Ideas Abandoned en Route to QBism

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The interpretation of quantum mechanics known as QBism developed out of efforts to understand the probabilities arising in quantum physics as Bayesian in character. But this development was neither easy nor without casualties. Many ideas voiced, and even committed to print, during earlier stages of Quantum Bayesianism turn out to be quite fallacious when seen from the vantage point of QBism.

If the profession of science history needed a motto, a good candidate would be, “I think you’ll find it’s a bit more complicated than that.” This essay explores a particular application of that catechism, in the generally inconclusive and dubiously reputable area known as quantum foundations.

QBism is a research program that can briefly be defined as

an interpretation of quantum mechanics in which the ideas of agent and experience are fundamental. A “quantum measurement” is an act that an agent performs on the external world. A “quantum state” is an agent’s encoding of her own personal expectations for what she might experience as a consequence of her actions. Moreover, each measurement outcome is a personal event, an experience specific to the agent who incites it. Subjective judgments thus comprise much of the quantum machinery, but the formalism of the theory establishes the standard to which agents should strive to hold their expectations, and that standard for the relations among beliefs is as objective as any other physical theory [1].

The first use of the term QBism itself in the literature was by Fuchs and Schack in June 2009 [2]. Prior to this, they had employed it in talks and correspondence [3, p. 1707], illustrating the lexicographer’s principle that words predate their preservation in books. Introducing a collection of correspondence, Fuchs wrote that three characteristics of the QBist research program distinguish it from existing interpretations [3, p. ix].

First is its crucial reliance on the mathematical tools of quantum information theory to reshape the look and feel of quantum theory’s formal structure. Second is its stance that two levels of radical “personalism” are required to break the interpretational conundrums plaguing the theory. Third is its recognition that with the solution of the theory’s conundrums, quantum theory does not reach an end, but is the start of a great journey.

QBism grew out of Quantum Bayesianism, a loosely-defined school of thought typified by a paper by Caves, Fuchs and Schack, “Quantum probabilities as Bayesian probabilities” ([4], hereinafter CFS 2002). The most prominent thread within Quantum Bayesianism may be the one that, in the hands of Fuchs and Schack, later developed into QBism, but the term is applicable much more broadly. It encompasses some writings of Bub and Pitowsky [5–7], for example, and could easily include earlier suggestions of Youssef [8] and Baez [9], work by Leifer and Spekkens [10], the “entropic dynamics” of Caticha [11] and so forth. These
views overlap to the extent that they all advocate interpreting the probabilities in quantum physics according to some variety of Bayesianism. They overlap, but they do not coincide. What else should one expect, given that even before quantum physics was brought into the game, the jest that there were “46,656 varieties of Bayesianism” [12] was only a mild exaggeration?

Perusing the quantum-foundational literature and partaking in conversations with quantum foundationers, I have been surprised by the inclination to cite CFS 2002 as defining QBism. Sometimes, it is invoked by itself as the canonical QBist document, and on other occasions, it is mixed together indiscriminately with genuinely QBist sources. Without commenting in depth on the merits of these varied works, I find this situation puzzling. All three authors disavow the perspective of this paper [2, 13], whether they have continued along the path of developing QBism (Fuchs and Schack) or not (Caves).

The philosophy of science, we fondly imagine, should be the discipline that exposes the distinctions which physicists coarsely gloss over. Perhaps QBism has generated too many expositions to pay attention to just one [14–17], but reading is in the scholar’s job description, and fortunately, not all QBist writings are as long as some of them are. Indeed, some authors have gotten the coordinates right for genuine QBist expositions [18–21].

Space constraints and the mostly ahistorical writing style of physics journals have prevented earlier QBist articles from delineating which Quantum Bayesian papers still have valuable portions, which are nearly obsolete, which are by authors who may sympathize with QBism without fully subscribing to it, and so forth. (These constraints have also inhibited enumerating those articles which fail to distinguish CFS 2002 from genuinely QBist sources, which ones seem to ignore all more recent developments and take CFS 2002 as the latest word in “informational interpretations”, etc.) And among the more leisurely portrayals of QBism, the book by von Baeyer [22] was pitched to the general pop-science audience, making it ill-suited to address the “inside baseball” matters like different schools of Bayesianism. Apart from a brief note in the context of a trendy but confined discussion [23], I myself have not drawn a hard line between QBism and the more amorphous Quantum Bayesianism that came before it, thinking in my innocence that the progression of thought was dramatic enough that it did not need pointing out. Correcting this deficit — and atoning, in part, for my blithe naivété — turns out to be an educational exercise. This is the second time I have marshalled historical evidence to show that a well-cited work was not in fact QBist, despite third-party claims to that effect [24]. From the viewpoint of QBism’s $h$-index, this must appear a quixotic or even self-destructive effort, but integrity is never easy.

I. LOCATING QBISM

Statements about probability have been given many different interpretations over the years. One sect would read an equation like “$p = 0.7$” as a claim about relative frequency in a large ensemble, while another would like to take it as concerning the extent to which a proposition follows logically from evidence. The Bayesian tradition regards probabilities as quantities asserted by gamblers, and it is within this tradition that QBism situates itself. Each of these intellectual genera contains many species. Within Bayesianism, one might mandate that in principle, all probabilities should reduce to 0 or 1 — maximal and complete information must resolve all uncertainty. This is the spirit we find, for example, in Jaynes or Garrett [25]. Another aspiration has it that within each physical situation, there dwells something like a chance density or a ratio of up to down probabilities, so that any gambler
aware of the value of that “objective chance” must set her odds to the exact figure it implies. This is difficult, perhaps impossible, to make logically self-consistent or to integrate with known quantities in physics; in day-to-day work, the postulation of such properties seems extraneous to scientific practice [17]. QBism instead follows the lead of personalist Bayesianism, a view historically associated with Ramsey and de Finetti [26, 27]. It eschews the probabilites and finds objectivity at a different level. For the QBist, no intrinsic attribute of a physical system can itself compel the outcome of a quantum measurement upon that system, nor even the probabilities that an agent should ascribe to the potential outcomes of that measurement before she performs it.

