Von Neumann Was Not a Quantum Bayesian

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Wikipedia has claimed for over two years now that John von Neumann was the “first quantum Bayesian.” In context, this reads as stating that von Neumann inaugurated QBism, the approach to quantum theory promoted by Fuchs, Mermin and Schack. This essay explores how such a claim is, historically speaking, unsupported.

I. INTRODUCTION

The Wikipedia article on Quantum Bayesianism has claimed since April 2012 that John von Neumann was the “first quantum Bayesian” [73]. To a reader acquainted with quantum foundations and the history of quantum theory, this is a strikingly odd assertion. This note explains why the claim is incorrect and explores how it came to be made.

A “Quantum Bayesian” is one who interprets the probabilities arising in quantum physics according to some variety of the Bayesian view of probabilities. Given the profusion of schools of thought under the Bayesian umbrella, it should not be surprising that a variety of ways to be some kind of Bayesian about quantum theory has also arisen [3, 5, 11, 13, 18, 34, 36, 49, 50, 70–72, 75, 86, 103, 106]. The most radical approach is QBism, which maintains that all quantum states are expressions of personalist Bayesian probabilities about potential future experiences [5, 28–33, 54–58, 87, 92]. For this essay, I will take the writings of Fuchs, Mermin and Schack [28–33, 54–58] as the defining statements of what a QBist is and is not. Furthermore, their union and intersection are indicative of what a QBist might be, might not be and is not obligated to entertain.

QBism is the primary focus of the Wikipedia article on Quantum Bayesianism, and the take-away impression is that John von Neumann was not just a partisan of the Bayesian lifestyle, but also the first QBist. This is an untenable claim.

In this essay, we will not be strongly concerned with which interpretation of quantum mechanics is “correct,” or with what it might mean for an interpretation of quantum mechanics to be “correct.” Our focus will instead be on who said what and when. However, to evaluate the “von Neumann was the first Quantum Bayesian” claim properly, we need to clarify what a “Quantum Bayesian” world view might be, and QBism, in many ways an extreme among such views, provides a convenient vantage point. Therefore, we will establish the basic notions of QBism, and then in following sections we will turn to the writings of von Neumann.

II. QBISM

QBism is an interpretation of quantum mechanics which takes as fundamental the ideas of agent and experience. A “quantum measurement” is, in QBism, an act which 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 result of carrying out an action. This holds true for all quantum states, pure or mixed; a state without an agent is a contradiction in terms. Furthermore, each experience is a personal event specific to the agent who evokes it.
Different authors have emphasized different aspects of QBism. The discussions by Barnum [5] and Mermin [54–58], and the remark by Schlosshauer and Claringbold [80], place their focus on how QBism gives meaning to the current mathematical formalism of quantum theory. Fuchs and Schack have also addressed this aspect [30], while in addition pushing forward technical work which aims to reformulate quantum theory and build it up anew from explicitly QBist postulates [31]. Due to the historical subject matter of this essay, the former will be more relevant here.\(^1\)

QBism has been written up both in *New Scientist* [15] and in *Scientific American* [96], though not terribly accurately in either case, thanks to the editorial process [56–58]. A better treatment, albeit in German, appeared in the *Frankfurter Allgemeine Sonntagszeitung* [102]. *Nature* addressed it briefly in the context of information-oriented reconstructions of quantum theory [4]. Later, Mermin published in *Nature* an opinion piece promoting QBism [55], which was featured on the 27 March 2014 cover.

Taking the recent writings of Fuchs, Mermin and Schack as establishing what QBism is, we can identify some things which QBism is not.

- **A hidden-variable model.** Quantum states in QBism are probability distributions over potential experiences, not over values of putative hidden variables or agent-independent “ontic states.” QBism is more compatible with the research programme which uses hidden-variable models to reconstruct portions of quantum theory [6, 16, 17, 23, 84, 85, 95]. In these constructions, one posits a classical theory with discrete or continuous degrees of freedom, and then one imposes a restriction on what can be known about those degrees of freedom at any one time. The resulting statistical theory can qualitatively reproduce features of quantum mechanics (no-cloning and no-broadcasting theorems, teleportation and so forth); in some cases, it exactly reproduces a subtheory of quantum mechanics, including a subset of the states and operations available in the full theory. The phenomena not reproduced, such as the hope of computational speedup, are taken as indications of what is strongly nonclassical about quantum physics. The goal is to follow these hints to an interpretation of quantum mechanics as a statistical theory about something other than pedestrian hidden variables. A QBist is not philosophically obligated to find this research programme appealing, but it is much more aligned with QBism than attempts to reproduce all of quantum theory using hidden variables could be.

- **Solipsism.** According to QBism, quantum theory concerns the interface between an agent who uses quantum theory and the external world. Without the external world,

\(^1\) This is not to say that the more technical side of QBism is without historical and philosophical interest. The roots of the mathematics involved go back to Schwinger [83] and Weyl [104, §IV.D.14], indeed to the very transition from the “old quantum theory” to the new [32, pp. 2055–56, 2257–58, 2280]. And, in the SIC representation of quantum states and channels, the Born rule—usually written like \(p(i) = \text{tr}(\rho E_i)\)—and unitary evolution—typically written like \(\rho(t) = U_t \rho(0) U_t^\dagger\)—take the same form. Both are simple affine deformations of the Law of Total Probability [31]. This clarifies that both are *synchronic* relations between probability ascriptions [30]. Alice carries a probability distribution for an informationally complete measurement, which she uses to summarize her expectations. Alice can calculate other probability distributions from it, synchronically, including distributions for other informationally complete measurements which she might carry out in the distant future.
there would be no interface, no subject matter for quantum theory and, indeed, no science [31, 58]. Notwithstanding this, over the years QBism has been accused of solipsism; for examples and responses, see the list on pages xlv–xlvi of [32].

• “The Copenhagen Interpretation.” It is difficult to define what, historically speaking, a “Copenhagen Interpretation” should be. We will return to this problem in more detail later. For the moment, it suffices to take a specific reference which might be designated as Copenhagenish, the quantum physics volume of Landau and Lifshitz [48]. The position of Landau and Lifshitz differs from QBism in ways which Fuchs, Mermin and Schack have tabulated [29, 57, 58]. In particular, the notions of “classical object” and “classical ‘apparatus’” are central to Landau and Lifshitz’s interpretation, but not to QBism.

