine for exploring how the notion of "measurement" I've been talking to you about (i.e., action on a system, followed by unpredictable reaction in the agent) is given *meaning* in the workaday sense. Maybe a way to put it is: Quantum measurements don't "inform," rather they "enable." And I think the Raussendorf-Briegel computational model starts to give that slogan some precision.

16-02-06 *Synchronicity* (to H. C. von Baeyer)

von Baeyerism 9 : *Cheers from sunny Williamsburg, where the total number of snowflakes during the recent storm was 17.*

Interesting number, since it turns out that throughout my yard I fairly evenly measured 17 inches.

First Fierz and Pauli, and now the two of us!

16-02-06 *Many Worlds Does Not Explain Much* (to B. C. van Fraassen)

Thanks for sending your notes. I enjoyed reading them (twice actually), and I think I get your point. I don't know that I have much to add at the moment by way of endorsement or, alternatively, criticism, but your ideas are tumbling around in my head—maybe something will eventually emerge.

Let me only point out a couple of connections that you evoked in me. First, I liked the way you put this:

> In retort it will perhaps be suspected that I yearn for a classical understanding of the world, if I'm not willing to count the 'many worlds' answer as explanatory. Quite the contrary, I would say: as I see it, quantum theory does leave unexplained why the actual outcome of a measurement is this rather than that one among the possible outcomes. Yearning for a classical understanding means yearning for an explanation of all that this theory leaves - and legitimately leaves - unexplained.

Its tone reminded me mildly of something David Mermin said in his lecture notes for his quantum computing course:

> There are nevertheless some who believe that all the amplitudes α_x have acquired the status of objective physical quantities, inaccessible though those quantities may be. Such people then wonder how that vast number of high-precision calculations (10^{30} different amplitudes if you have 100 Qbits) could all have been physically implemented. Those who ask such questions like to provide sensational but fundamentally silly answers involving vast numbers of parallel universes, invoking a point of view known as the many worlds interpretation of quantum mechanics. My own opinion is that, imaginative as this vision may appear, it is symptomatic of a lack of a much more subtle kind of imagination, which can grasp the exquisite distinction between quantum states and objective physical properties that quantum physics has forced upon us.

[Actually the source of this quote is `quant-ph/0207118`.]

The other thing you evoked in me was a memory of an article by Markus Fierz that I reprinted in my *Notes on a Paulian Idea* with the title: "Does a physical theory comprehend an 'objective, real, single process'?" I read it again, between my two readings of your note. I think you too will enjoy the piece, and particularly near the end of it, see some similarity to what you have written in your paragraph above. Tell me if I am on the money? For your convenience, I'll paste in the whole article below; I hope the remnants of LaTeX code in it won't bother you too much.

Finally let me remark on these words of yours:

> A beautiful program for the interpretation of quantum mechanics has lately received new life as new results, new techniques, and other approaches have been mined to aid in its elaboration. The excitement in such work is its own reward.

My own feeling is that all their purported progress is illusory. It is a lot of technical-looking huffing and puffing (which builds a shield of seeming protection), but in the end doesn't amount to much. At a crucial point they make the identification "weight = probability" for no other reason than that they know that's what they have to do to get the answer they want. Here's the way my friend Howard Barnum put it recently:

> The basic idea is that the Wallace argument is just some version of a Laplace's symmetry-principle argument in another guise ... and that I think all these arguments have things backwards: our belief that there is a "physical symmetry" just about IS the invariance of our preferences under the relevant transformation ...

Basically, I say beware of giving the New Everettians too much credit!

17-02-06 *Incompletely Knowable vs 'Truth in the Making'* (to W. G. Demopoulos)

Now I have to apologize to you again for a long silence: Soon after the new year my stepfather became very ill, finally passing away a couple of weeks ago. It has been very tough on the family, and I am only now catching myself back up.

However, don't think that throughout all that, your ideas have not been on my mind. Indeed I enjoyed another re-reading of your 'incompletely knowable domain' paper—it lifted me on my sad flight back from Texas—and I tried to think hard about whether there really is a substantial distinction between us or not.

Let me try to consider a situation and 1) try to imagine what you would say of it (but probably in my idiosyncratic language), followed by 2) what I think I would say of it ... and then see if there is a substantial distinction.

Start with a finite dimensional Hilbert space, say of dimension 3, and imagine it indicative of some real physical system within an observer's concern. From that Hilbert space, let us form all possible sets of three mutually orthogonal one-dimensional projection operators. That is, let us consider all possible sets of the form $\{P_1, P_2, P_3\}$.

What is it that you would say of those sets? If I understand you correctly, it is this. Each such set $\{P_1, P_2, P_3\}$ corresponds to a set of mutually exclusive properties that the system can possess. At any given time, one of those projectors will have a truth value 1 and the other two will have values 0. Now consider a potentially different such set $\{Q_1, Q_2, Q_3\}$; again, at any given time, one of those projectors will have a truth value 1 and the other two will values 0. What is interesting in your conception, if I understand it, is that even if two elements happen to be identified between those two sets—for instance, if $P_1 = Q_3$—there is *no requirement* that P_1 and Q_3 need have the same truth value; P_1 might have the truth value 0, whereas Q_3 might have the truth value 1. Another way to say this is that the truth-value assignments depend upon the whole set and not simply the individual projection operators. For you, all the identification $P_1 = Q_3$ amounts to is that the *probability* for the truth value of P_1 within the set $\{P_1, P_2, P_3\}$ is the *same* as the *probability* for the truth value of Q_3 within the set $\{Q_1, Q_2, Q_3\}$. (If you were a Bayesian about probabilities—though I don't think you are—you would say, "Well P_1 has whatever truth value it does, and Q_3 has whatever truth value it does (each within their appropriate set of mutually exclusive triples), but my degree of belief about the truth value of P_1 is the same as my degree of belief about the truth value of Q_3. That is the rule I am going to live by.") Then it follows from Gleason's theorem that there exist no probability assignments for the complete (i.e., continuously infinite) set of triples that is not of the quantum mechanical form. In particular, one can never sharpen one's knowledge to a delta function assignment for *each* triple. This is how you cash out the idea of an 'incompletely knowable domain.'

That is a novel idea, and if I understand it correctly, I like it.

However, now let me contrast my characterization of you with what I think has been my working conception. I prefer not to think of the triples $\{P_1, P_2, P_3\}$ as sets of mutually exclusive *properties*

inherent within the system all by itself, but rather *actions* that can be taken upon the system by an external *agent*. Each *set* of such projectors corresponds to a distinct action; what the individual elements within each set represent are the (generally unpredictable) *consequences* of that action. What are the consequences in operational terms? Distinct sensations within the agent. The reason I insist on calling them consequences, rather than "sensations" full stop, is because I want to make it clear that the domain of what we are talking about is sensations that come about through the action of an agent *upon* the external world.

The essential idea of the sexual interpretation of quantum mechanics is that no element of a set $\{P_1, P_2, P_3\}$ has a truth value before the action of the agent. Rather the truth value—if you want to call it that (maybe it is not the best terminology)—is generated (or given birth to) in the process. At the point, one of the P_i stands in autonomous existence (within the agent), whereas the other two fall.

I hope I have characterized both of us accurately!

Here is the question that has been troubling me. Is there any real distinction (one that makes an pragmatic difference) between our views? You say the truth value is there and revealed by the measurement, and I say it's made by the measurement and wasn't there beforehand. So what?

If there is a pragmatic distinction, Steven van Enk and I through discussions this week have come to believe that it may show up most clearly in how you and I would treat counterfactuals with regard to measurement. Let us take a situation where an agent ascribes a quantum state ρ to the system; contemplating the measurement $\{P_1, P_2, P_3\}$, we know that he will ascribe probabilities according to the Born rule $\mathrm{tr}(\rho P_i)$ for the various outcomes. Suppose he now performs that measurement and actually gets value P_2.

What does getting that outcome teach him about the quantum system? I think you would say it reveals which of the three mutually exclusive properties the system actually had. On the other hand, I would say it teaches him nothing about the system per se; the outcome P_2 is just the consequence of his action. What is the implication of this on counterfactuals? Here's at least one.

Suppose after you get your outcome, you contemplate magically having performed a distinct measurement $\{Q_1, Q_2, Q_3\}$ instead. I think you're careful to point out in your paper that the knowledge of P_2 carries no implication for what you would have found with this other imaginary measurement. But what happens if you conceptually transform this measurement $\{Q_1, Q_2, Q_3\}$ to one closer and closer to the original, i.e., to $\{P_1, P_2, P_3\}$? In the *limit* when the two are identical again, I think you would say that knowledge of the outcome P_2 in the original case implies that P_2 will also be the outcome in the limiting counterfactual case. But what would I say? From my conception, there is no reason at all to believe that the limiting counterfactual case will give rise to the same outcome P_2. The best one can do, either in the original case or the counterfactual case, is to say that an outcome i will arise with probability $\mathrm{tr}(\rho P_i)$. In fact, a counterfactual analysis with this kind of result may be the very meaning of the idea that quantum measurements are generative of their outcomes.

At least that is a potential distinction Steven and I see at the moment. We are toying with the idea that this may have some implications on the analysis of Hardy-type paradoxes, etc., where counterfactuals abound, and if anything comes of that, we'll let you know. In the meantime, would you say that we have given you a fair characterization?

18-02-06 *Two Questions* (to W. G. Demopoulos)

Here are two questions that came up in the discussions with Steven. I probably won't have a chance to think about your answers too deeply until the week after next (I've got to give the

Applied Maths Colloquium at Princeton and then run the Wheelerfest there Thursday and Friday ... and I've got a million things to do for both). But let me throw the questions on the table anyway.

1) Almost by definition, what you are proposing is a contextual hidden variable theory. But what is its status with regard to locality? At different times (while driving, taking a shower, etc.), I've been able to convince myself that a constraint of *locality* can be placed upon the truth values, but then I get confused. What can you say on the matter?

