From Gender to Gleason: The Case of Adam Becker's *What Is Real?*

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Abstract

It is easy to argue that the founders of quantum mechanics made statements which are opaque and confusing. It is fair to say that their philosophical takes on the subject are not infrequently unsatisfying. We can all use reminders that human flaws and passions are a part of physics. So, it would be nice to have a popular book on these themes, one that makes no vital omissions, represents its sources accurately and lives up to its own ideals. Sadly, we're still waiting.

1 Overview

Imagine a biography of Isaac Newton that treated as a shocking revelation the fact that Newton was arrogant and abrasive; that accused Newton of being unscientific when, regarding a mechanism behind gravitation, he replied "I feign no hypothesis"; that yearned for an Aristotelian understanding of the Earth-Moon interplay; that mourned the absence of women in the story of classical mechanics, while briefly dismissing Émilie du Châtelet as "Voltaire's mistress". Now, take that equation, replace Newton's arrogance with Niels Bohr's verbal obscurity, and solve to find Adam Becker's *What Is Real?: The Unfinished Quest for the Meaning of Quantum Physics* (2018). I had to take *What Is Real?* in small doses. It is not *literally* true that every time I dipped into it, I found an error of physics or a misrepresentation of history. That is, however, a good first approximation of what the reading experience was like. Becker professes great concern about biases in physics, while perpetuating them. To summarize the remainder of this review in a sentence: Becker needed a villain, and he chose Niels Bohr. I was counseled to keep this short, but I did not do a very good job of that, because I fully expect that people will be quoting this book at me for years to come, and I would like to have to write a response only once.

My colleague Chris Fuchs noticed many problems, and reported a selection of them in his review for the *American Journal of Physics* [1]. Tom Siegfried found other issues that both Chris and I had missed [2]. I think his review expresses the big picture rather well:

> Becker's main argument insists that the [Bohrian] interpretation embraces the philosophy known as positivism (roughly, nothing unobservable is real, and sensory perceptions are the realities on which science should be based), and then demonstrates positivism's fallacies. He does a fine job of demolishing positivism. Unfortunately, the [Bohrian] interpretation is not positivistic, as its advocates have often pointed out.

Becker throws about the word *positivism* the way a TV pundit invokes *socialism,* and with comparably solid justification. He calls the philosophers Charles Morris and Hans Reichenbach positivists; neither of them were. Physicists get tagged with the p-word as well, generally without more reason than extremely selective quotation.

The ideological target of *What Is Real?* is the "Copenhagen interpretation of quantum mechanics". In my opinion, this is a term we should expunge from our vocabularies. What Bohr thought was not what Heisenberg thought, nor was it what Pauli thought; there was no single unified "Copenhagen interpretation" worthy of the name. Indeed, the term does not enter the written literature until the 1950s, and that was mostly due to Heisenberg acting like he and Bohr were more in agreement back in the 1920s than they actually had been [3–5]. Strangely, Becker acknowledges these divisions at times, yet agglomerates them back together when he needs a place to land a rhetorical blow (e.g., "It took someone who had always known Copenhagen was rotten, who had seen David Bohm do the impossible").

I worry that those who read Becker's book without having learned the subject first will come away with a sense of getting a complete picture. For example, Becker quotes the philosopher Jeffrey Bub lamenting about the "Copenhagen interpretation" in a 1997 monograph. But he doesn't mention Bub's later turn to the Dark Side, that is, to an informational interpretation of quantum mechanics haunted with the specter of Copenhagen [6]. He certainly doesn't mention that in 2017, Bub wrote a paper titled "Why Bohr was (Mostly) Right" [7]!

For another example, in chapter 2, he claims that Dirac disparaged Bohr's idea of "complementarity", saying that it was too vague to "provide you with any equations which you didn't have before". This is true, but in the same interview — an interview in 1963, not contemporaneous with Bohr's presentation in 1927, as the book insinuates — Dirac is equally harsh on the good old uncertainty principle. Dirac called the uncertainty principle "qualitative" and "of secondary importance". He also reveals how much he trusted Bohr's scientific judgment: When confronted with the idea that the fundamental law of conservation of mass-energy might be violated, Dirac said, "I was rather prepared to accept it if Bohr proposed it" [8]. The same interview archive reveals that Hendrik Casimir saw the lack of mathematical detail as an advantage. He regarded Bohr's dwelling on complementarity as a way of training intuition, honing his ability to tell what quantum mechanics would predict without having to do all the intricate calculations [9]. Seen in this light, what Becker disparages in

Bohr is simply the skill that we try to develop in all physics students: the facility to, as John Archibald Wheeler advised, "never calculate without first knowing the answer".

The first three printings of Becker's book mangle the explanation of the Einstein–Podolsky–Rosen thought-experiment, the foundational example upon which the study of quantum entanglement is built. Moreover, the error is one that should never happen, given a serious effort to engage with what Einstein and company were trying to prove and how Bohr was trying to respond. I emphasize that this was not an error of *interpretation,* but one of *description*; Becker issued an erratum after being corrected by N. David Mermin and David Albert, two Davids with very different interpretations of quantum mechanics [10]. (A side note: In his main text, Becker says that Bohm gave his version of the EPR experiment in terms of polarized photons. He then "corrects" this statement in his endnotes, saying that Bohm's example really used electrons. In fact, neither is true: Bohm's textbook sets up his version of EPR using atoms and Stern–Gerlach magnets.)