The research program of QBism does not content itself with providing a story for the familiar mathematical formalism of quantum theory. Nor is it satisfied with detailing how QBism differs from previous attempts to interpret the quantum — a harmless pastime for those who treasure the peace of library basements. Rather, the goal is to understand why that formalism is useful: Why quantum theory, as opposed to any alternative we might envision? QBism finds the exhortation to “shut up and calculate!” unstable against perturbations by curiosity: “Were the world a different way, would we not, after we shut up, calculate in a different fashion?” [28].

Three Greek-derived words are helpful in discussing interpretations of quantum mechanics. Ontic refers to entities and quantities that exist, in their own right, in blunt reality — in a Newtonian worldview, the mass of a rock is ontic. Epistemic quantities have the character of knowledge, while doxastic, from the Greek for “belief”, captures the personalist Bayesian view of probabilities, and thus the QBist interpretation of quantum states. Writing a wavefunction \( |\psi\rangle \) is staking out a doxastic claim, though the fact that it has proven useful to use vectors in complex Hilbert spaces to express our doxastic statements has an ontological lesson subtly coded within it.

QBism is largely orthogonal to matters of “Bayesian inference” as understood in statistics or big-data science [1, §9]. Attempting to grasp what QBism is about by extrapolating a Google University education in those subjects has led more than a few poor souls into confusion, whether they recognize it or not.

The remainder of this article will be devoted to identifying the differences between QBism and what we might call “proto-QBism”, the views articulated by Fuchs, Schack and coauthors in the 1990s and early 2000s. We will begin with the CFS 2002 paper mentioned above, which in my informal experience has been most commonly confused with QBism proper, and which provides a rather nice contrast with it. We will follow that with a close study of a follow-up article that Caves, Fuchs and Schack wrote a few years later, which as we will see still is not QBism. We will then backtrack and examine older writings of Fuchs himself that are less widely invoked, and whose distance from QBism is in certain aspects almost shocking. Our final exhibit will be a position statement that Fuchs and Asher Peres wrote for Physics Today. My hope is that revealing this history may help explicate why QBism developed as it did, and that it may aid those displeased with QBism to be unhappy with QBism itself instead of a confabulation.

II. CAVES–FUChS–SCHACK (2002)

CFS 2002 makes a case that quantum probabilites should be regarded as Bayesian probabilities, but it doesn’t do much more than that — at least, not very well, and from a QBist perspective, not convincingly.
The ceaseless and uncritical invocation of “maximal information” in CFS 2002 is legitimately grating to a QBist ear. At best, one can wincingly try to find a reading where it is tautological, treating the statements Alice has maximal information about a system $S$ and Alice ascribes a pure quantum state to system $S$ as wholly synonymous. But one would be cruelly paid back for such generosity. The years have taught us that it is just not possible to shake the connotations of “maximal information” — connotations of pre-existing properties, of the “ontic states” that a more timid view would want to underlie quantum theory. As Fuchs and Schack would write much later \[2\],

The trouble with the phrase “maximal information is not complete” and the imagery it entails is that, try as one might to portray it otherwise (by adding “cannot be completed,” say), it hints of hidden variables. What else could the “not complete” refer to?

Therefore, we must read CFS 2002 through the prism of this later repudiation of it. Section IV begins,

Our concern now is to show that if a scientist has maximal information about a quantum system, Dutch-book consistency forces him to assign a unique pure state. Maximal information in the classical case means knowing the outcome of all questions with certainty. Gleason’s theorem forbids such all-encompassing certainty in quantum theory. Maximal information in quantum theory instead corresponds to knowing the answer to a maximal number of questions (i.e., measurements described by one-dimensional orthogonal projectors).

The language about “knowing the answer to a maximal number of questions” is redolent of ideas that QBism learned after much scrutiny to leave behind. These ideas go by names like “the eigenstate-eigenvalue link” and “the EPR criterion of reality”; their underlying, unexamined premise is that a probability-1 prediction equates to an objective, agent-independent physical truth.

How could we ever reconcile this language with the QBist insistence that a quantum measurement does not simply read off a pre-existing physical quantity? It just doesn’t work. Every instance of “maximal information” in CFS 2002 is an ironic echo of the traditional mantra in a scientist’s concluding remarks: When it came to the matter of conceptual consistency, further research was needed.

“Knowing the answer to a maximal number of questions” applies to the Spekkens toy model, by construction \[29\]. In this model, the fundamental atom is a system with four possible physical states. The observer is restricted never to know more than one bit of information about an atom that would require two bits to describe fully. It follows that there are three possible binary-valued tests that the observer can perform on an atom, but no state of knowledge can allow the answer to more than one of them to be foreseen. Consequently, the posit that “Maximal information ... corresponds to knowing the answer to a maximal number of questions” does not really get at anything uniquely quantum at all \[29\].