2) Take two triads of one dimensional projectors, $\{P_1, P_2, P_3\}$ and $\{Q_1, Q_2, Q_3\}$, as in my last note to you. And as before, suppose $P_1 = Q_3$. However this time, let us be careful to assume that P_1 and Q_3 differ in truth value in their respective sets. What happens now when we consider nonelementary propositions of the form $\{P_1, \neg P_1\}$ and $\{\neg Q_3, Q_3\}$ where by $\neg P_1$ I mean the orthocomplement of P_1, etc. Presumably you still want to view these sets as representative of mutually exclusive properties inherent within the quantum system. However, by construction the sets are identical: $\{P_1, \neg P_1\} = \{\neg Q_3, Q_3\}$. How does one decide on a truth value assignment here, given the previous truth value assignments for $\{P_1, P_2, P_3\}$ and $\{Q_1, Q_2, Q_3\}$?

06-03-06 *Be Forgiving* (to G. L. Comer)

Comerism 10: *I'm very much convinced that the historical approach to teaching special relativity is very much a disservice to the subject. It gives precisely the wrong foundational point-of-view.*

I think you ought to be a little forgiving to something like a historical process for thinking about the foundation of the theory—or, at least admit that it may have its place. It may not be the best pedagogy for students, but it may have its place elsewhere.

What I'm thinking of here particularly is how one might incorporate quantum mechanical ideas into the scene. If one starts the analysis off—crucial words, "starts the analysis off"—with spacetime before rods and clocks (or clocks and maybe some stand-in for light rays), then the only natural progression one can take may end up being to try to "quantize gravity" in the usual sorts of ways (for instance, wave functions over three-geometries and variations of that program).

On the other hand, a more operationalistic approach—one that doesn't assume spacetimes or wave functions of spacetimes at the outset—may be just what is needed to free up the conceptual playground enough to make good progress. For instance, if a quantum measurement event helps set the very notion of causal structure in the first place, then maybe it is best to rethink where along the lines of earlier thinking one could make the conceptual leap to "metric is fundamental." In the quantum world, maybe one cannot make that conceptual leap—and thus maybe that is not a good starting point.

On a different subject:

Comerism 11: *I mean not discussing the metric as fundamental is like presenting quantum mechanics without mentioning the wave function.*

You see, but that is what I would ultimately like to do! For, for me, wave functions are always about *someone's* degrees of belief. And what we (quantum Bayesians) would like to get at is a gambling-agent-independent account of what is up in the quantum world. As long as one invokes a wave function, one is always *explicitly* talking about an observer. See transparency #12 in my talk "Being Bayesian in a Quantum World" posted at the bottom of my webpage. It talks to that point directly!

08-03-06 *A Little Wheelerfest Report* (to H. C. von Baeyer)

von Baeyerism 10: *Did you learn anything, or teach anything, at the Wheelerfest?*

Indeed I got a lot from the meeting. And had many ridiculous fights with John Conway, who is afraid of the Bayesian idea of probability. Such a great mind; such a great prejudice!

Particularly useful for me in preparing my own talk was the *strong* realization that Born rule is not at all about setting probabilities but transforming them! It's role really is that of a transformation rule. There are hints of that in my writings before, but finally I feel it with all my heart and soul—that's what I meant by the "strong realization." Anyway it indicates that maybe one can get at the content of the Born rule through some Dutch-book-like arguments. Thus I posed that as a question at the end of my talk. Very nicely, Schumacher took the challenge and has started to make progress in that direction.

I sort of like the talk I put together for this—even though it goes out on the limb more than usual (even for me). I will try to scan in a copy soon (and send to you), and I will probably give an expanded variation on it in Sweden.

10-03-06 *Isms* (to J. E. Sipe)

Sipe-ism 1: *I'll be checking email until about 5PM today (Friday); I fly to Baltimore tonight, and hope to be checking email there, but I'm not sure that'll be possible until the conference starts, and my tutorial is Sunday morning!*

Ouch, I've run out of time for all those delicate and carefully constructed answers I wanted to give you! My fault—you deserve much better than me. It's now almost 3PM, and I'm just finished talking to Hans. Now I'm in a Panera's Bread in Princeton, having a cup of coffee before my drive home. Let me send you a very little note answering some of your questions, and maybe we can talk more next week.

I certainly liked the way you posed your questions.

Sipe-ism 2: *I gather from this that you see the ultimate task to be the identification of just what is the part of quantum mechanics that is "about the gambler-independent world." The Bayesian analysis is a strategy, as I understand it, to identify that task and undertake it. It is a means, not an end.*

But if I understand this correctly I am puzzled to see statements like: . . . [blah blah blah measurements create outcomes]

Fair enough. But might it not be that the "gambler-independent world" is in fact described by a hidden-variable theory of some sort? For example, why would you (immediately!) rule out a Bohm - de Broglie universe (or a modified one, with the usual distribution over configuration space being a kind of 'equilibrium distribution' that we might someday be able to overcome, such as Valentini suggests) simply because of your Bayesian strategy in trying to identify just what that "gambler-independent world" is? Could it not be that Bohm and de Broglie have really identified the "fact of the matter in the universe," or that some such approach would? At least, must not this possibility be seriously considered? How does a Bayesian strategy immediately rule it out? By rejecting even the possibility that some quantum measurement outcomes may pre-exist the measurement process, do you not prejudge the outcome of the larger quest, for which the Bayesian viewpoint is not a goal but a strategy?

Here is the way I once answered Jeff Bub when he asked a similar question. Jeff asked:

> You want to take quantum mechanics as a theory of the way information is represented and the limitations on the communication of information, and not a description of the behavior of particles, as in classical mechanics. Granted, the way the world is hard-wired might impose limitations on the gathering of information and the exchange of information – limitations expressed precisely by the 'limited sort of privacy' we have (i.e., secure kd, but no secure bc), hence by science necessarily taking the form of quantum mechanics. But how does it follow from this that we must interpret quantum mechanics as a theory of information, and not as a descriptive theory in the sense of classical mechanics?

My reply was:

> It doesn't. You're completely on track there. Do you remember my slide where I listed the axioms of quantum mechanics in Montreal? In my presentation I said how I'm always struck by the stark contrast between that list of axioms and the ones we take for our other cornerstone theory of the world (referring to special relativity): (1) the speed of light is constant, and (2) physics is the same in all frames. The debate over the foundations of quantum mechanics will not end until we can reduce the theory to such a set of crisp physical statements—I believe that with all my heart. However, just as special relativity will always be interpretable in Lorentz's way, quantum mechanics will likely always be interpretable in Bohm's way. There's nothing we can do about that.
>
> What I'm really searching for is just a polite way to say, "Ahh, blow it out your butt. You can believe that Lorentzian way of looking at things if you want to, but why when have this absolutely simple alternative conceptual structure?"
>
> What I'm looking for is just a couple of crisp physical statements—contradictory appearing even, just as Einstein's—that can characterize what quantum mechanics is all about. Something like (but more precise than):
>
> 1. The effects of our interventions into the world are nondiminishable. And,
> 2. But still we have science; all the world is not simply a dream of our own concoction.
>
> Once we get that cleared up, we'll finally be ready to move to the next stage of physics, much like Einstein was ready to move on to general relativity once he had reduced the Lorentz contractions to the two statements above.

That's the best answer I think I can give you at the moment. Even a Bayesian approach to quantum mechanics cannot DISPROVE nonlocal, noncontextual hidden variables theories. It just emphasizes that they don't smell right.

The "proof" of any approach will ultimately be in what new physics it gives rise to.

Now for your most important question:

Sipe-ism 3 : *In any case, (and to make sure in this tutorial I don't confuse somebody about what you really do hold!) could I get you to see if the way I describe your approach below is (a) basically right, (b) partly right but with major flaws, or (c) totally screwed up? ...*

I use the term "interpretation" of a modern physical theory to mean an identification of just what it is to which the abstract elements in the theory purportedly refer. A "type of interpretation" identifies the type of things to which the abstract elements in the theory refer.

The abstract elements in quantum mechanics are density operators, POVMs, etc., etc. A "realist" type of interpretation identifies the abstract elements with stuff that purportedly really exists in

the world, the "furniture of the universe." An "operationalist" type of interpretation identifies the abstract elements with tasks in the laboratory, such as preparations, transformations, and measurements. And so on. An "instrumentalist" type of interpretation says that abstract elements don't refer to anything at all, except perhaps in the way a hammer refers to a nail. That is, theories are just (only) tools for coping in the world. . . .

Now you and your colleagues hold "the idea that quantum STATES are of essence information" and the obvious question a beginning student would ask - and undoubtedly someone will ask if I say something like this in the tutorial – is "information about what?" Perhaps one could argue that this question has no meaning, but I don't see how that view can be sustained.

In any case, from the talks I have heard you give I came away with the impression that, at least in the short term and from the point of view of our current work-a-day physics, you take this to be information, broadly speaking, about the impact and results of future tasks in the laboratory, such as measurements. On the basis of this it seems to me you are in the "operationalist" camp.

Now within this camp there are various tribes. Some feel that any talk about underlying "furniture of the universe" is meaningless, or at least not worthwhile. They are "hard core operationalists." Gadgets and tasks for them are ultimate primitives in their theorizing, and for them an operationalist physics is all there can ever be, at least if it is to be what they would consider a proper physics.

I take you to be in a different tribe, whose members feel that ultimately we want a physics that does indeed talk about the furniture of the universe, the "gambler-independent world." Indeed, you refer to that as the "wheat," as opposed to the "chaff." But that "gambler-independent world" has not yet been identified. In the interim you argue that the abstract elements of quantum mechanics should be understood operationally, and the predictions and statements of quantum mechanics subjected to a kind of "Bayesian rack" to clarify exactly what beyond the objective rules of probability actually exists in them. This analysis then provides a firm footing for the search for the actual "furniture of the universe," whatever it is. Ultimately you want a theory that does describe this furniture; I would guess you are uncertain as to whether quantum mechanics (sufficiently purified by Bayesian analysis) would provide that, or whether a successor theory would be required. But in any case in your heart of hearts you hope someday for a realist science, or at least hope that that hope makes sense.

I think my way of looking at the terms *within* quantum mechanics runs the whole gamut from instrumentalism to realism, and there's no way to put it into one single category.