The ideas just itemized are incompatible with QBism: one who holds them is ipso facto not a QBist. We can also identify some things which QBism does not mandate. For example, a QBist does not have to expect that human beings, when tested in psychology laboratories, must follow a Bayesian decision-making scheme. Fuchs, Mermin and Schack all use a Dutch book argument which deduces the rules of probability from a normative requirement, and humans are very good at falling short of normative requirements.

On a more technical note, Fuchs and Schack demonstrate that we can view the Born rule as an empirical addition to the bare structure of probability theory [31]. The Born rule does not derive probabilities from some more fundamental kind of mathematical object; instead, it relates one probability distribution to another, tying together the probability assignments which an agent can consistently ascribe to different experiments. If one were enamored of a different physics-neutral scheme of learning—perhaps a system in which the quantities one updates in response to experience look very different from sequences of real numbers—then the Born rule would be an empirical addition to that structure, an extra consistency or coherence condition phrased in that theory’s terms. Thus, the use of personalist Bayesian probability theory itself may be secondary to a deeper physical principle; however, this line of thought has not been developed in any detail. And even if it were, some conceptions of probability, such as Lewisian objective chance, assert the existence of physical properties which are incompatible with QBism [31]. An alternative theory of learning would not erase that incompatibility.

Individual statements and arguments drawn from the writings of other scientists can sometimes fit neatly within the QBist programme. Examples come to mind in the works of Aaronson [1, pp. xii–xiii, 110], Bacon [2], Baez [3], Bell [57, 58], Einstein [33], Feynman [24, p. 6-7], Nielsen [63], Peierls [58], Schrödinger [55, 57, 58] and others. This is not to claim that any of these authors are QBist or proto-QBist (the latter term being also unpleasantly teleological). Physicists, physics and the history of physics are all sufficiently complicated that we cannot pigeonhole based on isolated snippets of text. Placement and classification require more systematic study than that, if they are to have any meaning. With this concern in mind, we turn to surveying the writings of von Neumann.

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2 A QBist could justifiably ask, “If there is no world outside of my head, where do all the papers on Bohmian mechanics keep coming from?”

3 In addition, QBism does not require that the process of science as a whole be understood as clockwork updating in accord with Bayes’ theorem [32, pp. 193, 500, 799, 1020, 1228, 1731]. The QBist investigations of when probabilistic coherence arguments are or are not in force [30, 31] militate against such a view.
III. VON NEUMANN ON PROBABILITY

A. Frequentism (1932)

To begin with, we examine von Neumann’s Mathematical Foundations of Quantum Mechanics, hereinafter MFQM. This book is indicative of von Neumann’s thinking in 1932, the time of the publication of the original German edition. Quoting from MFQM, page 298:

However, the investigation of the physical quantities related to a single object \( S \) is not the only thing which can be done – especially if doubts exist relative to the simultaneous measurability of several quantities. In such cases it is also possible to observe great statistical ensembles which consist of many systems \( S_1, \ldots, S_N \) (i.e., \( N \) models of \( S \), \( N \) large).\(^\text{156}\)

Note 156 reads as follows:

Such ensembles, called collectives, are in general necessary for establishing probability theory as the theory of frequencies. They were introduced by R. v. Mises, who discovered their meaning for probability theory, and who built up a complete theory on this foundation (cf., for example, his book, “Wahrscheinlichkeit, Statistik and ihre Wahrheit,” Berlin, 1928).

Solche Gesamtheiten, Kollektive gennant, sind überhaupt notwendig um die Wahrscheinlichkeitsrechnung als Lehre von den Häufigkeiten begründen zu können. Sie wurden von R. v. Mises eingeführt, der ihre Bedeutung für die Wahrscheinlichkeitsrechnung erkannte, und einen entsprechenden Aufbau derselben durchführte (vgl. z. B. sein Buch Wahrscheinlichkeit, Statistik und ihre Wahrheit, Berlin 1928).]

Notice the discrepancy in the titles given for von Mises’ book; apparently, the translator made an error here. In fact, von Neumann also errs in this passage, as the “ihre” is an interpolation. Nevertheless, the meaning of the passage is clear: in 1932, von Neumann interpreted probability in a frequentist manner.

Evidence of this occurs throughout MFQM, in fact. For example, “expectation value” is defined as “the arithmetic mean of all results of measurement in a sufficiently large statistical ensemble” (p. 308). Lüders, who improved upon von Neumann’s theory of measurement [52], also thought in terms of “an ensemble of identical and independent systems” [“einer Gesamtheit gleichartiger und unabhängig Systeme”]. This is one example of later researchers not finding a Bayesian message in von Neumann.

B. Probability as Extended Logic (c. 1937)

To see how von Neumann’s thinking on the foundations of probability changed, we turn next to an unfinished manuscript from about 1937, which is included in his Collected Works [101]. Von Neumann imagines a collection of a large number of “specimens” of a physical system \( S_1 \) and considers interpreting the transition probability \( P(a, b) = \theta \) in terms of a relative frequency:

\[
\text{If we measure on each } S_1^*, \ldots, S_N^* \text{ first } a, \text{ and then in immediate succession } b, \text{ and if then the number of those among } S_1^*, \ldots, S_N^* \text{ where } a \text{ is found to be true is } M, \text{ and the number of those where } a, b \text{ are both found to be true is } M', \text{ then:}
\]
(H) $P(a, b) = \theta$ means that $M'/M \to \theta$ for $N \to \infty$.

This view, the so-called "frequency theory of probability" has been very brilliantly upheld and expounded by R. V. Mises. This view, however, is not acceptable to us, at least not in the present "logical" context. We do not think that (H) really expresses a convergence-statement in the strict mathematical sense of the word—at least not without extending the physical terminology and ideology to infinite systems (namely, to the entirety of an infinite sequence $S_1^*, S_2^*, \ldots$)—and we are not prepared to carry out such an extension at this stage. The approximative forms of (H), on the other hand, are mere probability-statements, e.g. "Bernoulli's law of great numbers" [...] And such probability-statements are again of the same nature as the relation $P(a, b) = \theta$, which they should interpret.

Von Neumann then makes the following declaration:

We prefer, therefore, to disclaim any intention to interpret the relations $P(a, b) = \theta$ $(0 < \theta < 1)$ in terms of strict logics. In other words, we admit:

Probability logics cannot be reduced to strict logics, but constitute an essentially wider system than the latter, and statements of the form $P(a, b) = \theta$ $(0 < \theta < 1)$ are perfectly new and sui generis aspects of physical reality.