QBists have argued that the restriction in certainty is not fundamental, but rather derived. In order to explore this point, we must do something that is doubtless anathema in philosophical circles: discuss new technical developments rather than old terminology. The crucial idea is the concept of a reference measurement \[30\]. Now that the kilogram is no longer defined as a particular lump of platinum-iridium alloy \[31\], there is room in the vault
for a Bureau of Standards quantum measurement device. Consider a physicist Alice who has a qubit system in her possession. She can go to the Bureau of Standards and drop her system into the standard measurement device for qubits. Such a device must have at least four possible outputs that it can generate; that is, Alice’s mathematical representation of it must be a POVM with at least four elements. Let \( p(H_i) \) denote Alice’s probability for obtaining outcome number \( i \). Suppose that she intends to perform some other measurement \( \{E_j\} \): perhaps a von Neumann test corresponding to an orthonormal basis, or perhaps a trine POVM [32], or even a “noisy icosahedron” POVM relevant to the theory of exceptional Lie algebras [33]. Let \( r(E_j|H_i) \) denote her probability for eliciting outcome \( j \) in this other experiment given that she has performed the Bureau of Standards reference POVM and obtained outcome \( i \) in it first. Quantum theory then furnishes the tools for Alice to compute her probability \( q(E_j) \) for eliciting outcome \( j \) in this other measurement without her carrying out the reference experiment first. The vector of these probabilities will be

\[
q = \mu(p, r),
\]

where \( \mu \) is some function that depends upon the details of the reference measurement. A simple and illuminating choice is to make the reference measurement a POVM that corresponds to a regular tetrahedron inscribed in the Bloch sphere. For example, letting \( a \) and \( b \) take the values \( \pm 1 \), then the four positive semidefinite operators

\[
H_{ab} = \frac{1}{4} \left( I + \frac{1}{\sqrt{3}} (a\sigma_x + b\sigma_y + ab\sigma_z) \right)
\]

sum to the identity and thus constitute a POVM. With \( p(H_{ab}) = \text{tr}(\rho H_{ab}) \) by the Born Rule and \( r(E_j|H_{ab}) = 2\text{tr}(E_j H_{ab}) \) by the Lüders Rule, we have

\[
q(E_j) = \text{tr}(\rho E_j) = \sum_{ab} \left[ 3p(H_{ab}) - \frac{1}{2} \right] r(E_j|H_{ab}).
\]

In this case, the function \( \mu \) takes the form of the classical Law of Total Probability but with an elementwise deformation of the probability vector \( p \). The reference measurement establishes a mapping from density matrices into probability vectors, thereby yielding a wholly probabilistic representation of the quantum theory of a qubit. Not all probability vectors \( p \) correspond to valid quantum states in this representation. In fact, with any minimal informationally complete (MIC) experiment as the reference POVM, the state space is mapped into a proper subset of the four-outcome probability simplex. No more than one entry in a valid probability vector \( p \) can be equal to zero. Or, geometrically speaking, the vertices of the reference probability simplex are unavailable. This has the character of an uncertainty principle: Alice’s state of expectation can only be so sharp. But this is just a consequence of a deeper truth, namely that because intrinsic “hidden variables” do not exist, one should not use the Law of Total Probability to intermediate between different experiments [34, 35].

Later, CFS 2002 gets to this problematic passage:

In the classical case an i.i.d. assignment is often the starting point of a probabilistic argument. Yet in Bayesian probability theory, an i.i.d. can never be strictly justified except in the case of maximal information, which in the classical case implies certainty and hence trivial probabilities. The reason is that the only way
to be sure all the trials are identical in the classical case is to know everything about them, which implies that the results of all trials can be predicted with certainty [Jaynes [36]].

Note that the citation supporting this argument is to E. T. Jaynes’ unfinished textbook [36]. A QBist naturally asks, “If my probabilities really are mine, then who’s to stop me from choosing an i.i.d. prior? Experience may lead me to revise my beliefs away from that prior, but I have every right to assert it in the first place.” One could square the argument against the legitimacy of classical i.i.d. priors with a Jaynesian view, but ultimately not with a Ramseyan one. Saying “to be sure all the trials are identical” amounts to saying “to be sure the probabilities are physically equal”. The argument in CFS 2002 is a relic of an objective-Bayesian interpretation. One way to express this shift of interpretation is to say that in CFS 2002, probabilities are epistemic (about knowledge), while in QBism proper, they are doxastic (about belief). CFS 2002 is saying that an i.i.d. prior is only justified when the ratio of up to down probabilities is constant across all the trials, and that is just not a kind of Bayesianism that QBism can endorse.

CFS 2002 declares (italics in original),

Since one of the chief challenges of Bayesianism is the search for methods to translate information into probability assignments, **Gleason’s theorem can be regarded as the greatest triumph of Bayesian reasoning.**

From a QBist perspective, this is peculiar. Gleason’s theorem proves that it is possible to chop off part of the standard formalism of quantum theory and then re-grow it from the remainder [37]. More specifically, Gleason showed that if measurements correspond to orthonormal bases on a Hilbert space, and if the probability of a measurement outcome does not depend upon which basis the corresponding vector is embedded in, then any consistent way of assigning probabilities to measurement outcomes has to take the form of the Born rule. Thus, if \( \Pi \) is a projection operator and \( p(\Pi) \) is the probability ascribed to obtaining the outcome corresponding to \( \Pi \), then we must have

\[
p(\Pi) = \text{tr}(\rho \Pi)
\]

for some density matrix \( \rho \). Both the set of valid \( \rho \) and the rule for what to do with a \( \rho \) come tumbling out of Gleason’s insight. This is of course pertinent to the project of reconstructing quantum theory, a task to which much QBist and QBist-adjacent effort has been devoted — but Gleason’s theorem itself has barely figured in that effort. Why? One reason is that the premises of Gleason’s theorem are themselves rather late in the game: Gleason’s starting point is a Hilbert space and orthonormal bases upon it. The natural question is thus how to arrive at Hilbert space — out of all the mental contrivances that the mathematicians have conjured, why that very particular class of structure? The stated goal of the reconstruction project in which Fuchs and others have participated is to derive complex Hilbert space, linear operators, the space of valid quantum states and all the rest of the formalism from principles that are more deeply rooted. In that light, Gleason’s theorem is more a proof of principle, a historically significant demonstration that the machinery can be taken apart and rebuilt, rather than “the greatest triumph” of anything.