1. When I posit a physical system about which I will speak (by assigning it a Hilbert space, etc.), I am doing that in an almost naïve realistic way. I.e., the $\mathcal{H}$ I write down, represents a piece of the world that is out there independently of me.

2. When I assign a dimension d to that Hilbert space, I am hypothesizing an inherent *property* of that system. I might be right, or I might be wrong, but d is something I hypothesize of it, even if only provisionally. Realism again.

3. When I draw a quantum state out of the space of operators defined by $\mathcal{H}$, however, I am expressing a bundle of my expectations. These are not properties inherent in the system. They are subjective expectations that I bring into the picture (presumably because they have served me well in the past, or at least done me no harm). If I were to conceptually delete myself from the picture, these expectations would disappear with me. To that extent, the view might sound a little like—or at least be confused with—idealism (but that's only if one—as Howard Wiseman often does—forgets elements 1 and 2 above and one of the further elements below.)

4. The subject matter of those expectations, i.e., what they are about, refers both to me and the system I posit. They are MY expectations for the consequences (for ME) of MY interactions with the system. That, you might think is a kind of operationalism: For if I were to conceptually delete myself from the picture, those interactions would disappear too.

5. On the other hand, the reason we use the formal structure of quantum mechanics to bundle our expectations, to manipulate and update them, to do all that we do with them, is to better cope with the world. It is a means to help our species to survive and propagate. That is a kind of instrumentalism. That part of quantum mechanics is a tool like a hammer; it can be used to fix a lot of things, or simply as an aid to help defend ourselves.

6. Still one can never forget the ultimately uncontrollable nature of each quantum measurement outcome—and through Kochen-Specker, at least the way I view it, the non-pre-existence of those "outcomes". That smacks of realism in the oldest, most time-honored way. The world surprises us and is not a creation of our whims and fancies. Back to realism ... but the twist is quantum mechanics, as used by each individual user, only refers to the outcomes HE helps generate. (That smacks of alchemy ... dangerous to say so, but I call it like it is.)

7. Nevertheless, having learned a little from Copernicus, it seems we should ultimately try to abstract away from these personal encounters with the world (having learned what we could from the formalism concerned with gambling on them). If a tiny little system and I create something new in the world when we get together—i.e., we give rise to a birth or new fact—so must it be likely, it seems to me, that any two things give rise to a birth when they get together. That is "F-theory" or the "sexual interpretation of quantum mechanics" ... but you'll have to wait for the movie if you question me any further on that ...

I hope that answers you a little. It took me a cup and a half of coffee, and I think I just barely made deadline.

Have a safe trip. It'll be fun talking to you next week.

10-03-06 *Isms, 2* (to H. Halvorson)

The other thing I wanted to write you about isms is this. Here is the paper by Huw Price that I was telling you about:

> "Naturalism without Representation"
> `http://www.usyd.edu.au/time/price/preprints/naturalism-final.pdf`

The abstract for the paper is below. I recall liking the paper very much. And to the extent that I understand the term, I believe I can classify myself (presently) as what Huw calls a "subject naturalist."

I wonder how you would characterize a) yourself, and b) your other colleagues in your department, in these terms.

> Abstract: I begin with a distinction between two ways of taking science to be relevant to philosophy. The first ("object naturalism") is a ontological thesis – it holds that what exists, what we should be realists about, is the world as revealed by science. The second ("subject naturalism") is a prescription for philosophy, based on the belief that we humans (and in particular, our thought and talk) are part of the natural world. What is the relationship between these two kinds of naturalism? Contemporary naturalists

are apt to think that the latter view is a mere corollary of the former. I argue that there is an important sense in which the priority is the other way around: object naturalism depends on "validation" from a subject naturalist perspective – in particular, on confirmation of certain "representationalist" assumptions about the functions of human language. Moreover, I maintain, there are good reasons for doubting whether object naturalism deserves to be validated, in this sense. Thus, an adequate naturalistic philosophy threatens to undermine what most contemporary philosophers have in mind, when they call themselves philosophical naturalists.

10-03-06 *Singularities and Evolutionary Laws* (to H. Halvorson)

Below are the three quotes I have by Poincaré concerning the subject above. The quotes seem horribly inadequate now (with respect to what I remember of the niceness of the article); I wish I had more extensive quotes in the computer. Always too much to do ...

From: H. Poincaré, "The Evolution of Laws," in his book *Mathematics and Science: Last Essays (Dernières Pensées)*, translated by J. W. Bolduc, (Dover, New York, 1963), pp. 1–14.

> Mr. Boutroux, in his writings on the contingency of the laws of Nature, queried, whether natural laws are not susceptible to change and if the world evolves continuously, whether the laws themselves which govern this evolution are alone exempt from all variation. ... I should like to consider a few of the aspects which the problem can assume.

and

> In summary, we can know nothing of the past unless we admit that the laws have not changed; if we do admit this, the question of the evolution of the laws is meaningless; if we do not admit this condition, the question is impossible of solution, just as with all questions which relate to the past. ...
>
> But, it may be asked, is it not possible that the application of the process just described may lead to a contradiction, or, if we wish, that our differential equations admit of no solution? Since the hypothesis of the immutability of the laws, posited at the beginning of our argument would lead to an absurd consequence, we would have demonstrated *per absurdum* that laws have changed, while at the same time we would be forever unable to know in what sense.
>
> Since this process is reversible, what we have just said applies to the future as well, and there would seem to be cases in which we would be able to state that before a particular date the world would have to come to an end or change its laws; if, for example, our calculations indicate that on that date one of the quantities which we have to consider is due to become infinite or to assume a value which is physically impossible. To perish or to change its laws is just about the same thing; a world which would no longer have the same laws as ours would no longer be our world but another one.

and

> No doubt many readers will be dismayed to note that I seem constantly to substitute for the world a system of simple symbols. This is not due simply to a professional habit of a mathematician; the nature of my subject made this approach absolutely necessary. The Bergsonian world has no laws; what can have laws is simply the more or less

> distorted image which the scientists make of it. When we say that nature is governed by laws, it is understood that this portrait is still rather lifelike. It is therefore according to this description and this description only that we must reason, or else we run the risk of losing the very idea of law which was the object of our study.

12-03-06 *The Spirit of Stevie Ray* (to G. L. Comer)

Boy you sure send a lot of pictures of your guitar! Handsome picture of the singer though—you're right, you do look authentic.

I'm just off to the APS March meeting in Baltimore ... where our little efforts in physics has finally been recognized with a topical group all of its own. Sessions all week long. I chair the one on entanglement tomorrow morning, and then give my talk in the session on quantum foundations in the afternoon.

Just made a transparency of this William James quote:

> Metaphysics has usually followed a very primitive kind of quest. You know how men have always hankered after unlawful magic, and you know what a great part in magic *words* have always played. If you have his name, or the formula of incantation that binds him, you can control the spirit, genie, afrite, or whatever the power may be. Solomon knew the names of all the spirits, and having their names, he held them subject to his will. So the universe has always appeared to the natural mind as a kind of enigma, of which the key must be sought in the shape of some illuminating or power-bringing word or name. That word names the universe's *principle*, and to possess it is after a fashion to possess the universe itself.

I'm gonna read that to the audience, and then put up a transparency with a big $|\psi\rangle$ and nothing else on it. Then I'll say that since the beginning of quantum mechanics, the debate has been over what this thing actually is. But now we know its name: it is "expectation." Raw expectation. (A better word than "information" I think.) And now that we finally possess its name, we will after a fashion possess the universe itself.

Then I'll bombard them with too many equations ... as one is expected to do in a physics talk.

23-03-06 *A Review of General Covariance* (to G. L. Comer)

Waking up very slowly. Silly, after an almost sleepless night. All night long I tossed and turned obsessing over the same thought/question:

> An 'action' is defined by the set of its 'consequences.' However the only consequences I can see in a quantum measurement are the refinement of one's expectations. But some actions lead to identical sets of refined expectations with differing probabilities. So they must be different actions after all. But that contradicts the first part. Repeat. Repeat. Repeat. What's going on? Repeat. Repeat. The only consequences of quantum measurement should be refined expectations. What's going on?

Somewhere around 6:00, I said, "Aha, it's 'likelihoods' that are the consequences." I still don't really understand that, but somehow it was enough to let me finally fall asleep. But then it was almost time to get up.

09-05-06 *The Bayesian Big Bang* (to H. Barnum)

Wow, that's some report! Thanks for sending me all that. It sounds like I really did miss a thought-provoking time.

I wouldn't know what to suggest to put in a note to Albert.

Barnumism 3: *Incidentally, the discussion with Albert ended with him saying "so you don't believe parts of the early universe do work on other parts even though there is no subject who is using its knowledge to extract work..." I was rather tired, so called a pause there and will deal with that later. I think it can be dealt with, the main point being that WE understand parts of the early universe as doing work on other parts [...], perhaps, because OUR KNOWLEDGE of those parts is the same kind of knowledge (canonical ensemble at some temperature, blah-de-blah) that would allow us, given large enough apparatus etc..., to extract that much work... because some of the processes that actually go on in the early universe are (I guess...) similar to various kinds of thermodynamic processes of putting stuff in pistons and expanding, etc. [...] [N]evertheless I believe this is basically right... our description of early-universe processes in thermodynamic terms is because we have the same kind of knowledge about it that we have about gases and liquids being boiled and compressed etc... "The radiation background was at XX degrees Kelvin when it decoupled from matter" is a statement of what we think we know about it, not a property of its microstate at the time.*

I've told you my wacky idea that for all Bayesians there must be a big bang, haven't I? In case not, the idea is this. Consider my beliefs about yesterday's events. If we were to lump all of them into a big joint probability distribution, it would have some entropy. Now consider my beliefs about the events the day before that. They too could be lumped into a distribution. However, since I'm probably less sure about the things that happened the day before last than I am about the things that happened yesterday, the latter distribution should have more entropy. And so on we could go further back in time. Assuming I lose effectively all predictability as I conceptually reach back to some finite time (based on the size of my brain and my processing capability), I should end up with a distribution of effectively infinite entropy. Associating (somehow) this distribution with one of canonical form—that's one of the hard steps—I find an effectively infinite temperature in the finite past.