So probability logics appear as an essential extension of strict logics. This view, the so-called "logical theory of probability" is the foundation of J. N. [sic] Keynes's work on the subject.

In short, the later von Neumann interprets quantum probabilities as logical probabilities. Moreover, he explicitly identifies this view with that worked out by Keynes.

At this point, it is a good idea to compare Keynes' "logical probability" to the thinking of F. P. Ramsey, whose interpretation is closer to that invoked in QBism [32, pp. ix, 1225–29, 1374]. Fortunately, we have a statement by Keynes himself on this subject. In October 1931—after Ramsey's death at the age of twenty-six—Keynes wrote the following [46].

Formal logic is concerned with nothing but the rules of consistent thought. But in addition to this we have certain "useful mental habits" for handling the material with which we are supplied by our perceptions and by our memory and perhaps in other ways, and so arriving at or towards truth; and the analysis of such habits is also a sort of logic. The application of these ideas 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, having made this comparison, Keynes goes on to say,

So far I yield to Ramsey—I think he is right. But in attempting to distinguish "rational" degrees of belief from belief in general he was not yet, I think, quite
It is not getting to the bottom of the principle of induction merely to say that it is a useful mental habit. Yet in attempting to distinguish a “human” logic from formal logic on the one hand and descriptive psychology on the other, Ramsey may have been pointing the way to the next field of study when formal logic has been put into good order and its highly limited scope properly defined.

C. Debating Bohr in Warsaw (1938)

In 1938, von Neumann attended a conference in Warsaw on “New Theories in Physics.” The meeting, which ran from 30 May to 3 June, was attended by Bohr, Brillouin, de Broglie, C. G. Darwin, Eddington, Gamow, Kramers, Langevin, Wigner and others. Bohr presented a report on “The Causality Problem in Atomic Physics,” to which von Neumann replied in the discussion afterward [7]. Von Neumann’s remarks begin by interpreting probabilities in terms of ensembles:

If we wish to analyse the meaning of the statistical statements of quantum mechanics, we must necessarily deal with « ensembles » of a great number of identical systems, and not with individual systems.

He segues, however, into a discussion of quantum logic, arguing that the central point is the failure of the distributive law. This leads to the following:

A complete derivation of quantum mechanics is only possible if the propositional calculus of logics is so extended, as to include probabilities, in harmony with the ideas of J. M. Keynes. In the quantum mechanical terminology: the notion of a « transition probability » from \( a \) to \( b \), to be denoted by \( P(a,b) \) must be introduced. \( P(a,b) \) is the probability of \( b \), if \( a \) is known to be true. \( P(a,b) \) can be used to define \( a \leq b \) and \( -a : P(a,b) = 1 \) means \( a \leq b \), \( P(a,b) = 0 \) means \( a \leq -b \). But \( P(a,b) = \phi \), with a \( \phi > 0 \), \( < 1 \) is a new « sui generis » statement, only understandable in terms of probabilities.

It is interesting that von Neumann does not attempt to use Keynesian logical-probability theory to define single-shot probabilities. Instead, he still treats statistical statements as having meaning only for ensembles.

The report of Bohr’s reply is also instructive, as it illuminates the differences in motivation and outlook between them.

Professor Bohr expressed his admiration for the skill with which Professor von Neumann had treated the fundamental problems of quantum theory from the mathematical and logical point of view. He pointed out at the same time how the very simple experimental cases which he alluded to in his paper showed, in more elementary form, the same essential points as those which appeared in the mathematical analysis. We must also notice that the question of the logical forms which are best adapted to quantum theory is in fact a practical problem, concerned with the choice of the most convenient manner in which to express the new situation that arises in this domain. Personally, he compelled himself to keep the logical forms of daily life to which actual experiments were necessarily confined. The aim of the idea of complementarity was to allow of keeping the usual logical forms while procuring the extension necessary for including the new situation relative to the problem of observation in atomic physics.
The contrast made by Bohr here is reminiscent of the distinction Fuchs drew between his pre-QBist, Bayesian position and the viewpoint of Griffiths [27, p. 200]. Also, it illustrates the kind of divergence which a closer examination finds between physicists who are casually lumped together in retrospect [12].

D. Game Theory (1944)

Von Neumann coauthored the textbook *Theory of Games and Economic Behavior* with Oskar Morgenstern. The book, first published in 1944, is frequentist in orientation, though the authors express this as a matter of convenience rather than necessity. Von Neumann and Morgenstern call the “interpretation of probability as frequency in long runs” a “perfectly well founded” notion, but they leave the door open to alternative conceptions of probability [99]. Morgenstern later explained [60],

We were, of course, aware of the difficulty with the logical foundations of probability theory. We decided we would base our arguments on the classical frequency definition of probability, but we included a footnote saying that one could axiomatize utility and probability together and introduce a subjective notion of probability. This was done later by others.

For the work in question, see Pfanzagl [67–69].

E. Generating Random Numbers (1951)

One of von Neumann’s memorable remarks has gained a certain infamy: “Any one who considers arithmetical methods of producing random digits is, of course, in a state of sin.” This quotation occurs in an item in a 1951 volume of conference proceedings, where von Neumann also discusses physical phenomena which can be used to generate random numerical sequences [100]. He proposes “nuclear accidents” as the ideal source, which in the era after Chernobyl and Fukushima comes across as slightly ominous. However, in context it is plain enough that the “accidents” in question are events like individual clicks from a Geiger counter.

There are nuclear accidents, for example, which are the ideal of randomness, and up to a certain accuracy you can count them. One difficulty is that one is never quite sure what is the probability of occurrence of the nuclear accident. This difficulty has been overcome by taking larger counts than one [does] in testing for either even or odd. To cite a human example, for simplicity, in tossing a coin it is probably easier to make two consecutive tosses independent than to toss heads with probability exactly one-half. If independence of probability tosses is assumed, we can reconstruct a 50–50 chance out of even a badly biased coin by tossing twice. If we get heads-heads or tails-tails, we reject the tosses and try again. If we get heads-tails (or tails-heads), we accept the result as heads (or tails). The resulting process is rigorously unbiased, although the amended process is at most 25 percent as efficient as ordinary coin-tossing.

The language here is *prima facie* frequentist or propensity-inclined, treating probabilities as unknown quantities to be measured (“one is never quite sure what is the probability
of occurrence of the nuclear accident”). A Bayesian can give meaning to statements about “unknown probabilities”—this is the territory of the de Finetti theorem [14] and, in category theory, the Giry monad [35]—but von Neumann’s phrasing does not sound like a stringent Bayesian’s first choice of words.