Busch [38] — and, later, independently Caves et al. [39] — proved an analogue of Gleason’s theorem in which POVMs are the basic notion of measurement.\(^1\) Gleason’s original

\(^1\) Busch discovered this result while salvaging von Neumann’s attempt at a no-hidden-variables proof. The
theorem fails for two-dimensional Hilbert spaces. The essential reason is that in two dimensions, one cannot hold one vector of a basis in place and twirl the rest of the basis around to generate multiple distinct measurements. This difficulty does not apply to the POVM version of the theorem. The question of which classes of measurements allow the proof of a Gleason-type theorem continues to be studied [45–47]. For the present purposes, we need only note that the emphasis on orthonormal bases is another way that CFS 2002 does not read at all like the genuinely QBist writings of Fuchs and Schack.

In my experience, Gleason’s theorem is unfamiliar to many physicists, and when they learn of it, they may find it unsatisfying. This warrants a moment of consideration. I suspect that they want a story about energy flow, thermalization, a phase transition manifesting as symmetry breaking. (I went to physicist school too!) But here we have no initial coarse approximation by dimensional analysis, no semi-heuristic judgments about the comparative strengths of different couplings. The state space and the Born Rule just fall out of the geometry, like a regular pentagon from a nest of construction lines. The emotional reaction is to say that there is no physics in it.2 But the math — intricate, laborious, much simplified by POVMs — does work. The lesson, to a certain mindset, is that pursuing a “physics answer” in the pedestrian sense is redundant, needless, an effort better spent elsewhere.

Next, we consider the interpretation that CFS 2002 places on the quantum de Finetti theorem [52–55]. This is a quantum analogue of the de Finetti theorem in classical probability theory, which provides a viable meaning for the term “unknown probability” in a subjectivist, or personalist, form of Bayesianism. Consider a scenario in which an agent wishes to conduct a long experiment, made up of many successive trials. We can represent the outcome of each trial by a random variable \( x_j \), and Alice assigns a joint probability distribution \( p(x_1, x_2, \ldots, x_N) \) over the possible outcomes of an \( N \)-trial experiment. Imposing two conditions on this joint distribution turns out to simplify its form dramatically. First, we require that it be finitely exchangeable: Its value is invariant under permutations of its indices. If \( \pi \) is any permutation of the indices \( \{1, \ldots, N\} \), then

\[
p(x_1, \ldots, x_N) = p(x_{\pi(1)}, \ldots, x_{\pi(N)}).
\]

Second, we require that Alice’s \( p(x_1, \ldots, x_N) \) be extendable, in the following manner. For

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structure of POVMs allows one to make an additivity condition that replaces the unwarranted assumption in von Neumann’s argument, the postulate that had been criticized by Hermann and probably by Einstein [40–44].

2 Zurek [48] calls Gleason’s proof “rather complicated”. True, but it’s no Four Color Theorem, and a profession that boasts of using over 6300 tenth-order Feynman diagrams to compute the electron magnetic moment [49] has no right to complain about anything that is merely “rather complicated”. Zurek further says that Gleason “provides no motivation why the measure he obtains should have any physical significance — i.e., why should it be regarded as probability”. This exchanges cart and horse. The ethos of Gleason’s theorem is to start with structures that do not look probabilistic in character (Hilbert spaces and orthonormal bases, or orthomodular lattices if you’re a quantum logician [50, 51]). Then one says that to be meaningful, a physical theory must at least make statistical predictions. One introduces Gleason’s “frame functions” because one is motivated to find probabilities. And thus, all the structures that follow naturally inherit a probabilistic character, including the set of pure states. Of course, this clashes with the widespread gut-level conviction that a state vector \( |\psi\rangle \) simply cannot be probabilistic in nature, and thus with Zurek’s more specific claim that symmetries within a \( |\psi\rangle \) are somehow “objective”. Most arguments in quantum foundations are, ultimately, gastric distress.
any integer $M > 0$, there must be a finitely exchangeable distribution with more arguments, $p_{N+M}$, such that
\[
p(x_1, \ldots, x_N) = \sum_{x_{N+1}, \ldots, x_{N+M}} p_{N+M}(x_1, \ldots, x_N, x_{N+1}, \ldots, x_{N+M}).
\] (6)

These two requirements make precise the idea that Alice’s probability assignment $p$ derives from an arbitrarily long sequence of random variables, the order of which is, in Alice’s judgment, inconsequential. We say that a $p$ which satisfies both conditions, finite exchangeability and extendable, is exchangeable.

Let $\Delta_k$ denote the space of valid probability assignments over $k$ outcomes. Then, the classical de Finetti theorem shows that exchangeability implies that
\[
p(x_1, \ldots, x_N) = \int_{\Delta_k} d\vec{p} P(\vec{p}) p_{x_1} \cdots p_{x_N} = \int_{\Delta_k} d\vec{p} P(\vec{p}) p_1^{n_1} \cdots p_k^{n_k},
\] (7)
where $P(\vec{p})$ is properly normalized over $\Delta_k$:
\[
\int_{\Delta_k} d\vec{p} P(\vec{p}) = 1.
\] (8)

The quantum version replaces an integral over the probability simplex $\Delta_k$ with an integral over quantum state space, furnishing a representation of exchangeable quantum-state ascriptions:
\[
\rho^{(N)} = \int d\rho P(\rho) \rho^{\otimes N}.
\] (9)

Just as the classical de Finetti theorem revealed how the term “unknown probability” is merely a convenient shorthand, so does the quantum de Finetti theorem for “unknown quantum state”.

Having established this background, we turn to the following passage from CFS 2002:

Exchangeability permits us to describe what is going on in quantum-state tomography. Suppose two scientists make different exchangeable state assignments and then jointly collect data from repeated measurements. Suppose further that the measurements are “tomographically complete”; i.e., the measurement probabilities for any density operator are sufficient to determine that density operator. The two scientists can use the data $D$ from an initial set of measurements to update their state assignments for further systems. In the limit of a large number of initial measurements, they will come to agreement on a particular product state $\hat{\rho}_D \otimes \hat{\rho}_D \otimes \cdots$ for further systems, where $\hat{\rho}_D$ is determined by the data. This is what quantum-state tomography is all about. The updating can be cast as an application of Bayes’s rule to updating the generating function in light of the data [Schack, Brun and Caves [56]]. The only requirement for “coming to agreement” is that both scientists should have allowed for the possibility of $\hat{\rho}_D$ by giving it nonzero support in their initial generating functions.