Just another way of saying the universe came into existence (at least with regard to my ability to extract work from it).

Homework Problem: Given reasonable models of a typical brain's inferential powers and the resolution of human senses, estimate the number of years since the big bang.

22-06-06 *Triangles* (to H. C. von Baeyer)

I was just taking care of some administrative things to do with the Växjö conference, and it dawned on me that I haven't yet written you since the conference's end. I was surprised to find you gone already by the Wednesday. I hope you didn't become ill or something. It was also too bad to see you gone especially as I think you missed three of the most interesting talks of the meeting: Bengtsson, Leifer, and D'Ariano. For your reference, here are the two papers associated with Leifer's and D'Ariano's talks:

- quant-ph/0606022
  Title: Quantum Dynamics as an Analog of Conditional Probability
  Author: M. S. Leifer

- quant-ph/0603011
  Title: How to Derive the Hilbert-Space Formulation of Quantum Mechanics From Purely Operational Axioms
  Author: Giacomo Mauro D'Ariano

The D'Ariano construction, to me, really starts to smell right from the Paulian perspective—so, I'm quite pleased with it. It still could use a good bit of Bayesianization, but the mathematics, I think, is starting to get in place. Saturday, I'm off to Italy for a little meeting that D'Ariano put together; it'll be another good opportunity to delve into these things.

Below, I'll paste in the fragment of the note I started to write you in March. One of these days, I should finish that up.

I hope all is well with you, and that you got something out of your Växjö experience.

— — — — — — — — — — — — — — — —

Dear Hans,

I hope you're better by now. I'm between meetings once again: Last Friday I got back from the APS March meeting in Baltimore, and this coming Monday I zip off to Portugal.

While in Baltimore at dinner one evening, as we were waiting for our crabs, somehow your name came up, and I ended up drawing your B3 diagram:

von Baeyerism 11: *The "B3 triad" is a triangle with the words psi, information, and probability at its vertices, and the names Bayes, Born, and Bohr, respectively, along their opposite sides.*

(though I accidentally inverted the vertices and sides).

I was trying to explain why I thought the B3 idea was pretty catchy. However, as we—that is, Rob Spekkens, Matt Leifer, Carl Caves, Hideo Mabuchi, and I—discussed it, I found myself slowly morphing the content of your diagram to better fit my predilections. Here was the result, which was left on a sheet of butcher paper in Obrycki's Crab House:

> A triangle with the names Bayes, Pauli, and Gleason at the vertices, and $p(x)$, "contextuality", and $|\psi\rangle$ along the opposite sides.

It won't be as catchy and memorable, I know (what small fraction of physicists have ever heard of Gleason, and what even smaller fraction have ever heard of "contexuality"?). But let me explain the reasons.

At first it was just that I was dissatisfied with ...

24-06-06 *Notes on "What are Quantum Probabilities"* (to J. Bub)

ABSTRACT: Jeff says, "Chris Fuchs presents a different analysis of the status of the projection postulate as Bayesian updating, associated with a very different account of quantum probabilities as degrees of belief than the view I want to argue for here. ... But this rather complicated analysis misses the essential point." Chris says, "No. It may not be perfect, but it captures the essential point it was meant to capture. It just so happens that that essential point is different from the one Jeff wants to make."

Abstract

Bubism 4: *I distinguish between the measurement problem of quantum mechanics, which is a problem about truth, and a problem about probabilities.*

I like that strategy and think it is an important step forward. It has been my strategy since 1995, though I didn't know it in these terms then. In July 2001, I learned a little about James, Dewey, and pragmatist theories of truth, and realized that that's what I had been on about with respect to quantum measurement. But I started to take the Bayesian step toward quantum probabilities long before that.

At this stage, I doubt very much that you'd be happy to think that you might ultimately be led to a pragmatist notion of truth (in the quantum measurement setting at least), but the distinction you make is—in my eyes—the first step toward it. In any case, there is certainly nothing to be lost by starting to make a distinction between the "issue of quantum measurement" and the "problem of quantum-state collapse." I will certainly admit that the former is not refined or understood well enough for my own tastes at this stage, but I think worrying about the latter anymore is just a waste of time—I think the evidence is simply overwhelming.

Here's the way I put the distinction of the concepts to Oliver Cohen in December 2003. I will quote the passage at decent length because it helps set the stage for the particular points I want to make with regard to your own paper.

> I'm writing you because I've been reading your paper "Classical Teleportation of Quantum States" this week. It's a nice paper, and I very much like the simplicity of your scheme and the point you make with it. I am in complete agreement.
>
> In fact it took me a little down memory lane. You see, Asher Peres and I had used teleportation as an example in our March 2000 *Physics Today* article, "Quantum Theory Needs No 'Interpretation'," precisely to illustrate the sensibility of the conception of a quantum state as a "state of knowledge, rather than a state of nature". When the paragraph peaked in clarity (i.e., before the editor's knife), it went like this:
>
>> The peculiar nature of a quantum state as representing information is strikingly illustrated by the quantum teleportation process. In order to teleport a quantum state from one photon to another, the sender (Alice) and the receiver (Bob) need a pair of photons in a standard entangled state. The experiment starts when Alice receives another photon whose polarization state is unknown to her, though known to some preparer in the background. She performs a measurement on her two photons, and then sends Bob a classical message of only two bits, instructing him how to reproduce the unknown state on his photon. This economy of transmission appears remarkable because to completely specify the state of a photon, namely one point in the Poincare sphere, we need an infinity of bits. However, the disparity is merely apparent. The two bits of classical information serve only to transfer the preparer's information, i.e., his *state*, to be from describing the original photon to describing the one in Bob's possession. This can happen precisely because of the previously established correlation between Alice and Bob.
>
> ... The conclusion you draw, I think, is particularly important: the phenomenon of quantum teleportation only looks surprising and remarkable if one takes an ontic view of the quantum state. In fact, in the past, I have accused some of my friends (some of whom were authors on the original teleportation paper) of sticking with an ontic

interpretation of the quantum state precisely because it is the only way to keep the phenomenon surprising and newsworthy. ...

If you are interested in seeing the struggle Asher and I had in constructing the paragraph above see ... Maybe the main lesson in those discussions is how difficult it is to give up an objectivist language when using quantum states, even for a recalcitrant positivist like Asher, and even in an example intended to be illustrative of why quantum states should be viewed as states of knowledge, rather than states of nature. (The philosophy being: if quantum mechanics looks too very mysterious, then you're probably being wrong-headed about it. Case in point: if teleportation looks mysterious, then you're probably being wrong-headed about it too.)

The sense I get from your paper is that you are much more neutral about the lesson than I am. You say simply: "[O]ur classical version of teleportation is just as impressive as the original protocol, if we think of quantum states as representing states of knowledge. ... If, on the other hand, we think of a quantum state as having ontological content, ..., then our classical version of teleportation is not equivalent to the quantum case," and leave it at that. However, there is a spate of evidence starting to come out that a significant fraction of some of the most 'remarkable' phenomena in quantum information theory can be mocked up with classical toy models just as your own. The only requirement for seeing it is that one must focus on the epistemic states (i.e., the states of knowledge) in such models rather than the ontic states (like the actual H or T in your own model). For instance, Rob Spekkens has a toy model which he has presented in several conferences and which he is writing up presently as a paper, "In Defense of the Epistemic View of Quantum States: A Toy Theory," in which he can reproduce the following quantum mechanical and quantum information-theoretic type phenomena in a pretty NON-REMARKABLE way: the noncommutativity of measurements, interference, a no-cloning theorem, ... and many others. (In particular, he gets teleportation too, just like you do.) As Rob puts it in his abstract:

> Because the theory is, by construction, local and non-contextual, it does not reproduce quantum theory. Nonetheless, a wide variety of quantum phenomena have analogues within the toy theory that admit simple and intuitive explanations. ... The diversity and quality of these analogies provides compelling evidence for the view that quantum states are states of knowledge rather than states of reality, and that maximal knowledge is incomplete knowledge. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

So, given that your paper is an independent and particularly notable link in that, and as opposed to his paper, your result is not buried within over 70 pages (and counting) of text, I very much endorse it. I think the lesson is this: A good lot of quantum information theory is simply regular probability theory and information theory applied in ways that had not been deemed interesting before. *What is interesting and unique to the quantum itself, thus, must be something else.*

In my paper `quant-ph/0205039`, "Quantum Mechanics as Quantum Information (and only a little more)," I tried to give the community to a call to arms by saying this:

> This, I see as the line of attack we should pursue with relentless consis-

tency: The quantum system represents something real and independent of us; the quantum state represents a collection of subjective degrees of belief about *something* to do with that system (even if only in connection with our experimental kicks to it). The structure called quantum mechanics is about the interplay of these two things—the subjective and the objective. The task before us is to separate the wheat from the chaff. If the quantum state represents subjective information, then how much of its mathematical support structure might be of that same character? Some of it, maybe most of it, but surely not all of it.

Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as subjective information will we be in a position to make real progress in quantum foundations. The raw distillate left behind—minuscule though it may be with respect to the full-blown theory—will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.

What your work and Spekkens' work does, from my perspective, is give the best illumination yet of what I was hoping for when I was speaking of "combinations of terms" in that passage. Teleportation—being a certain combination of uses of the axioms of quantum mechanics—is nevertheless a purely probabilistic or information-theoretic effect. As such, it tells us very little about the ontology behind quantum mechanics.

My own view—and the thrust of my research program presently—is that these examples help us to realize that what is unique in quantum mechanics is not the probabilities (i.e., the quantum states) but what the probabilities are applied to. There, I think, lies the essence of quantum mechanics: It is localized in the Kochen-Specker theorem. "Unperformed measurements have no outcomes," as Asher Peres likes to say. That is to say, where quantum mechanics gets its uniqueness is from *breaking* with the old idea that a probability (as a subjective state of knowledge) must be knowledge about a pre-existent reality. Instead, probabilities can just as fruitfully be applied to capturing one's knowledge of "what will come about due to one's actions." The predominant issue becomes how to formalize the difference between probability theory as applied to pre-existent facts and probability theory as applied to "creatables" (for want of a better word). . . .