In summary, von Neumann’s interpretation of probability moved from a kollectiv-based strict frequentism to a Keynesian view. Nowhere do we find an outright endorsement of personalist Bayesianism; the closest approach is much later than MFQM, is not in the context of quantum theory, and is itself mixed in with a claim that thinking of probability as long-run frequency is good enough for practical purposes.

IV. PURE AND MIXED STATES

Von Neumann’s philosophy of probability, and the way his thinking changed over time, has been discussed by others—for example, by Bub [10], Rédei [76], Stairs [89] and Valente [94]. Less remarked-upon, but also important to this comment, are von Neumann’s statements concerning the distinction between pure and mixed quantum states.

Returning to MFQM, on page 295 we find the following:

In the state $\phi$ the quantity $\mathfrak{R}$ has the expectation value $\rho = (R\phi, \phi)$ and has as its dispersion $\epsilon^2$ the expectation value of the quantity $(\mathfrak{R} - \rho)^2$, i.e., $((R - \rho \cdot 1)^2 \phi, \phi) = ||R\phi||^2 - (R\phi, \phi)^2$ (cf. Note 130; all these are calculated with the aid of $\bar{E}$!.) which is in general $> 0$ (and $= 0$ only for $R\phi = \rho \cdot \phi$, cf. III.3.) — therefore there exists a statistical distribution of $\mathfrak{R}$, even though $\phi$ is one individual state – as we have repeatedly noted.) But the statistical character may become even more prominent, if we do not even know what state is actually present – for example, when several states $\phi_1, \phi_2, \ldots$ with the respective probabilities $w_1, w_2, \ldots$ ($w_1 \geq 0, w_2 \geq 0, \ldots, w_1 + w_2 + \cdots = 1$) constitute the description. Then the expectation value of the quantity $\mathfrak{R}$, in the sense of the generally valid rules of the calculus of probabilities is $\rho' = \sum_n w_n \cdot (R\phi, \phi_n)$.

The language here indicates that for von Neumann, a pure state is something an individual system has, and a more general density matrix stands for an ensemble in which different pure states are physically present with different frequencies.

Von Neumann writes freely of properties possessed by quantum systems (p. 338):

Instead of saying that several results of measurement (on $S$) are known, we can also say that $S$ was examined in relation to a certain property $\mathcal{E}$ and its presence was ascertained. [...] The information about $S$ therefore always amounts to the presence of a certain property $\mathcal{E}$ which is formally characterized by stating the projection $\bar{E}$.

And, shortly thereafter, bluntly:

That is, if $\mathcal{E}$ is present, the state is $\phi$.

An exhaustive measurement fixes the value of a physical property, and the presence of a physical property can mandate the correctness of a choice of quantum state. Unsharp measurements, in von Neumann’s development, “are incomplete and do not succeed in determining a unique state” (p. 340). Again, we see the language making a category distinction between quantities which are “states” and more general entities which are not.
Later, von Neumann writes the time-evolution equation for a pure state’s “statistical operator” (density operator) as

\[ \frac{\partial}{\partial t} U_t = \frac{2\pi i}{\hbar} (U_t H - H U_t). \]  

(1)

Then, he writes, “Now if \( U_t \) is not a state, but a mixture of several states […] with the respective weights \( w_1, w_2, \ldots \), then it must be changed in such a way as results from the changes of the individual” states in the mixture (p. 350). In his way of thinking, a “mixture of several states” is “not a state” itself. This distinction is maintained throughout his discussion of what we now call the von Neumann entropy. During his study of thermodynamics, von Neumann considers a \( \frac{1}{2}(P_\phi + P_\psi) \) gas; he glosses this as a gas consisting “of \( N/2 \) systems in the state \( \phi \) and \( N/2 \) systems in the state \( \psi \)” (p. 370). More generally, for a density operator given by a weighted sum of projectors,

\[ U = \sum_{n=1}^{\infty} w_n P_{\phi_n}, \]  

(2)

a “\( U \)-gas” is “composed of a mixture of \( P_{\phi_1}, P_{\phi_2}, \ldots \) gases of \( w_1 N, w_2 N, \ldots \) molecules respectively” (p. 376).

It is helpful at this point to see how other physicists read von Neumann. Fortunately, we have an example on record, provided by David Bohm’s 1951 textbook, *Quantum Theory* [8]. Bohm wrote this book before developing what we now call “Bohmian mechanics”; instead of trying to make sense of quantum theory using pilot waves, it aims to present the perceived practical orthodoxy of the time. Bohm’s chapter on the “quantum theory of the measurement process” references MFQM’s discussion of that topic, with no indication that Bohm found it flawed (p. 583, §22.1). Comparing Bohm and von Neumann is instructive: it reveals how the ideas which von Neumann propagated were not Bayesian ones.

First, Bohm interprets quantum probabilities as long-run frequencies. The meaning of a probability found by squaring the magnitude of a wave function depends, he says, on “a large number of equivalent systems” prepared identically (p. 224, §10.30). Furthermore, for an individual quantum system, there is in principle a correct quantum state; “the physical state of the system” determines “the probability of a quantum jump” (p. 30, §2.5). Bohm describes one thought experiment in the following terms (p. 606, §22.11):

The entire system, consisting of spin, \( z \) co-ordinate of the atom, apparatus which measures the \( z \) co-ordinate, and apparatus which records the results of this measurement, is assumed to have some pure wave function when the experiment starts. (It is not necessary that any human observer know exactly what this wave function is.)

If one of a set of quantum states might be physically present, Bohm treats the situation with a “statistical ensemble of states” (p. 604, §22.10). Refining an ensemble to contain only a single state “represents absolutely no change in the state of the spin”; indeed, Bohm goes so far as to argue that this means the terms “mixed state” and “pure state” are misleading.

It seems unwise to adopt a terminology that suggests the spin changes its state (from mixed to pure) under circumstances in which nothing changes except the observer’s information about the spin. The phrase “statistical ensemble of states” provides a more accurate description.
For Bohm and von Neumann alike, states are physically present outside the physicist, and uncertainty about which state might be extant is represented by statistical ensembles. Like Bohm, von Neumann draws a line between what we would call pure states and mixed states. This idea, that pure and mixed states are qualitatively different kinds of entity, was soon challenged by Jaynes [42]. One can conceive of uncertainty intrinsic to a pure state and uncertainty about which pure state might be present. However, as Jaynes writes,

If the former probabilities are interpreted in the objective sense, while the latter are clearly subjective, we have a very puzzling situation. Many different arrays, representing different combinations of subjective and objective aspects, all lead to the same density matrix, and thus to the same predictions.