QBism refuses to go down this path. Indeed, it balks at the first step. “Suppose two scientists make different exchangeable state assignments and then jointly collect data from repeated measurements” — no, we’re stopping right there. QBism insists that measurement outcomes are personal to the agent who elicits them.
A QBist take on the quantum de Finetti theorem puts all the state assignments into a single user’s internal mesh of beliefs. Alice supposes that tomorrow, she will make a multipartite, exchangeable state ascription, perhaps $\rho_1^{(N)}$ or perhaps $\rho_2^{(N)}$. Using the quantum de Finetti theorem, she represents the first joint state as a “meta-probability density” $P_1(\rho)$:

$$\rho_1^{(N)} = \int d\rho P_1(\rho) \rho^{\otimes N},$$

and likewise for $\rho_2^{(N)}$. If the density functions $P_1(\rho)$ and $P_2(\rho)$ have at least a little agreement, Alice can expect that her initial choice will wash out. That is, her expectation now for her future mesh of beliefs is that the choice between the ascriptions $P_1(\rho)$ and $P_2(\rho)$ will eventually become inconsequential. So, there is definitely a story to be told about the mathematics, perhaps a rather important one, but it is not the story given in CFS 2002.3

Recalling Fuchs’ three distinguishing characteristics of QBism, it is difficult to argue that they are present in CFS 2002. The “formal structure” is not reshaped in look or in feel, merely propped up by a couple extant theorems, one of them known since 1957. Two levels of radical personalism are not present — there is only one, and it is but tepidly embraced, while the other is flatly contradicted. The third characteristic, the “start of a great journey”, receives an endorsement of sorts in the paper’s send-off, but one so disconnected from everything that went before, it reads more like an afterthought than an outlook.

III. CAVES–FUCHS–SCHACK (2007)

Caves, Fuchs and Schack moved away from some positions held in their 2002 paper just a few years later. “Subjective probability and quantum certainty” ([13], hereinafter CFS 2007) is the last work coauthored by all three of them, since as that paper was being written and published, it became clear that Caves disagreed with Fuchs and Schack on various points that are essential to Fuchs and Schack’s further development of QBism.4 In their introduction, they state the following:

In a previous publication [CFS 2002], the authors were confused about the status of certainty and pure-state assignments in quantum mechanics and thus made statements about state preparation that we would now regard as misleading or even wrong.

Discussions leading up to this change of heart can be found in [3, pp. 193 ff.].

There is less material in CFS 2007 that is overtly contra-QBist than there is in CFS 2002, yet Fuchs’ later warning covers it as well [59]:

The present work, however, goes far beyond those statements in the metaphysical conclusions it draws—so much so that the author cannot comfortably attribute the thoughts herein to the triumvirate as a whole. Thus, the term QBism to mark some distinction from the known common ground of Quantum Bayesianism.

3 For a pedagogical introduction to the “probabilities for future probabilities” thinking, see [57, §5.1]. For a technical result motivated by “expected changes in expectation” concerns, see [58].

4 Caves has confirmed to me (e-mail, 24 November 2019) that he does not subscribe to QBism.
One would think that this hazard sign would prompt a degree of caution to be taken before treating all the citations in a list of “Quantum Bayesian” papers on equal footing.\(^5\) We can see a trace of a divergence already manifesting in this aside:

Bayesian updating is consistent, as it should be, with logical deduction of facts from other facts, as when the observed data \(d\) logically imply a particular hypothesis \(h_0\), i.e., when \(\Pr(d|h) = 0\) for \(h \neq h_0\), thus making \(\Pr(h_0|d) = 1\). Since the authors disagree on the implications of this consistency, it is fortunate that it is irrelevant to the point of this paper. That point concerns the status of quantum measurement outcomes and their probabilities, and quantum measurement outcomes are not related by logical implication.

CFS 2007 is evasive on the question of whether measurement outcomes are personal to the agent who elicits them. They write of “the facts an agent acquires about the preparation procedure”, and they say, “The occurrence or nonoccurrence of an event is a fact for the agent.” But if the occurrence or nonoccurrence of an event is a fact for everybody, it is a fact for a specific agent too. Without forthrightly clarifying this point, CFS 2007 does not qualify as QBist. Indeed, CFS 2007 rather undermines the point, with loose talk about “two agents starting from the same facts, but different priors” and the like. A QBist description would instead involve a single agent, considering the same set of quantitative data points with either of two background meshes of belief (compare [61, 62]).

Before moving on, we note that CFS 2007 briefly discusses the Lewisian “objective chance” philosophy. Fuchs would shortly thereafter find this discussion weak enough to be irrelevant, or potentially even counterproductive [3, p. 1287]. Overall, CFS 2007 is the product of too many compromises among its three authors to fairly reflect the view of any of them.

IV. THE FIRST SAMIZDAT

Notes on a Paulian Idea (2003), later reissued as Coming of Age with Quantum Information (2011), is an edited collection of e-mail correspondence that Fuchs made public to “back up the hard drive” after the Cerro Grande fire destroyed his family’s home in Los Alamos [63]. My colleagues and I have elsewhere [30] quoted passages from this document to show how later research has fulfilled their aspirations almost to the letter. Here, I take the opposite tack.

One theme that is quite surprising to a reader familiar with QBism proper is how frequently Fuchs insists upon multiple agents being necessary to reveal the hidden ontological lesson of quantum theory. This stands in stark contrast to the “I-I-me-me-mine!” declaration of Fuchs’ later manifesto [59]. In addition, the stories of multiple agents intermingle with the theme that the deep physical principle of quantum theory is captured by information-disturbance tradeoffs. This idea had vanished by the time QBism proper was articulated.