Bubism 5 : *I show that the projection postulate can be interpreted as a probability updating rule, and I argue for a subjective Bayesian interpretation of quantum probabilities as rational degrees of belief, in the sense of Ramsey rather than de Finetti.*

I would certainly call it a probability update rule too—so, if I have missed an "essential point" it is certainly not that. On the other hand, I think you're hoping that there is an essential distinction between Ramsey and de Finetti, where as far as I can tell there is none. However I'll have much more to say about both of these subjects later.

Introduction

From Classical to Quantum Mechanics

Bubism 6 : *Faced with the conceptual puzzles of quantum mechanics, there is a temptation to begin with a blank slate. It seems, then, that if one were only careful enough with implicit assumptions*

about physical theory and measurement, the characteristic features of the theory could have been foreseen before classical mechanics.

I think this view is entirely mistaken.

I am in agreement with your last sentence. Attempts like the former effectively erase the empirical content (or contingency) of quantum mechanics, and I just don't see that. When I myself invoke the slogan "Quantum Mechanics is a Law of Thought," I only do it partially tongue in cheek, quickly correcting myself. For if quantum mechanics were *only* law of thought, it would be like one of Kant's a priori categories of the understanding. On the other hand, I come dangerously close to viewing "probability theory" (i.e., the theory of coherent gambling) in such a Kantian kind of way. And, indeed, it is partially because of this that I would not want to view quantum mechanics as a *generalized* probability theory.

Bubism 7: *The transition from classical to quantum mechanics involves replacing the representation of properties as a Boolean lattice, i.e., as the subsets of a set, with the representation of properties as a certain sort of non-Boolean lattice.*

The? I would rather say *one possible way of looking at* the transition from classical to quantum mechanics involves blah, blah, blah. And, you partially recover from this a few paragraphs later where you write:

Bubism 8: *Of course, other ways of associating propositions with features of a Hilbert space are possible, and other ways of assigning truth values, including multi-valued truth value assignments and contextual truth value assignments. Ultimately, the issue here concerns what we take as the salient structural change involved in the transition from classical to quantum mechanics, and this depends on identifying quantum propositions that take the same probabilities for all quantum states.*

But let me hang on this point for a moment despite your partial recovery. For when you say things like, "Fuchs misses the essential point," you should realize that that judgement (at most) comes from within a context very different from the one I am working in.

I would, for instance, never say "the representation of properties in quantum mechanics involves as a certain sort of non-Boolean lattice." That is just not the context I'm working in. Similarly, I would not say, as you say in the next section, "Somehow, a measurement process enables an indeterminate property, that is neither instantiated nor not instantiated by a system in a given quantum state, to either instantiate itself or not with a certain probability."—i.e., I would not say that a measurement process instantiates any *property* at all for a quantum system.

Instead, the setting for our quantum Bayesian program (i.e., the particular one of Caves, Schack, and me), is one where all the *properties* <u>intrinsic</u> to a quantum system are timeless and have no dynamical character whatsoever—moreover, those properties have nothing to do with particular quantum state assignments or particular quantum measurement outcomes. In that way, the idea of a non-Boolean lattice simply doesn't apply to them.

John Sipe recently made a nice write-up of our view for his book that, I think, brings this one difference between you and me into pretty stark relief. Maybe it's worthwhile to quote it at length, as it may lay the groundwork for a good bit of our later discussion:

> This interpretation shares some features with operationalism. *Measurements*, for example, are understood in a manner close to that adopted by an operationalist. They are characterized by POVMs, and those abstract elements are associated with tasks in the laboratory undertaken with gadgets that are part of the primitives of the theory.

The result of any such measurement is simply one of a possible number of outcomes, and there is no talk of these measurements "revealing" the value of any variable, in the sense that an arbitrarily precise position measurement in classical mechanics is often described as revealing the position of a particle. Yet, compared to the operationalist's quiet, unassuming terminology of "tasks" and "outcomes," advocates of this interpretation adopt a more active manner of speaking, referring to "actions" (or even "interventions") undertaken by an agent, and the "consequences" that those actions elicit.

This indicates a role for the observer (or agent) in this interpretation that is more significant than the role played by such a person in operational quantum mechanics. The significance of that role becomes clear when we consider the reference of density operators in this interpretation. Density operators do not refer to sets of tasks that define *preparations*, as they do in operational quantum mechanics. Rather, a density operator is taken to encode the beliefs of an agent concerning the probabilities of different consequences of possible future actions. While these beliefs may be *informed* by knowledge of the tasks involved in setting up the particular gadgetry associated with a preparation, they are not *determined* by it. Hence there is not a unique, "correct" density operator necessarily associated with each preparation procedure, as there is in operational quantum mechanics. In the present view two different researchers, one more skilled in quantum mechanics than the other, could adopt different density operators after being identically informed of the details of a particular preparation procedure. One density operator might be more successful than the other in predicting the possible consequences of future actions, but each would be the correct density operator *for that agent* insofar as it correctly encoded that agent's beliefs.

Thus, while the abstract elements in the theory associated with measurements are identified with tasks in the laboratory, as in operationalism, the abstract elements in the theory associated with preparations are identified with beliefs of the agent, signaling a kind of empiricist perspective.

So in contrast to operational quantum mechanics, where density operators are necessarily updated following a measurement — since the combination of the previous preparation and the measurement constitutes a new preparation, and an operationalist associates the new density operator with *that* — in this view there is no necessary updating of a density operator in the light of measurement outcomes, since there is no *necessary* connection between the consequences of an agent's action (more prosaically, "measurement outcomes") and his or her beliefs. After all, foolish researchers, like foolish men and women more generally, could choose not to modify their beliefs concerning the consequences of future actions despite their knowledge of the consequences of recent ones. And note that even wise researchers will not update their beliefs concerning future actions until they *know* the consequences of recent ones; hence a wise researchers "personal density operator" (the only kind of density operator there is in this view!) will not change until that researcher is actually aware of a measurement outcome.

Other abstract elements in the theory, such as the dimension of the Hilbert space, and the dimensions of various factor spaces, are actually associated with instances of attributes of physical objects. Hence with respect to the reference of *these* abstract elements this interpretation is realist. The manner in which this works can best be seen by first reviewing the role measurement outcomes play in revealing aspects of the universe in realist classical mechanics, and then comparing that with the role such outcomes play in this interpretation of quantum mechanics.

An arbitrarily precise position measurement of a bead moving along a wire, in re-

alist classical mechanics, reveals the position of the particle, the instance (say, x = 10 cm) of a particular attribute (bead position) of a physical object (bead) that actually exists in Nature. In contrast, a usual Stern-Gerlach device oriented along the z direction *does not*, in this interpretation of quantum mechanics, reveal the z-component of angular momentum, or for that matter anything else. The particular outcome of one experimental run is simply a consequence of performing the experiment. Nonetheless, repeated experimentation *does* reveal that the electron associated with the atom passing through the device should be taken as a spin-1/2 particle. Here the attribute under consideration is taken to be *internal angular momentum*, and the instance – the irreducible representation appropriate to the particle of interest – *spin-1/2*. The role of an "instance of an attribute" in this interpretation is not to specify one of a number of possible expressions of existence, as it is in realist classical mechanics, but rather to specify one class of possible beliefs – the one that the theory recommends – about the consequences of future interventions of a particular type.

Note that, at least within nonrelativistic physics, the instances of the attributes in this interpretation are fixed. A spin-1/2 particle remains a spin-1/2 particle. Thus there are no dynamical variables in this theory, only nondynamical variables analogous to the mass of a particle in nonrelativistic classical mechanics. The point of physics is to identify these nondynamical variables. Repeated interventions by experimentalists, and the careful noting of the range of consequences that those interventions elicit, is how these fixed instances are discovered.

In this interpretation of quantum mechanics, with its mix of operationalist, empiricist, and realist identification of abstract elements in the theory, these fixed instances specify the [[agent independent features]] of the "quantum world," and it is the business of physics to figure them out. This is done by experimentation, and the theoretical linking of basis vectors in the appropriate Hilbert space with various measurements, providing an "anchor" for those basis kets to our experience, the consequences of our actions. Particularly significant is the Hamiltonian operator and its basis kets [[in Caves' particular version of all this]]. As time evolves during what is colloquially described as "unitary evolution," we have the option to modify our beliefs or to modify the anchors of those beliefs; the first strategy corresponds to the usual Schroedinger picture, the second to the Heisenberg picture.

Regardless of the strategy, the [[properties intrinsic to the]] quantum world of this interpretation [[are]] a fixed, static thing. [[This aspect of the quantum world]] is a frozen, changeless place. Dynamics refers not to the quantum world, but only to our actions, our experiences, and our beliefs as agents. Or, more poetically (a la Chris), life does not arise from our interventions; it is our interventions.

John doesn't represent us correctly in every detail of this presentation—for the purpose at hand, it only seemed essential to modify him in a few instances, which I have have marked with double brackets [[·]]—but I would say he is roughly on track, and he certainly gets it that we are not concerned with the usual way of ascribing properties to quantum systems via the values of measurement outcomes or probability-1 predictions (i.e., the eigenvector-eigenvalue link).

Which brings me back again to your paper:

Bubism 9 : *For a quantum state, the properties represented by Hilbert space subspaces are not partitioned into two such mutually exclusive and collectively exhaustive sets: some propositions are assigned no truth value. Only propositions represented by subspaces that contain the state are*

assigned the value 'true,' and only propositions represented by subspaces orthogonal to the state are assigned the value 'false.' This means that propositions represented by subspaces that are at some non-zero or non-orthogonal angle to the ray representing the quantum state are not assigned any truth value in the state, and the corresponding properties must be regarded as indeterminate or indefinite: according to the theory, there can be no fact of the matter about whether these properties are instantiated or not.

You see, my way of looking at things wouldn't even allow me to say what you say here. It is just a very different world that I am working in.