This argument was later made by Ochs [65], and by Caves, Fuchs and Schack [14].

Since this point will be important later, we go into the mathematics in more detail [40, 79]. Let \( \rho \) be a density matrix, and let \( \{|\hat{e}_i\rangle\} \) be a normalized eigenbasis of \( \rho \) with eigenvalues \( \{\lambda_i\} \). Denote the rank of \( \rho \) by \( k \), and pick an arbitrary matrix \( M \) of dimensions \( r \times k \), where \( r \geq k \), and the columns of \( M \) are orthonormal vectors in \( \mathbb{C}^r \). Then, for any such choice of matrix \( M \), the set of unnormalized states

\[
|\psi_i\rangle = \sum_{j=1}^{k} M_{ij} \sqrt{\lambda_j} |\hat{e}_j\rangle, \quad \text{with } i = 1, \ldots, r,
\]

(3)

provide a resolution of the density matrix \( \rho \). The proof is straightforward. First,

\[
\sum_{i=1}^{r} |\psi_i\rangle\langle\psi_i| = \sum_{i=1}^{r} \sum_{l,m} M_{il}^* M_{im} \sqrt{\lambda_l \lambda_m} |\hat{e}_m\rangle\langle\hat{e}_l|.
\]

(4)

Using the orthonormality of the columns of \( M \), we simplify this to

\[
\sum_{i=1}^{r} |\psi_i\rangle\langle\psi_i| = \sum_{m=1}^{k} \lambda_m |\hat{e}_m\rangle\langle\hat{e}_m|.
\]

(5)

The quantity on the right is just the density matrix \( \rho \), meaning that

\[
\rho = \sum_{i=1}^{r} |\psi_i\rangle\langle\psi_i|.
\]

(6)

If we normalize the states \( \{|\psi_i\rangle\} \) by

\[
|\hat{\psi}_i\rangle = \frac{|\psi_i\rangle}{\sqrt{\langle\psi_i|\psi_i\rangle}},
\]

(7)

then the density matrix is

\[
\rho = \sum_{i=1}^{r} w_i |\hat{\psi}_i\rangle\langle\hat{\psi}_i|,
\]

where \( w_i = \langle\psi_i|\psi_i\rangle \).

The coefficients \( w_i \) give the statistical weightings of the states in the decomposition. As Caves, Fuchs and Schack note, “a mixed state has infinitely many ensemble decompositions
into pure states”—even into different numbers of pure states—“so the distinction between subjective and objective becomes hopelessly blurred.”

The mathematical point of the multiplicity of ensemble decompositions was made by Hughston, Jozsa and Wootters [40] in 1993, whose notation I mostly follow here. It was also demonstrated almost sixty years earlier by Schrödinger [79], who disclaimed priority for the result, suggesting that some form of the idea was folk knowledge or shared conversationally at the time.  

Arguably, it would be more consistent for a strict Kollectivist to treat all quantum states, pure and mixed, in ensemble terms, where pure states correspond to the most purified possible ensembles. However, von Neumann’s statements about the presence of physical properties determining unique states clash with this position. Maintaining such a stance reads into the book a rigidity which is lacking in its language.

V. MEASUREMENT AND SUBJECTIVITY

As we noted in Section II, it is not so difficult to find in physicists’ writings statements which, taken in isolation, are compatible with QBism. The more important question is whether a verbal corpus yields up enough of these cherries to fill a bowl.

The last chapter of MFQM concerns “The Measuring Process.” Here, we find mentions of “subjective perception” and “the intellectual inner life of the individual” (p. 418). Surely this is where we should look for evidence of von Neumann’s Quantum-Bayesian sympathies. He writes (p. 420),

Indeed experience only makes statements of this type: an observer has made a certain (subjective) observation; and never any like this: a physical quantity has a certain value.

But the idea that we ultimately rely on sense impressions to adjudicate between scientific models is hardly original to quantum mechanics, or to Bayesian interpretations thereof. A form of the idea is attributed to Democritus [19], and Lucretius discussed it in verse [51]. Schrödinger commented, “Quantum mechanics forbids statements about what really exists—statements about the object. Its statements deal only with the object-subject relation.” However, he continued, “this holds, after all, for any description of nature”; the crucial point is that in quantum physics, it “holds in a much more radical and far reaching sense” [29].

In classical mechanics, the mass of an object is a basic property which that object has whether or not a physicist is nearby to be interested in it. Let us imagine a physicist working in the days before quantum theory. He drops a rock on his foot and sees red—a subjective perception. He then uses this perception, which is part of his “inner life,” to rule out the hypothesis that the rock is of negligible mass. The variable \( m \) in his equations refers to an intrinsic property of the rock, not to any of his sensations, even though his sensory

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4 The fact that a mixed state can be decomposed in multiple ways is one of the phenomena reproduced in the hidden-variable statistical models mentioned above. For example, in Speckens’ original “toy theory” of a qubit-like object, the completely mixed state has three different decompositions into convex combinations of pure states [84, §III.A.4]. This also holds when that theory is extended to composite systems [84, §IV.C]. Furthermore, it occurs in the Gaussian subtheory of quantum mechanics, which one can obtain by epistemically restricting (or “epistricting”) classical Liouville mechanics [6, 85].
perceptions are what he uses to assign a value (or a spread of reasonable values) to the variable $m$.

The critical question is whether von Neumann reads the mathematical entities appearing in the quantum formalism as physical quantities akin to Newtonian masses, about which we can use subjective perceptions to make estimations, or if he reads those mathematical entities as standing for perceptions themselves. The blanket statement quoted above that “experience only makes statements of this type” certainly suggests that he views the point about “(subjective) observation” to be applicable to classical physics. The evidence we saw in the previous section indicates that von Neumann treats quantum states as physical properties held by objects themselves, that is, as more analogous to the mass of a Newtonian rock than to experiences in the flow of “intellectual inner life.”

At one point in the Warsaw proceedings, von Neumann does approach a statement which might not sound completely out of place coming from a QBist. In a later discussion than the exchange we examined above, the Warsaw proceedings record the following [7, p. 44].