\(^5\) Indeed, such a list of predecessors occurs in Fuchs and Schack’s Reviews of Modern Physics article [2]. Cursory inspection reveals that the views tallied there should not be identified with QBism or with each other. For example, the list includes Appleby’s “Facts, Values and Quanta” [60], which takes pains to remain distinct from the other Quantum Bayesian writings of that period: “I should say that I do not entirely agree with them about that. […] My feeling is that a completely satisfactory theoretical account has yet to be formulated.” And so forth.
We can point to multiple reasons why it could easily have fallen by the wayside: The quantitative expressions of it started out dense and not too illuminating \cite{64,65}, and despite some promising indications \cite{66}, they never really simplified.\footnote{Asks Fuchs in \cite{66}, “Why is the world so constituted that binary preparations can be put together in a way that the whole is more than a sum of the parts, but never more so than by \( Q \approx 0.202 \) bits?” Note that the bound of \( Q \approx 0.202 \) bits is attained when the two alphabet states are drawn from two different MUB; while by another measure of “quantumness” in that paper, the average global fidelity, two qubit states are “most quantum with respect to each other” when they are drawn from a SIC.} Moreover, the basic phenomenon of “no information gain without disturbance” ended up being too easy to reproduce in theories with underlying local-hidden-variable models.\footnote{True, one might be able to establish a difference in the precise shape of the tradeoff curves (compare \cite{67}), but that is too slender a reed to hang the distinction between quantum and classical upon. Like NASA, at the critical juncture we want a declaration that is go/no-go.}

In an 18 September 1996 letter to David Mermin, we find an early occurrence of the slogan that Fuchs and Schack would later spurn:

One always assigns probabilities based on incomplete information; it is just that in quantum physics “maximal information is not complete.”

We find this again in another letter to Mermin, this one dated 23 July 2000:

The theory prescribes that no matter how much we know about a quantum system—even when we have maximal information about it—there will always be a statistical residue. There will always be questions that we can ask of a system for which we cannot predict the outcomes. \textit{In quantum theory, maximal information is not complete and cannot be completed.}

Even sentences that were blessed with italics can turn out to be quite wrong-headed.

We can uncover early intimations of the QBist desire to find in quantum theory an ontological lesson, without naively identifying the elements in the mathematical formalism with an ontology. However, the place and manner of the search is not yet QBist. From a 4 January 1998 letter to Greg Comer:

The “fact” that \textit{my} information-gathering yields a disturbance to \textit{your} predictions is the only “physical” (or ontological) statement that the theory makes; all the rest of the structure is “law of thought” subject to that consideration. To put it another way, quantum theory is a theory of “what we have the right to say” in a world where the observer cannot be detached from what he observes. It is that and nothing more.

And again on 22 April 1999:

Our experimentation on the world is not without consequence. When \textit{I} learn something about an object, \textit{you} are forced to revise (toward the direction of more ignorance) what you could have said of it.

Likewise, in a 30 August 1998 letter to Adrian Kent:
Suppose in a few years I could come up with a clear, precise statement of what it is that I’m trying to get at. Something in essence that takes away the vagaries of the statement: “The world in which we live happens to have a funny property. It is that my information gathering about something you know, causes you to lose some of that knowledge … and this happens even in the case that you know all my actions precisely. Physical theory, and quantum mechanics in particular, is about what we can say to each other and what we can predict of each other in spite of that funny property.” Would that constitute something that fulfills your Desideratum #1?

More concisely, in a letter on 2 September 1998:

Disturbance to what? To each other’s descriptions, nothing more.

The themes of information tradeoffs and multiplicity of agents are developed rather extensively in correspondence with David Mermin. On 8 September 1998:

It is crucial to my point of view that there be at least two players and two quantum states in the game. […] I think one of the troubles in our founding fathers’ discussions is that they continually focused their attention on one observer making measurements on a quantum system described by one (known) quantum state. This led them to say things—in language similar to some of the specimens in your note—like, “The gain of knowledge by means of an observation has as a necessary and natural consequence the loss of some other knowledge.” (Pauli) Without at least a second player in the game, those gains and losses hardly seem to be sensible concepts to me: they can only refer to the observer’s attempt to ascribe one or another classical picture to the quantum system in front of him. Since we know—from Bell’s argument and the religion of locality—that it is not reasonable to assume that those classical variables (correlata) are there and existent without our prodding, it is hard to call the revelation of a measurement outcome a “gain of knowledge.” What did you learn about the world that was there before your looking? Nothing. However, throw a second player into the game and that situation changes. Those random quantum outcomes now have something existent, some unknown truth, that they can be correlated with. The revelation of an outcome really can correspond to a “gain of knowledge,” but you need at least two information processing units in the world for that to be the case.

And, shortly thereafter in the same note,

In your original Ithaca paper you speak of the minimal requirements for a quantum mechanical universe: it is, you say, two qubits—two things to have correlation without correlata. I, however, am more afraid to go that far, i.e., to some final/overarching ontological statement. Instead, the most I think I’m willing to ask is, “What are the minimal requirements for a physical theory?” And there, I think the answer is two “theory makers” and a physical system.

More dramatically still, on 20 July 2000:
Somehow I feel that I had an epiphany in Mykonos. Do you remember the parable of “Genesis and the Quantum” from my Montréal problem set? And do you remember my slide of an empty black box with two overlays. The first overlay was of a big $|\psi\rangle$ (hand drawn in blue ink of course). I put the slide of the box up first, and said, “This is a quantum system; it’s what’s there in the world independent of us.” Then I put the first overlay on it and said, “And this symbol stands for nothing more than [what] we know of it. Take us away and the symbol goes away too.” I then removed the $|\psi\rangle$. “But that doesn’t mean that the system, this black box, goes away.” Finally I put back up the $|\psi\rangle$ over the box, and the final overlay. This one says: “Information/knowledge about what? The consequences of our experimental interventions into the course of Nature.”