To try to make this point, let me quote a couple of emails I wrote to Bas van Fraassen a few months ago. It started with my saying this:

> The way I view quantum measurement now is this. When one performs a "measurement" on a system, all one is really doing is taking an ACTION on that system. From this view, time evolutions or unitary operations etc., are not actions that one can take on a system; only "measurements" are. Thus the word measurement is really a misnomer—it is only an action. In contradistinction to the old idea that a measurement is a query of nature, or a way of gathering information or knowledge about nature, from this view it is just an action on something external—it is a kick of sorts. The "measurement device" should be thought of as being like a prosthetic hand for the agent—it is merely an extension of him; in this context, it should not be thought of as an independent entity beyond the agent. What quantum theory tells us is that the formal structure of all our possible actions (perhaps via the help of these prosthetic hands) is captured by the idea of a Positive-Operator-Valued Measure (or POVM, or so-called "generalized measurement"). We take our actions upon a system, and in return, the system gives rise to a reaction—in older terms, that is the "measurement outcome"—but the reaction is in the agent himself. The role of the quantum system is thus more like that of the philosopher's stone; it is the catalyst that brings about a transformation (or transmutation) of the agent.
>
> Reciprocally, there [[may]] be a transmutation of the system external to the agent. But the great trouble in quantum interpretation—I now think—is that we have been too inclined to jump the gun all these years: We have been misidentifying where the transmutation indicated by quantum mechanics (i.e., the one which quantum theory actually talks about, the "measurement outcome") takes place. It [[may]] be the case that there are also transmutations in the external world (transmutations in the system) in each quantum "measurement", BUT that is not what quantum theory is about. [[Quantum mechanics]] is only a hint of that more interesting transmutation. [[Instead, the main part of quantum mechanics is about how]] the agent and the system [[together bring about]] a little act of creation that ultimately has an autonomy of its own—that's the sexual interpretation of quantum mechanics.

which led to the following dialogue:

> **van Fraassenism 8 :** *Writers on the subject have emphasized that the main form of measurement in quantum mechanics has as result the value of the observable at the end of the measurement – and that this observable may not even have had a definite value, let alone the same one, before.*
>
> Your phrase "MAY NOT even have a definite value" floated to my attention. I guess this floated to my attention because I had recently read the following in one of the Brukner/Zeilinger papers,

> Only in the exceptional case of the qubit in an eigenstate of the measurement apparatus the bit value observed reveals a property already carried by the qubit. Yet in general the value obtained by the measurement has an element of irreducible randomness and therefore cannot be assumed to reveal the bit value or even a hidden property of the system existing before the measurement is performed.

I wondered if your "may not" referred to effectively the same thing as their disclaimer at the beginning of this quote. Maybe it doesn't. Anyway, the Brukner/Zeilinger disclaimer is a point that Caves, Schack, and I now definitely reject: From our view all measurements are generative of a NON-preexisting property regardless of the quantum state. I.e., measurements never reveal "a property already carried by the qubit." For this, of course, we have to adopt a Richard Jeffrey-like analysis of the notion of "certainty"—i.e., that it too, like any probability assignment, is a state of mind—or one along (my reading of) Wittgenstein's—i.e., that "certainty is a tone of voice"—to make it all make sense, but so be it.

and

van Fraassenism 9 : *Suppose that an observer assigns eigenstate $|a\rangle$ of A to a system on the basis of a measurement, then predicts with certainty that an immediate further measurement of A will yield value a, and then makes that second measurement and finds a. Don't you even want to say that the second measurement just showed to this observer, as was expected, the value that A already had? He does not need to change his subjective probabilities at all in response to the 2nd measurement outcome, does he?*

It is not going to be easy, because this in fact is what Schack and I are actually writing a whole paper about at the moment—this point has been the most controversial thing (with the Mermin, Unruh, Wootters, Spekkens, etc., crowd) that we've said in a while, and it seems that it's going to require a whole paper to do the point justice. But I'll still try to give you the skinny of it:

- Q: He does not need to change his subjective probabilities at all in response to the 2nd measurement outcome, does he?
- A: No he doesn't.
- Q: Don't you even want to say that the second measurement just showed to this observer, as was expected, the value that A already had?
- A: No I don't.

The problem is one of the very consistency of the subjective point of view of quantum states. The task we set before ourselves is to completely sever any supposed connections between quantum states and the actual, existent physical properties of the quantum system. It is only from this—if it can be done, and of course we try to argue it can be done—that we get any "interpretive traction" (as Chris Timpson likes to say) for the various problems that plague QM. [[...]]

This may boil down to a difference between the Rovellian and the Bayesian/Paulian approach; I'm not clear on that yet. [[. . .]] Rovelli relativizes the states to the observer, even the pure states, and with that—through the eigenstate-eigenvalue link—the values of the observables. I'm not completely sure what that means in Rovelli-world yet, however.

I, on the other hand, do know that I would say that a measurement intervention is always generative of a new fact in the world, whatever the measurer's quantum state for the system. If the measurer's state for the system HAPPENS to be an eigenstate of the Hermitian operator describing the measurement intervention, then the measurer will be confident, CERTAIN even, of the consequence of the measurement intervention he is about to perform. But that CERTAINTY is in the sense of Jeffrey and Wittgenstein above—it is a "tone of voice" of utter confidence. The world could still, as a point of principle, smite the measurer down by giving him a consequence that he predicted to be impossible. In a traditional development—with ties to a correspondence theory of truth—we would then say, "Well, that proves the measurer was wrong with his quantum state assignment. He was wrong before he ever went through the motions of the measurement." But as you've gathered, I'm not about traditional developments. Instead I would say, "Even from my view there is a sense in which the measurer's quantum state is WRONG. But it is MADE WRONG by the ACTUAL consequence of the intervention—it is made wrong on the fly; it's wrongness was not determined beforehand." And that seems to be the main point of contention.

Particularly this is going to be a key point when I finally come to the analysis in Section 7 of your paper.

The Probability Problem

Bubism 10: *The orthodox answer is that the probability assigned to a property of a system by a quantum state is to be understood as the probability of finding the property in a measurement process designed to ascertain whether or not that property is instantiated. I will defend this proposal later in the paper, but a little thought will reveal that it is rather problematic. When the system is represented by a quantum state that assigns a certain property the probability 1/2, say, this property is indeterminate. Physicists would say that ascribing the property to the system in that state is 'meaningless.' But somehow it makes sense to design an experiment to ascertain whether or not the property is instantiated by the system. And in such a measurement, the probability is asserted to be 1/2 that the experiment will yield the answer 'yes,' and 1/2 that the experiment will yield the answer 'no.' Clearly, a measurement process in quantum mechanics is not simply a procedure for ascertaining whether or not a property is instantiated in any straightforward sense. Somehow, a measurement process enables an indeterminate property, that is neither instantiated nor not instantiated by a system in a given quantum state, to either instantiate itself or not with a certain probability; or equivalently, a proposition that is neither true nor false can become true or false with a certain probability in a suitable measurement process.*

I found this paragraph interesting. Particularly as I could both agree and disagree with the last sentence! The difference comes at the semi-colon. That is, I disagree with this: "Somehow, a measurement process enables an indeterminate property, that is neither instantiated nor not instantiated by a system in a given quantum state, to either instantiate itself or not with a certain probability." But I agree with "A proposition that is neither true nor false can become true or false with a certain probability in a suitable measurement process." The trouble comes in at the connective "equivalently." I don't believe those separate thoughts are equivalent at all. The saving grace of the second of the two clauses for me is that you don't explicitly mention what the proposition is about. In the first clause, on the other hand, you are talking about properties of the system.

The Measurement Problem

Bubism 11: *In classical theories, we measure to find out what we don't know, but in principle a measurement does not change what is (and even if it does change what is, this is simply a change or disturbance from one state of being to another that can be calculated on the basis of the classical theory itself). In quantum mechanics, measurements apparently bring into being something that was indeterminate, not merely unknown, before, i.e., a proposition that was neither true nor false becomes true in a measurement process, and the way in which this happens according to the theory is puzzling.*

Here again, I can agree. You nicely did not say anything about what the proposition refers to. For instance, with your wording here—as long as I am careful to take the paragraph out of context!—I am free to think that the proposition that gains a truth value with the process of measurement refers to MY sensations, not a property of the system at all.

Indeed, I rather like the very next sentence:

Bubism 12: *The standard measurement problem of quantum mechanics is fundamentally* a problem about truth *. . . , distinct from the probability problem.*

as long as I am not forced to refill the ellipses with your parenthetical "(or the instantiation of properties)." I think this is the fundamental problem—it is about *truth*—and I am very happy that you're saying that much. When you say things in the philosophy-of-science community people listen. It is quite important to make a careful distinction between the problem of probability and the problem of truth, and not many people are doing that presently.

My own conviction that the measurement problem is fundamentally a problem about truth explains my fascination with James, Dewey, Schiller, Rorty, and Putnam, and the whole pragmatist framework for truth. Here is something I wrote Carl Caves in 2001:

> Today I focused on rounding up some more William James, John Dewey, Percy Bridgman material. I think James is taking me over like a new lover. I had read a little bit of him before, but I think I was more impressed with his writing style than anything. But I was drawn back to him by accident, after reading Martin Gardner's *Whys of a Philosophical Scrivener*. Gardner devoted a lot of time knocking down James' theory of truth, because it is just so much easier to accept an underlying reality that signifies whether a proposition is true or false, rather than saying that the knowing agent is involved in eliciting the very proposition itself (along with its truth value). And something clicked! I could see that what James was talking about might as well have been a debate about quantum mechanics. He was saying everything in just the right way. (Let me translate that: he was saying things in a way similar to the way I did in my NATO "appassionata.") And things have only gotten better since.

My recommendation—tongue in cheek—I like the direction your paper starts to move in, but I think you could take a good dose of James!