Professor von Neumann thought that there must always be an observer somewhere in a system: it was therefore necessary to establish a limit between the observed and the observer. But it was by no means necessary that this limit should coincide with the geometrical limits of the physical body of the individual who observes. We could quite well « contract » the observer or « expand » him: we could include all that passed within the eye of the observer in the « observed » part of the system — which is described in a quantum manner. Then the « observer » would begin behind the retina. Or we could include part of the apparatus which we used in the physical observation — a microscope for instance — in the « observer ». The principle of « psycho-physical parallelism » expresses this exactly: that this limit may be displaced, in principle at least, as much as we wish inside the physical body of the individual who observes. There is thus no part of the system which is essentially the observer, but in order to formulate quantum theory, an observer must always be placed somewhere.

Terms like “observer” and “measurement” imply an essential passivity, Fuchs and Schack have argued [31]; such words suggest a casual, uninvolved reading-off, rather than a participatory act. But if we replace “observer” with “agent” in von Neumann’s concluding line, we would have the statement, “In order to formulate quantum theory, an agent must always be placed somewhere”; this claim, that agent is a fundamental concept which quantum theory is built upon, would fit within QBism.

To a QBist, this is the killing flaw in von Neumann’s interpretation of quantum mechanics. On the one hand, von Neumann affirms that one cannot formulate the theory without an observer, but on the other, quantum states are physical properties of systems outside the observer, and probabilities are frequencies in kollectivs or Keynesian logical valuations—conceptions of probability which try to delete the agent at all cost.²

² Arguably, von Neumann is not always consistent with his own insistence on “psycho-physical parallelism.” A careful reading of MFQM §VI.3 suggests that it elides the “limit between the observed and the observer” which he deems essential, both in MFQM (§VI.1, p. 420) and at the Warsaw conference. But teasing out the meaning of “psycho-physical parallelism” is no simple task [38], and for the purposes of this essay, pursuing it in greater depth is not essential.
In broad overview, von Neumann’s approach to quantum measurement begins with a physical system, which then interacts with some kind of measuring apparatus, which is then studied by an observer. The system-apparatus and apparatus-observer interactions are treated as physically distinct kinds of time evolution.

If one takes quantum states to be intellectual tools held by an individual agent, then the procedure of inserting an intermediate apparatus adds nothing to the basic philosophical understanding of what quantum theory is about. It could well be a beneficial mathematical exercise, part of working out what to do when making use of multipartite systems. There are good practical reasons to understand how the quantum formalism applies to a system one of whose parts is a probe or an ancilla for the other, or is a communication channel whose function is limited in some way. Regardless, if quantum states are personalist Bayesian quantities, then introducing probes and ancillas brings nothing intrinsically new. And if von Neumann had seen quantum states in anything like this fashion, it is difficult to find a rationale for why MFQM’s entire chapter on “the measuring process” assumes the shape it does.

Fuchs has written, “von Neumann’s setting the issue of measurement in these terms was the great original sin of the quantum foundational debate” [32, p. 2035].

VI. MEASUREMENT REDUX: THE “QUANTUM BAYES RULE”

Wikipedia attributes the statement “The first quantum Bayesian was von Neumann” to R. F. Streater [90]. As mentioned earlier, the effect of saying this in an article which primarily concerns QBism is to claim that von Neumann was himself either QBist or something much like it. Looking up this source, we find that what Streater calls “Quantum Bayesianism” could indeed reasonably include QBism. For example, he states that Bayesians “attribute all the entropy in a state to the lack of information in the observer” (p. 71). And in discussing density matrices, he writes, “the Bayesian’s ρ is entirely about his knowledge” (p. 72). So, the meaning of the statement created by placing it within the Wikipedia article is not too much of a stretch.

Streater bases his claim that von Neumann was the “first quantum Bayesian” on MFQM, never addressing the plainly frequentist orientation of that book. Nor does Streater refer to the passages about “subjective perception” and “experience.”

The root of the confusion appears to be that, towards the end of MFQM, von Neumann derives a formula which turns out in retrospect to be a specialized case of a quantum analogue of the Bayes conditioning rule. Von Neumann motivates his argument with the following (p. 337):

If anterior measurements do not suffice to determine the present state uniquely, then we may still be able to infer from those measurements, under certain circumstances, with what probabilities particular states are present. (This holds in causal theories, for example, in classical mechanics, as well as in quantum mechanics.) The proper problem is then this: Given certain results of measurements, find a mixture whose statistics are the same as those which we shall expect for a system S of which we know only that these measurements were carried out on it and that they had the results mentioned.

Here, von Neumann treats quantum states as analogous to the physical states of classical mechanics, i.e., to points in phase space. In classical mechanics, if a system could be at one
of multiple points in its phase space, we write a Liouville probability density over that space; we can infer that for von Neumann, it is *mixtures* which are analogous to Liouville densities. This is in sharp contrast with QBist and much other Quantum-Bayesian thinking, in which *all* quantum states, however pure, are expressions of an agent’s probability assignments.

As always in MFQM, a system has a state, even if we don’t know what that state is. And, as always in MFQM, statistics means ensembles of identically prepared systems:

If, for many systems $S_1',\ldots,S_M'$ (replicas of $S$), these measurements give the results mentioned, then this ensemble $[S_1',\ldots,S_M']$ coincides in all its statistical properties with the mixture that corresponds to the results of the measurements.

Changes in statistical properties mean the creation of new ensembles with different population demographics:

That the results of the measurements are the same for all $S_1',\ldots,S_M'$ can be attributed, by $M.$, to the fact that originally a large ensemble $[S_1,\ldots,S_N]$ was given in which the measurements were carried out, and then those elements for which the desired results occurred were collected into a new ensemble. This is then $[S_1',\ldots,S_M'].$

Here, $M.$ refers to the measurement postulate, “If the physical quantity $\mathfrak{R}$ is measured twice in succession in a system $S$, then we get the same value each time” (p. 335).

So, we have something like the updating of probabilities by the Bayes rule. However, von Neumann phrases the scenario in completely Kollectivist language. Merely invoking Bayes’ theorem does not make one a Bayesian. For example, von Mises makes use of Bayes’ theorem, calling it “a proposition applying to an infinite number of experiments,” or in other words, to a kollectiv [97, p. 123]. One could be wholly agnostic about the interpretation of probability, setting up the theory from measure-theoretic or abstract-algebraic axioms [91]; multiplication and division of probabilities would then be legitimate operations having meaning only with respect to those axioms.