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

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

The number of agents is getting out of hand — not just an Alice, with whom a QBist narrative would content itself, but a Bob and now an Eve. This is quite the dramatic excess in the light of QBism’s single-user focus (a later development codified in response to drilling down on the issue of Wigner’s Friend [3, p. xlii]).

“What, four? thou saidst but two even now.”

“Four, Hal; I told thee four.”

— 1 Henry IV, 2.4

We do see a backing-away from some early choices of terminology, in a 1 July 2000 letter to Hideo Mabuchi:

I’m in Greece right now, just finished with the NATO meeting. Tomorrow morning I leave for Capri (the QCMC conference), and then finally join Kiki in
Munich at the end of the week. My talk was pretty successful in Mykonos; I was pretty happy with it. For Capri I’m going to make a completely new one, this time based on the stuff I did with Kurt Jacobs. I’ve decided the best way to say what I’ve been hoping to get at. Question: “If the wavefunction isn’t real, then what is it that IS real about a quantum system?” Answer: “The locus of all information-information tradeoff curves that one can draw for such a system.” (I’ve decided to stop calling it information-disturbance because it conveys bad imagery and preconceptions. The disturbance is to information, so why not just make it explicit.)

In retrospect, this prefigures Fuchs and Schack’s later excommunication of the “maximal information is not complete” slogan — but only in retrospect. It is still locked into an epistemic mindset, rather than a doxastic one.

In an 11 December 2000 letter to Joseph Renes:

Why am I so obsessed with always having two players in the game? Because I want to connect all the concerns in quantum mechanics with Bayesianism as much as I can.

At this point in time, Fuchs pretty explicitly takes the convergence among agents as the meat of Bayesianism, rather than making the fundamental point the normative principle of consistency within a single agent’s mesh of beliefs, with inter-agent agreement a secondary notion (when it can meaningfully be defined at all).

V. FUCHS (2002)

Having grounded ourselves in Fuchs’ less formal solo-author writings from the late 1990s, we are now in a better position to examine “Quantum Mechanics as Quantum Information (and only a little more)” [68]. I have deferred discussion of this essay until now, because much of it is technical development, and the conceptual passages where it comes off most strongly non-QBist are best appreciated after becoming familiar with the Notes on a Paulian Idea era. Of the three distinguishing features of QBism, we can discern the “reliance on the mathematical tools of quantum information theory to reshape the look and feel of quantum theory’s formal structure”, at least in a preliminary way. And, not bound by the length and style constraints of a journal article’s “Conclusions” section, Fuchs takes the opportunity to press the “start of a great journey” theme. But there is still only one level of personalism: Probabilities are personal, but experiences are not. A brief excerpt suffices to show that Fuchs attempts to launch the “great journey” just as he did at Mykonos:

The wedge that drives a distinction between Bayesian probability theory in general and quantum mechanics in particular is perhaps nothing more than this “Zing!” of a quantum system that is manifested when an agent interacts with it. It is this wild sensitivity to the touch that keeps our information and beliefs from ever coming into too great of an alignment. The most our beliefs about the potential consequences of our interventions on a system can come into alignment is captured by the mathematical structure of a pure quantum state $|\psi\rangle$. Take all possible information-disturbance curves for a quantum system, tie them into a bundle, and that is the long-awaited property, the input we have been looking for from nature. Or, at least, that is the speculation.
The first sentence would read fine coming from a QBist, but the rest goes barrelling down a blind alley.

The technical discussions hold up rather better, and the paper is noteworthy as an early example of the MIC-as-reference-measurement idea. The particular class of MIC it discusses, later designated the orthocross MICs, still has some open conjectures about it [69].

Fuchs gives an argument for why the tensor product rule for composing state spaces follows from Einstein locality and a Gleason-type context-independence condition. This proof may be of significance to a category theorist [70, §2.3], as it deduces the tensor product from the requirement that the functions of interest be linear on both halves of the composite system. However, it does go somewhat against the grain of later reconstruction work with Schack and others. Those efforts focused on deriving the state and measurement spaces of a single system, which can then be resolved into components if desired. In other words, the emphasis shifted from composition to decomposition.

The 2002 paper leaves open the question of why the joint states for a bipartite system should be specifically positive semidefinite operators on the tensor-product space. The later literature provides at least one answer to this question [71, 72], but at the cost of assumptions that may feel unsatisfying on account of being physically under-motivated or mathematically over-powered. (For example, why in the grand scheme of things should the set of entangled pure states form a continuum?) There may yet be a theorem or two worth proving here.

Examining the motivations interleaved between the equations, we find another conceptual issue that marks the 2002 paper as not yet QBist. It is the distinction between doxastic consistency conditions and update rules, a point that Fuchs and Schack did not fully resolve until the better part of a decade later [73]. Quoting [1],

> Adopting a personalist Bayesian interpretation of probability does not mean treating all changes of belief as applications of the Bayes rule. This is shocking to some people! And distancing ourselves from the dogmatists who claim to follow that creed is one reason why we prefer QBism over “Quantum Bayesianism”.

In the tradition of Ramsey, Savage and de Finetti, there are consistency conditions that an agent’s probability assignments should meet at any given time, and then there are guidelines for updating probability assignments in response to new experiences. Going from the former to the latter requires making extra assumptions — the two are not as strongly coupled as many people think. The Bayes rule is not a condition on how an agent must change her probabilities, but rather a condition for how she should expect that she will modify her beliefs in the light of possible new experiences. For this observation, we credit Hacking, Jeffrey and van Fraassen.

Fuchs’ writing in 2002 had not yet distinguished the crucial gap between a rule for how Alice must change her beliefs and a criterion for how Alice should expect today that she will act tomorrow.