Here's a little dialogue I had with Bill Demopoulos earlier this year:

> **Demopoulosism 5:** *I also don't see why we should need something as fundamental as KS to sustain the notion that "unperformed measurements don't have outcomes." I'm being a devil's advocate here because I think what you really mean is that without a measurement of whether the cat is alive, the cat is neither alive nor not alive. But would you put it this baldly? If not, why not?*

To answer your question in the best way I know how at the moment, I would say: The transformation that quantum mechanics speaks about, the transformation from a 'superposition' to 'aliveness' or 'deadness', is a transformation *within the agent*, and that transformation cannot take place without some interaction with the external physical system labelled by the word 'cat'. What happens to 'cat' itself (described in a way that makes no reference to the agent)? On that, I think quantum mechanics is silent. With a mantra: Quantum mechanics is a theory for ascribing (and intertwining) personal probabilities for the personal consequences of one's personal interactions with the external world.

Bubism 13: *The most sophisticated formulation of Everetts interpretation is probably the Saunders-Wallace version (Saunders, 1998; Wallace, 2003). Here the preferred basis is selected by decoherence, and probabilities are introduced as rational degrees of belief in the Bayesian sense via a decision-theoretic argument originally due to Deutsch (Deutsch, 1999).*

Sophisticated in the sense of being complicated, yes. Sophisticated in the sense of hiding things in layer upon layer. See our original criticism of Deutsch—Barnum et al., "Quantum Probability from Decision Theory?," Proc. Royal Society London A **456**, 1175–1182 (2000), and `quant-ph/9907024`—which still stands, as well as some new nice stuff by Huw Price, `quant-ph/0604191`. Or talk to Jos Uffink, who's got some very good replies.

Bubism 14: *Of course, quantum mechanics could be false, but it seems wildly implausible that a modification of quantum mechanics whose sole motivation is to solve the measurement problem will survive fundamental advances in physics driven by other theoretical or experimental questions.*

I like this point very much.

Solving the Probability Problem

Bubism 15: *[T]he physical world is* nonlocal*, in that spacelike separated systems can occupy entangled states that persist as the systems separate.*

This language hints of an ultimately ontic view of quantum states.

Bubism 16: *Solving the probability problem* without reducing the problem to a solution of the measurement problem (the truth problem)*, amounts to treating quantum mechanics as a theory of information, in which no measurement outcomes are certified as determinate* by the theory*. Rather, measuring instruments are sources of classical information in Shannon's sense, where the individual occurrence of a particular distinguishable event produced stochastically by the information source lies outside the theory. In this sense, a measuring instrument, insofar as it functions as a classical information source, is ultimately a 'black box' in the theory. So a quantum description will have to introduce a 'cut' between what we take to be the ultimate measuring instrument in a given measurement process and the quantum phenomenon revealed by the instrument. The 'cut' is just a reflection of the fact that quantum mechanics is a* theory about the representation and manipulation of information *constrained by the possibilities and impossibilities of information-transfer in our world, rather than a theory about the ways in which nonclassical waves and particles move.*

I like much of this of course, if not not literally all.

Bubism 17: *If we set aside the measurement problem, the Gleason probabilities cannot be intelligibly interpreted as the objective chances or relative frequencies that dynamical variables take determinate values, and the only viable option is a subjective Bayesian interpretation of the quantum probabilities as rational degrees of belief.*

Why? Of course, I like the sentiment of this, but it'd be nice to see in your words an extended discussion of this point. As it is written right now, I think this represents a weak point of the paper.

On the other hand, I don't agree with any of this:

Bubism 18: *To show that quantum mechanics can 'stand on its own feet' as a theory of probability, i.e., as theory of information, we need to take account of the phenomenon of decoherence: an extremely fast process that occurs in the spontaneous interaction between a macrosystem and its environment that leads to the virtually instantaneous suppression of quantum interference. What happens, roughly, is that a macrosystem like a measuring instrument or Schrödinger's cat typically becomes correlated with the environment—an enormous number of stray dust particles, air molecules, photons, background radiation, etc.—in an entangled state that takes a certain form with respect to a preferred set of basis states, which remain stable as the interaction develops and includes more and more particles. It is as if the environment is 'monitoring' the macrosystem via a measurement of properties associated with the preferred states, in such a way that information about these properties is stored redundantly in the environment. This stability, or robustness, of the preferred basis, and the redundancy of the information in the environment, allows one to identify certain emergent structures in the overall pattern of correlations—such as macroscopic pointers and cats and information-gatherers in general—as classical-like: the correlational information required to reveal quantum interference for these structures is effectively lost in the environment. So it appears that the information theoretic constraints are consistent with both (i) the conditions for the existence of measuring instruments as sources of classical information, and (ii) the existence of information-gatherers with the ability to use measuring instruments to apply and test quantum mechanics, given a characterization of part of the overall system as the environment. That is, by selecting a preferred basis, decoherence provides an explanation for the emergence of classical information in a quantum correlational structure.*

This is just Zurekian obfuscation. You have already invoked the black-box concept for measurement, explaining the need for this if quantum mechanics is to be viewed as a theory of information. You did that nicely. Why back off and re-ontologize all the ideas to try to *explain* the existence of classical observers? WHO is writing down these decohering wavefunctions? In an information theoretic approach to quantum mechanics (by that I mean a non-ontic approach to wave functions), it is not fair to give in to temptation at the last moment and invoke God in the quad.

Bubism 19: *If something like the above account of decoherence is acceptable, then the probability problem reduces to showing that the probabilities assigned to measurement outcomes by these information-gatherers, in the subjective Bayesian sense, are just the Gleason probabilities.*

Again, why? It seems you're being very short on the key points you want to make here (i.e., this point and the last point above where I said, "Why?").

The Projection Postulate as Bayesian Updating

Bubism 20: *An analysis of quantum probabilities as measures of ignorance in the Bayesian sense, i.e., as degrees of belief measured by rational betting behaviour, has been developed by Schack et al.*

(2001), and Caves et al. (2002). In Pitowskys formulation (Pitowsky, 2002, 2005), the structure of 'quantum gambles' encoded in the subspace structure of Hilbert space imposes nonclassical probabilistic constraints that define a logic of partial belief in the sense of Ramsey.

What is this explicit distinction between Ramsey and de Finetti that you keep invoking? Can you articulate it?

Here is a quote by Keynes on Ramsey. How do you react to it?

> The application of these ideas [regarding formal logic] to the logic of probability is very fruitful. Ramsey argues, as against the view which I had put forward, that probability is concerned not with objective relations between propositions but (in some sense) with degrees of belief, and he succeeds in showing that the calculus of probabilities simply amounts to a set of rules for ensuring that the system of degrees of belief which we hold shall be a *consistent* system. Thus the calculus of probabilities belongs to formal logic. But the basis of our degrees of belief—or the *a priori*, as they used to be called—is part of our human outfit, perhaps given us merely by natural selection, analogous to our perceptions and our memories rather than to formal logic.

And here is a long quote by Sandy Zabell, "Ramsey, Truth, and Probability." How do you react to it, along with my parenthetical comment?

> The key point is that previous attempts to explain induction had attempted to model the process by a unique description of prior beliefs [[references]], or by a very narrow range of possibilities [[references]]. De Finetti realized that because probability is a logic of consistency, one can never—*at a given instance of time*—uniquely dictate the partial beliefs of an individual; at most one can demand consistency. The essence of inductive behavior, in contrast, lies not in the specific beliefs that an individual entertains at any given point in time, but the manner in which those beliefs evolve over time. [[In this way it is exactly like classical logic: One is not judged as irrational for starting with the incorrect truth value for some proposition in one's considerations; one is judged irrational only if one makes an incorrect inference in the proof process.—CAF]] Let us pause briefly over this point.
>
> I change my mind slowly; you do so with rapidity; you think I am pigheaded, I think you are rash. But neither of us is of necessity irrational. Disagreement is possible even if we share the same information; we may simply be viewing it in a different light. This is what happens every time the members of a jury disagree on a verdict. Of course it can be argued that the members of the jury do not share the same body of facts: each brings to the trial the sum total of his life experiences, and on juror tries to persuade another in part by drawing upon those experiences and thus enlarging the background information of their fellow jurors. It is the credibilist view of probability that if you knew what I knew, and I knew what you knew, then you and I would—or at least should—agree.
>
> Such a metaphysical stance may well be, as I. J. Good says, "mentally healthy". But it is an article of faith of no real practical importance. None of us can fully grasp the totality of our own past history, experience, and information, let alone anyone else's. The goal is impossible: our information cannot be so encapsulated.

Bubism 21 : *Here I want to show that the quantum rule is in fact just a noncommutative version of the classical rule.*

The next three pages give a very nice treatment! (And I'm serious about that.) Why would I—Bayesian to the core—ever want to find to find a different analogy between quantum collapse and Bayesian conditionalization than the one you present here? In a quick word, the idea of a POVM (even a standard von Neumann measurement) as an analog of an indicator function goes in the wrong direction for what I want to squeeze out as the essential structure of quantum mechanics. Indicator functions carry the baggage of thinking about quantum measurement as a process of revealing pre-existent values. But I'll come back to this in a longwinded way, as you can guess.

Bubism 22 : *The prior probability assignment of an observer about to make a measurement on a quantum system S is given by an initial density operator $\rho_S \in \mathcal{H}_S$. What should an observer take this density operator to be?*

Here we go again ... You sure have some dangerous remaining objectivist tendencies when it comes to probabilities and quantum states.

Bubism 23 : *Suppose the universe $U = S + E$ is in an initial pure state $|\psi\rangle \in \mathcal{H}_R$.*

Whose pure state? Is the state information (in the sense of Bayesian probability theory), or is it not?

Quantum Probabilities as Rational Degrees of Belief

Bubism 24 : *Now, Fuchs points out (Fuchs, 2002b, p. 34) that the state change following a quantum measurement of a POVM $\{E_d\}$ can be presented as a 2-state process:*

I think you meant "2-stage" here.