Lüders keeps to von Neumann’s philosophy on this matter [52].

In a measurement of $R$ followed by a selection of $r_k$, $Z$ is transformed to

$$Z_k' = P_k Z P_k.$$

$Z_k'$ is not normalized [...] but instead is chosen so that the trace shows the relative frequency of the occurrence of $r_k$ in the ensemble.

[Bei Messung von $R$ mit nachfolgender Aussonderung von $r_k$ geht $Z$ über in

$$Z_k' = P_k Z P_k.$$]

Here we have someone who read von Neumann carefully and offered a correction to von Neumann’s work. Lüders is plainly frequentist, yet never argues that this represents a departure from von Neumann—because, as we have seen, it doesn’t.

It is in this context that von Neumann mentions “a priori” and “a posteriori” probabilities. These terms could be glossed in a Bayesian way, but only at the cost of ignoring
everything else MFQM says about the interpretation of probability, including the discussion of “ensembles” in the same paragraph. And even if one were to do so, the way in which von Neumann allows pre-existing physical properties to determine quantum states would imply a view in which mixed states are Bayesian probability distributions over pure states. (And though it could potentially be called “quantum Bayesian,” it is definitely not QBist.) This is a difficult position to maintain, per the Jaynesian argument given above. Furthermore, given the professed Kollectivism of MFQM, we should recall what von Mises said about these terms [97, p. 46]:

It is useful to introduce distinct names for the two probabilities of the same attribute, the given probability in the initial collective and the calculated one in the new collective formed by partition. The current expressions for these two probabilities are not very satisfactory, although I cannot deny that they are impressive enough. The usual way is to call the probability in the initial collective the a priori, and that in the derived collective the a posteriori probability.

This usage exactly parallels von Neumann’s. To continue:

The fact that these expressions suggest a connexion with a well-known philosophical terminology is their first deficiency in my eyes. Another one is that these same expressions, a priori and a posteriori, are used in the classical theory of probability in a different sense as well, namely, to distinguish between probabilities derived from empirical data and those assumed on the basis of some hypothesis; such a distinction is not pertinent in our theory. I prefer, therefore, to give to the two probabilities less pretentious names, which have less far-reaching and general associations. I will speak of initial probability and final probability, meaning by the first term the probability in the original collective, and by the second one, the probability (of the same attribute) in the collective derived by partition.

Von Neumann uses the more common terminology, but the meaning which MFQM vests in the words is, by all evidence, the same as that which von Mises does. The result is not an argument that probabilities should be seen as quantified fervencies of belief, but rather that a certain problem involving ensemble frequencies admits nonunique solutions.

Von Neumann derives his basic relation between initial and final ensembles by considering the following procedure (p. 340). We measure some binary physical property $E$ on each element of the initial ensemble, which is described by the statistical operator $U_0$. The elements for which this measurement yields the outcome 1 (instead of 0) are collected to form a new ensemble, whose statistical operator is $U$. The two ensembles are related by

$$U = \sum_n (U_0 \phi_n, \phi_n) P[\phi_n].$$

Here, $P[\phi_n]$ is the projector onto the state $\phi_n$, and the set $\{\phi_1, \phi_2, \ldots, \phi_n\}$ is an orthonormal basis which spans the subspace in which measuring $E$ yields the value 1.

Lüders criticized von Neumann’s result and proposed a correction [52]. A more modern way to represent state-change upon measurement is to write the Lüders rule using the mathematics of effects and operations. A measurement is a positive operator valued measure (POVM) which furnishes a resolution of the identity:

$$\sum_k E_k = 1,$$
and each of the \( \{E_k\} \) can be written

\[
E_k = \sum_l A_{kl}^\dagger A_{kl}.
\]  

(11)

Here, the index \( k \) labels the possible outcomes of the measurement. If the initial density operator is \( U_0 \), then upon obtaining the outcome \( k \), we update the density operator to

\[
U = \frac{\sum_l A_{kl} U_0 A_{kl}^\dagger}{\text{tr}(E_k U_0)}.
\]  

(12)

For analyses of how this update rule is analogous to, or a variant of, Bayesian conditioning, see Schack, Brun and Caves [78]; and also Fuchs [25]. Illustrative examples are developed in Fuchs and Schack [26].

Streater bases his criticism of von Neumann on that found in the textbook of Krylov [47]. This book is not an easy read; it is an unfinished manuscript on the foundations of statistical physics published after Krylov’s death, and then translated from Russian to English some years later. (Streater refers to the English translation.) If we follow Streater’s summary, we read, “Krylov did not believe that all the characteristics of the state reflect only the lack of knowledge of the observer, but that there was a physical state, \( \rho_0 \) out there to be found.” But this is exactly what von Neumann stated: recall that for him, a “mixture of several states” is “not a state” itself (MFQM, p. 350).

Krylov’s primary complaint with von Neumann (pp. 184–85) is the multiplicity of valid decompositions of mixed-state density operators.

Firstly, in using the statistical operator, we assume the selection of a certain orthogonal system of coordinates in the subspace delimited by an inexhaustively complete experiment and we also assume a certain choice of weights \( w_i \). […] A change in the orthogonal system means, generally speaking, a transition to another physical state described by the statistical operator, to what is said to be another statistical aggregate. (“A state” is understood here in a more general sense than a state exhaustively completely determined, using a \( \Psi \)-function.) […] Therefore, the selection of a certain orthogonal system of functions and the fixing of certain weights \( w_i \), which the von Neumann operator presupposes, amounts to the introduction of some physically fictitious properties of the reality being described.

That is, Krylov finds the conjunction of the following two statements unacceptable:

- A pure quantum state is a physical property of a system.
- The quantum formalism implies that a mixed-state statistical operator has multiple decompositions into linear combinations of pure states.

Krylov insists upon the former and therefore finds the latter unsatisfactory. As we discussed earlier, QBists (and some other varieties of quantum Bayesians) agree that these two statements clash with each other, but discard the first instead. Von Neumann holds onto both (MFQM, §IV.3).
VII. DISCUSSION

Classifying scientists is not easy. A good classification summarizes, as succinctly as possible, the known statements and actions of an individual, and should have predictive power for statements yet unmade or undiscovered. Furthermore, the descriptive terms in commonest circulation should provide the most useful and meaningful understanding of relevant distinctions that painting with a broad brush can convey [9]. It is, to say the least, debatable that the jargon we have today on the philosophical side of quantum theory fares at all well in this regard.