This is a slip-up we encounter now and then in conversations with people who have only heard a little about QBism, usually secondhand. (Other confusions — “But how does QBism explain X?” — typically occur when the interlocutor has unwittingly switched from subjective probability to objective, or from a first-person perspective to third-person, midway through a thought process. These are habits which take discipline to avoid, at least at first.) Evading this mental trap is another good reason not to take Fuchs’ 2002 salvo as a definitive, genuinely QBist position statement.
VI. FUCHS–PERES (2000)

On occasion, we have seen CFS 2002 cited on its own to define QBism (for example, in [74, 75]). A similar yet more egregious misattribution occurs in an article by Jaeger [76], which equates QBism with the 2000 Physics Today piece coauthored by Fuchs and Peres, “Quantum Theory Needs No ‘Interpretation’” [77]. I must regretfully report that Asher Peres was no QBist.

The specific point at which Jaeger elides the difference between QBism and fin-de-siècle Fuchs–Peres is the following, which attributes an opinion of the latter to the former:

One QBist claim is that “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 consequence 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 this is not a QBist claim. Of course, it predates QBism chronologically, but also, it contradicts what QBism actually stands for, and in a series of rather blunt and obvious ways. It is helpful here to quote a letter from Fuchs’ second samizdat [3, p. 1011], sent on 19 June 2005 to Greg Comer:

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.

So, “detector clicks” is misleading. Moreover, “macroscopic” is a red herring, a relic of earlier generations’ shifty grasp on what might differentiate quantum from classical. For example, an agent whose species has evolved eyes just a bit better than human ones might have seen individual photons flashing on a cold and lonely night. Such an agent might regard the direct personal experience of a single photon as a microscopic event, but they can employ the “user’s manual” that is quantum theory just as well as humans do. In brief, the micro/macro distinction is not, to a QBist, fundamental.

Would a QBist agree that a “strict definition of the scope of quantum theory is the only interpretation ever needed, whether by experimenters or theorists”? No, ever needed is all wrong there. Nothing that is all that’s ever needed can be the start of a great adventure.

The Fuchs–Peres collaboration has a very un-QBist reliance upon the first-person plural. As David Mermin has noted [78],

There is a little remarked upon but important ambiguity in the first person plural. When Heisenberg says that quantum states are about our knowledge, “our” can mean all of us collectively or it can mean each of us individually. […] To avoid ambiguity it is better to say “My (your, Alice’s) quantum state assignments encapsulate my (your, her) belief” to avoid misreadings based on implicit assumptions of a unique state assignment or of common knowledge.
The Fuchs–Peres essay has some affinities with the Rudolf Peierls opinion piece from a decade earlier [79] that they discussed during the writing process [63]. Mermin observes that Peierls would sound more QBist than perhaps any other figure from the early generations of quantum physicists, if he had not used the first-person plural collectively. Instead, he propagated the old confusions. This applies with equal force to the Fuchs–Peres collaboration.

Having dismissed “our”, “macroscopic”, “detector clicks”, “ever needed” and “does not describe physical reality”, what about “algorithm”? This, too, reflects an understanding that had not yet matured. An algorithm is a step-by-step procedure that can be executed mechanically. For example, taking the trace of the product of two matrices is a task for which an algorithm might be written. In that sense, computing a quantum-mechanical probability is algorithmic — but there is a deeper level of meaning, too, that the word algorithm misses. In QBism, a state vector $|\psi\rangle$ is not more ontologically fundamental than, say, the probability of getting a “+” outcome in a spin-$z$ experiment. True, quantum theory provides a rule for calculating $p_z(+) \text{ given } |\psi\rangle$, but when in life is one ever given a $|\psi\rangle$, other than the first line of a textbook problem demanding that the student “assume the state vector is $|\psi\rangle$”?

The deeper truth is that quantum theory provides a normative lesson: When Alice contemplates two or more von Neumann measurements upon a system, she should strive to make her expectations for those different, mutually exclusive scenarios all consistent with the Born Rule. But if she detects an inconsistency within her mesh of beliefs — if she finds that there is no density operator $\rho$ with which her varied probabilities are all in accord — the quantum formalism itself provides no algorithm to resolve that awkwardness [16].

The novice at any art often begins by following a procedure — say, the exact volume measurements and timings given by The Joy of Cooking, or the rubric for cranking through questions on the AP Physics exam. With further experience, one learns how to season for taste, what can be substituted for chicken or for eggs, how to linearize around the fixed points and so on. The procedures are always there to be relied upon when required — chopping a root or solving by radicals — but they are not the soul of the matter. So, too, for quantum theory: Algorithms are what we use, not the sum total of what we need.

Fuchs and Peres present a version of the Wigner’s Friend thought-experiment. In their portrayal, Erwin applies quantum mechanics to his colleague Cathy, who in turn applies it to a piece of cake. The Fuchs–Peres discussion is, from a QBist standpoint, rather unforgivably sloppy about the distinctions between ontic degrees of freedom, epistemic statements about ontic quantities and doxastic statements regarding future personal experiences.

I would not have imagined it possible to declare that the Fuchs–Peres opinion piece defines QBism, had I not seen it done in print.

VII. CONCLUSIONS

Basically nothing posted on the arXiv before 2009 should be cited as an example of QBism, no matter who the authors are. All the older writings fail in one or another readily

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8 At least, so it was in the year 2000. Nowadays, rather than the Knuthian sense of a procedure for a machine of known architecture, published so its performance can be analyzed, an algorithm is a trade secret that runs “in the cloud” and whose goal is to disguise injustice and inequality as objective logic [80]. O tempora, o mores.
apparent way to recognize at least one point that later investigation found to be necessary for a self-consistent interpretation of quantum mechanics. That said, various technical matters raised in those pre-QBist papers continue to be interesting even though the metaphysical frontier has left their original motivations far behind.

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You may already know what a blow to the ego it can be to have to read over anything you wrote twenty years ago, even cancelled checks.

— Thomas Pynchon

Looking back it is clear, but so much prevents you from seeing it.

— Autumn Kent