Bubism 25 : *In the case of a projective measurement $\{E_d\} = \{E_d = |d\rangle\langle d|\}$, where the E_d are projection operators, the state change on measurement with outcome d is a collapse corresponding to a readjustment by the unitary operator $U_d = |d\rangle|\psi\rangle$.*

There's a typo at the end of the sentence here; an operator is not a state. Here is the way I said it in the final version of the paper (after I had fixed my own typo at this very spot!):

> In particular, when the POVM is an orthogonal set of projectors $\{\Pi_i = |i\rangle\langle i|\}$ and the state-change mechanism is the von Neumann collapse postulate, this simply corresponds to a readjustment according to unitary operators U_i whose action on the subspace spanned by $|\psi\rangle$ is
> $$|i\rangle\langle\psi| \ . \tag{.1}$$

Finally, we come to the real point of contention:

Bubism 26 : *Fuchs concludes from this analysis that quantum collapse can be regarded as a non-commutative version of Bayes's rule. But this rather complicated analysis misses the essential point. It is precisely the 'violent' collapse transition (17), where measurement is only disturbance, that has to be explained as a noncommutative variant of Bayes' rule and not merely the 'gentle' selection from an initial density operator of a term corresponding to the outcome of a measurement.*

"When a distinction in concepts can be made, a distinction in concepts should be made." I thought that methodology was the working bread and butter of the philosopher? Let me try to better explain what I did, and why I did it. I made a distinction; I followed through with the implications; I tried to learn a lesson.

The distinction, when it was found, was made because of this: I am working in a quantum foundational context that is trying very hard to dispel that idea that quantum "measurement" has anything *a priori* to do with information gathering about things intrinsic to the quantum system. I tried to give you ample evidence and explanation of that above. Still, though, let me give one last point of reference in that regard. It comes from the same email discussion with van Fraassen that I referenced before. Then I'll return to giving an account of the "distinction" I've alluded to.

> **van Fraassenism 10:** *I thought I would after all not follow you in replacing the term "measurement", despite all the bad effects old connotations have had in various discussions. We need to bracket the old connotations such as that a measurement result reveals a pre-existing value for the measured observable. But I think we can do that because: ...*
>
> *[T]here is a certain kind of retrodictive inference possible also on the basis of qm measurements. For a long time the paradigm was a source preparing a stream of particles in a certain state – measurements on samples taken from the stream give a good basis for conclusions about just what state the source was preparing, and these conclusions can then be used to predict the outcomes of further measurements made on later samples of the stream.*
>
> This is what we in quantum information call quantum-state tomography. One can indeed think of a quantum measurement outcome as *giving information* in the old standard sense in that case and not simply being the "unpredictable consequence of one's action." But then "giving information" is quantified by Shannon's "mutual information," $I(X, Y)$ and not simply by his entropy function $H(Y)$. That is, one has two random variables in the game—one treated classically, namely the "unknown preparation" X, and the other one purely quantum mechanical, the result Y of the measurement interaction. Those two variables have quite different roles, and one indeed would not want to think of X as the "consequence of one's interaction." On the other hand, without making explicit mention of X one has no means for thinking of the elicitation of Y as giving information about anything at all. Before seeing the value of Y, one can expect to be *surprised* to the extent quantified by $H(Y)$, but that's where the story stops.
>
> [For a more detailed Bayesian-like development of this point, you might have a look at our paper "Unknown Quantum States and Operations, a Bayesian View," `quant-ph/0404156` and some of the references therein. Particularly the Introduction and Concluding section might be of some interest to you with regard to the present discussion.]
>
> The only point I want to make to you with regard to your remark above is that, for these reasons, I would say it has no bearing on the issue at debate: I.e., the debate of whether it is better to think of a "quantum measurement" as simply an action with an unforeseeable consequence, or rather as a kind of "question-asking" or "information-gathering." It is tangential.

So, what is this distinction that I was trying to capture in my formalism that it seems your way of viewing quantum conditionalization does not take into account? Take a single quantum

system, for which some quantum-state assignment ρ has been made. Now imagine performing a quantum "measurement" on the system and updating the quantum state to some new value ρ_d, consequent to the measurement's outcome. One can write that update in the usual Kraus way or in my idiosyncratic way. Let's even take the special case where the update is just the Lüders rule—noting that it too can be viewed in both ways.

You have a state change; I have a state change. In the end, they're identical of course. But whereas you have a change that looks *solely* like an application of Bayesian conditionalization, I don't. I have a handle for making a distinction—and in fact it is a distinction I want to make. You write, "It is precisely the 'violent' collapse transition (17), where measurement is only disturbance, that has to be explained as a noncommutative variant of Bayes' rule and not merely the 'gentle' selection from an initial density operator of a term corresponding to the outcome of a measurement."

No. It doesn't need to be, and I don't want it to be. I want it to remain a distant, maybe unrecognizable addition to Bayes, not a variant at all. It quantifies the extent to which my previous opinion changes radically (as compared to the change it would have made solely through Bayes) upon the receipt of the data. It gives me a handle for exploring a new issue. If a quantum "measurement" is not merely the receipt of data with regard something pre-existent, why should it look like a learning process at all? Well, in truth, the consequence of the measurement may allow me a refinement in predictions for the next round of measurement. But to some extent it is simply new input into my beliefs that my previous opinion could not take into account.

In the end, where I think I want to go with this kind of thing is this. Sandy Zabell puts it nicely in her essay on Ramsey.

> Ramsey did consider the question of the dynamic evolution of belief. Conditional probability is defined in terms of conditional bets; it states the odds that someone "would now bet on p, the bet only to be valid if q is true." This approach has since been adopted as the basis of the commonly accepted subjectivist definition (see, e.g., de Finetti, 1972, p. 193); but of course it does not address the relation that conditional probabilities—thus defined—may have to the actual degrees of belief one holds after the observation of an event. And here we run up against an apparent inconsistency in Ramsey's views. Initially, Ramsey notes there is not reason to automatically identify the two quantities:
>
> > the degree of belief in p given q is not the same as the degree to which a person would believe p, if he believed q for certain; for knowledge of q might for psychological reasons profoundly alter his whole system of beliefs.
>
> But further on in the last section of his essay Ramsey writes: ...
>
> Clearly the emphasized portion of the quotation completely ignores the profound insight of the preceding quotation; perhaps this second passage represents a portion of the text written at an earlier stage.

(OK, I'm too lazy to copy the second passage of Ramsey, which has no relevance to my argument.) Anyway, this idea of Ramsey's has been developed at great length in the work of van Fraassen, Skyrms, and Richard Jeffrey—in fact it is the standard fair of what Richard Jeffrey calls "radical probabilism." Standard conditioning in the probabilistic setting is NOT always the way to go. (The precise statement is that diachronic Dutch book arguments *always* need supplementary assumptions that the synchronic ones don't need.

I want to fit quantum conditioning into that framework, if possible, and this distinction I draw—i.e., splitting the quantum state change into two components—gives me some handle on that. I regret some of the phrases in my original description of this now—all this business about

"gut-wrenching violence"—(remember, that was 4 years ago!), and I am sorry it may have caused confusion about my motivations, but the technical result is still exactly where I want to go.

Bubism 27 : *The above analysis simply illustrates the way the projection postulate works in quantum mechanics. It does not explain it, in the sense of showing how quantum collapse can be understood as a noncommutative version of Bayes rule for updating states of belief. What Fuchs analysis in terms of POVMs shows is that the relevant features of my example are quite general. But, again, this in itself does not demystify quantum collapse as a noncommutative version of Bayes' rule.*

Well, I didn't think I had claimed to demystify the particular form of the quantum collapse rule so much as quantify it in a new way (one more in line with the point of view I am trying to build). If my bad writing style confused you, again I apologize.

Bubism 28 : *Fuchs' Bayesian interpretation of quantum probabilities follows de Finetti rather than Ramsey and reflects de Finetti's instrumentalism.*

When I get back home, I'll send you a reading list on Ramsey. The divergence is not as drastic as you think. The only difference I have ever made note of is that Ramsey was willing to call "intersubjective agreement" on a probability assignment "objective chance." But mixing the logical and empirical realms, Ramsey did not do or advocate. I'll bet money on that.

Bubism 29 : *For de Finetti, science is just an extension of common sense and cannot inform the 'logical' aspect of probability formalized in the probability calculus through the notion of coherence. So it would not make sense to regard quantum mechanics as a nonclassical probability theory or theory of information, where the formal features of this new theory encode objective structural relations about the physical world. Physics can only be relevant to the extra-logical and context-dependent evaluation of probabilities.*

That is pretty much correct. And it strikes me as exactly what one would want if one were a *realist*! There is our reasoning about the world, and then there is the world itself. When one starts to mix those two ingredients, that's when one is becoming an instrumentalist!

Yes, I definitely want to see the essence of quantum mechanics being born out as an extra-logical and context-dependent (let me say empirical) addition to probability theory. It is a layer on top of probability (for instance the restrictive region that I always draw on the simplex), and in that way quantum reality (whatever that is) keeps its autonomy from my thought.

Bubism 30 : *What Fuchs takes himself as establishing is that quantum states represent subjective degrees of belief . . .*

Yes, my paper was meant to be a foray in that direction. But, the remainder of your sentence

Bubism 31 : *. . . and that quantum collapse is Bayesian conditionalization* in the standard sense.

not at all. Besides this blatantly contradicts the remaining sentences in your paragraph:

Bubism 32 : *What the* physics *tells us is summed up in his statement . . . : 'The world is sensitive to our touch.' That is, there is an irreducible nonclassical disturbance that occurs whenever we probe the world. But this enters into the* readjustment *of the observer's probabilistic beliefs* after *the application of Bayesian updating, which is a straightforward refinement of prior degrees of belief in the usual sense.*

And finally,

Bubism 33 : *The difference, ultimately, is between an instrumentalist approach to quantum mechanics with the application of a strictly classical Bayesian theory of probability, and an interpretation of quantum mechanics as a nonclassical theory of information, in which the structure of quantum gambles, considered as an objective feature of reality, informs the correlational structure of quantum probabilities.*

No, no, a million times, no! My point of view is in no form an instrumentalism. With each of our theories we are making direct or indirect statements of what we believe of the objective, external world. The quantum Bayesian view Caves, Schack, Appleby, me, and whoever else to quantum mechanics is no less so. On the other hand, imagining melding empirical statements into the very structure of the laws of thought—as you and Itamar seem to do—represents a real danger with regard to the realist-instrumentalist divide.

Maybe more later, but I'll stop now and print this out.

Index