Consider the practical problems which make classification difficult:

We change our views over time. We leave fragmentary records of our thoughts, not atypically muddled by the compromises of coauthorship and journal publication. (The rise of an electronic preprint culture has, among other things, provided a way to track what squeezing our works into journals can do to them; for an example, see [22].) And because our attitudes can be moving targets, combining a physicist’s statement from year $N$ with another statement they made in year $N + 20$ to deduce what they “must logically have believed” is an exercise fraught with a scholarly kind of peril. Worse yet, the views a physicist holds at a specific time might be internally inconsistent. Perhaps they are aware of the contradiction and work to remedy it, or perhaps they are aware but shrug it off and live with it. Perhaps they are unaware because nothing has forced the problem to their mind, or perhaps they refuse to admit the possibility of a contradiction whenever it is raised. It is even conceivable that no human mind has yet followed the logical chain from the accepted ideas to the lurking contradiction.\footnote{Compare the observation by Żukowski and Brukner, during a discussion of the role “counterfactual definiteness” plays in Bell’s Theorem: “One could only counterfactually wonder what the views of EPR would have been had the GHZ paper appeared before 1935” [107].}

If our goal is to summarize and predict the activity of human beings, we ought to admit the ways in which we are not logico-deductive automata. Peres quips, “There seems to be at least as many Copenhagen interpretations as people who use that term, perhaps even more” [66]. Fuchs writes, in the same vein, “I don’t know what the hell the Copenhagen interpretation is and no one else does either” [32, p. 1295]. Indeed, a good case can be made that the idea of a unified “Copenhagen interpretation” was a myth of the 1950s, which now elides in our perception the differences among views held by physicists thirty years earlier [12, 39]. Moving to the other extreme of what we might guess to be the first principal component of the quantum-foundations cloud, Kent tabulates at least twenty-one varieties of Everettian interpretation [43]. These can differ in underacknowledged ways from Everett’s original [44], and many are “generally incompatible” with one another [45]. Ambiguities like this make formulating sensible survey questions about physicists’ opinions regarding quantum foundations into an uphill, not to say Sisyphean, task. The surveys done so far on this subject have had small sample sizes [81], but to my mind, the problem of terminology is a more serious difficulty. Conducting a good opinion poll is a practical problem, while sorting out philosophical jargon is a conceptual one.

The jargon which has become commonplace—the sort of terms which Wikipedia brings up if we ask it for its category, “Interpretations of quantum mechanics”—are Crayola slashes across history. Worse yet, they matter. They are signifiers which we use to determine what we bother to read. They establish the distinctions which we allow to exist between the players in the histories we retell. Then we pose those players according to our sentiments.
for the person and for the philosophy.\(^7\)

The scholarly literature is quite capable of spreading urban legends of its own \([59, 77]\). What happens when we bring in Wikipedia?

The Wikipedia project bills itself as “the free encyclopedia that anyone can edit”; even if we gloss “anyone” as “anyone with an Internet connection,” this is only true to a first approximation. Individual users, or the machines they edit from, can be blocked from editing for various lengths of time for offenses like persistent hate speech \([21]\). Also, individual pages can be protected from editing to different extents. Wikipedia has its own policies and community institutions, often referred to by acronyms (NPOV, NOR, FAC, FARC and so forth). These, in addition to sheer size and visibility, distinguish Wikipedia from other applications of the wiki concept, such as Scholarpedia \([82]\), the nLab \([64]\), Memory Alpha \([53]\) and TV Tropes \([93]\). For example, Wikipedia has a “No Original Research” policy \([105]\), but the nLab has as one of its primary goals the facilitation of original mathematical work.

Obvious disruption on Wikipedia, like replacing entire articles with strings of profanity, gets caught and reversed quickly. Subtle changes last longer and propagate farther. Some silliness about the Golden Ratio and Duchamp’s *Nu descendant un escalier n° 2* has survived, as of this writing, for several months \([88]\). A couple of stoned college kids make a joking edit to the page of a children’s book author, which endures for over five years and ends up uncritically accepted in, among many other places, a book about Jesus \([20]\). Sometimes, the process even closes back upon itself: careless writers elsewhere use a bit of entertaining trivia invented on Wikipedia, and their writings in turn become sources to fill in the “citation needed” tag on the Wikipedia page. This is how the coati became the “Brazilian aardvark” \([74]\). Similarly, thanks to other sites mirroring Wikipedia content, the Wikipedia page for “Pareto efficiency” is self-referential \([37]\). The results of this “citogenesis” \([61]\) can be remarkably difficult to sort out, particularly if the debunking requires “Original Research,” which in the Wikipedian argot includes “any analysis or synthesis of published material that serves to reach or imply a conclusion not stated by the sources” \([105]\).

William James once said of academic philosophy \([41]\),

\[
\text{[T]he forms are so professionalized that anybody who has gained a teaching chair and written a book, however distorted and eccentric, has the legal right to figure forever in the history of the subject like a fly in amber. All later comers have the duty of quoting him and measuring their opinions with his opinion. Such are the rules of the professorial game—they think and write from each other and for each other and at each other exclusively. With this exclusion of the open air all true perspective gets lost, extremes and oddities count as much as sanities, and command the same attention[.]}
\]

On Wikipedia, the “right to figure forever in the history of the subject like a fly in amber” can be supported even by a mention in nothing more substantial than the popular press, like by *New Scientist* magazine, despite the well-known failure modes of that industry \([32, p. 2221]\).

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\(^7\) I myself have heard Feynman claimed as a Copenhagener, a Gell-Mannian, an Everettian and an enthusiast for nonlocal hidden variables. The implication is that the correct interpretation of the quantum will be decided, not even by opinion poll \([33, 62]\), but by who occupies the most shelf space in the Caltech bookstore.
Thanks to the No Original Research policy mentioned earlier, correcting misconceptions propagated by popular-science magazines and the like cannot begin with Wikipedia itself. The NOR policy makes sense for what Wikipedia is and what it tries to be: an encyclopedia is a tertiary source, rather than a primary or secondary one. Moreover, Wikipedia lacks the infrastructure to evaluate original scholarship, and identifying who wrote what in its articles is an arduous task, making it a poor place to advance new claims in a forthright way. Academic life depends on receiving credit for one’s own work, and the way Wikipedia articles are made flattens all contributions together, obscuring authorship. This essay is an attempt to do in a different venue what cannot feasibly be done within Wikipedia alone: set the record straight.

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