In chapter 7, while trying to explain "contextuality", Becker invents an analogy about neutrons that is more esoteric than the thing he is trying to define, which seems poor pedagogy. And in the process, he pretends that energy and momentum can be measured simultaneously, then doubles down on the error and says that energy and position are simultaneously measurable as well. There's writing for the masses, and then there's just being wrong. What follows is a bit better, phrasing the issue of "contexuality" in terms of a roulette wheel, which makes me wonder why neutrons were brought in to begin with. And then it all goes awry again when it confuses the result that *a hidden-variable model for* quantum mechanics has to be "contextual" for a proof that quantum mechanics *itself* is "contextual". Just because all excuses share a property does not mean that an honest explanation has that property too. When the condition is defined in a mathematically precise way that does not lean on hidden-variable models, one finds that quantum mechanics is not contextual at all [11].

On a related note, Becker poses the testing of Bell's inequality as a trilemma. Doing the experiment, one might find that the results deviate from the predictions of quantum theory (and thus, "instant Nobel Prize" — the Nobel figures largely in his rhetoric). Or, if the results accord with the quantum predictions, one could embrace a many-worlds picture. Thirdly, if the results agree with quantum theory, the other option is that nature allows for nonlocal influences. In re the first choice, enough Bell tests have closed enough loopholes that I feel comfortable saying that the quantum predictions are trustworthy. I am still skeptical that a fully consistent and genuinely predictive version of the many-worlds creed has ever been posed [12–15]. And if the theory is not logically consistent, I doubt that it can be the logical conclusion of any argument. Setting that path aside for the moment, then, we are left with the third horn of the trilemma: In brief, we are told, quantum physics must be nonlocal. I have seen this argument presented before at much greater length and in greater detail, and I remain unconvinced [16, 17], as are physicists who probably see themselves as a good deal more mainstream than I.[1]

[1] A sampling from proponents of various "interpretations": [18–23].

2 Details

Becker accuses John von Neumann of "sounding a positivist note" in his textbook, *Mathematical Foundations of Quantum Mechanics* (1932). I must admit that I take this a bit personally, because several years ago, I discussed that very passage at length in my own deep dive on von Neumann — spoiler, it's not positivist. In fact, Schrödinger (who is one of Becker's good guys) articulated the same sentiment, and *he* attributed it all the way back to Democritus of Abdera [24]. There is a wide gulf between saying that we ultimately use our sense impressions to judge between one scientific theory and another, and saying that unobservables are unmentionables. The latter is a positivistic attitude; the former is just, well, life, man.

In that 1932 text, von Neumann presented what he claimed was a proof that quantum uncertainties could not be explained away as merely our being ignorant of a deeper, classical-type layer of reality. His mathematics was correct, but one of his assumptions was unwarranted, a fact first pointed out by Grete Hermann [25, 26], a philosopher, mathematician and member of the anti-Nazi group known as the Internationaler Sozialistischer Kampfbund. Hermann's observation was, however, ignored for many years, and it's tough to say why [27]. She was a woman, a philosopher trying to talk to physicists (though she earned praise from von Weizsäcker and Heisenberg), and a socialist. Take your pick, I suppose. It is also worth noting that the set of people who *cared* about the philosophical foundations of quantum theory was, at the time, pretty darn small. In 1928, Hermann Weyl predicted that "the fate of the general scheme of quantum mechanics" was to be "submerged" in an improved future theory. A decade later, Hans Kramers declared that "everyone knew that the quantum theory was provisional"; to him, it was not at all definitive that "after some years, Schrödinger's equation would still be considered the root of everything". Heisenberg shared this attitude, speculating that quantum mechanics might break down at sufficiently high energies.[2] When the pioneers of the subject did not yet trust it in their bones, why would anyone waste time philosophizing about it?

To his credit, Becker raises the possibility that Hermann's work languished in obscurity because of sexism. But his assertion is so brief that I almost wish it weren't made at all. Two points:

1. While she had correctly taken down von Neumann's "proof" that no "hidden variables" can underlie quantum physics, Grete Hermann herself did not believe in hidden variables! She had her own reasons, due to her own philosophical concerns about causality. Becker says nothing about this! It's hard to take a man seriously when he says he wants to promote the cause of women in science, and he doesn't even describe what the most important woman in his story actually did. Hermann's "interpretation" of quantum mechanics seems to have been

[2] See the proceedings for the 1938 Warsaw conference, *New Theories in Physics* (International Institute of Intellectual Co-operation, 1939). I found Bohr's contribution to these proceedings much more clear than his reputation led me to expect, and both more approachable and more thought-provoking [10] than his reply to EPR. Catherine Chevalley notes that Bohr's Warsaw paper and the following discussion with von Neumann are among the important documents which "have been entirely omitted in the discussion of the so-called Copenhagen Interpretation" [28].

akin to Bohr's, and perhaps even more radical — but Bohr is the great villain of Becker's book, and nothing that lends him respectability can be allowed.

2. Sexism in science is a tangle of thorns now, and I'd say it was the same then. Scientific men are great believers in meritocracy — "We judge the work, and the work alone" — so much so that they cannot conceive the possibility that, even if the playing field were level, not everybody gets to join the game. A two-sentence toss-off does no justice to this problem. If you're not willing to see beyond mustache-twirling, why are you even talking? We will return to this point momentarily, but first, the question of von Neumann and "hidden variables" requires further exploration.

Becker goes into roof-raising passion about how von Neumann's proof was uncritically accepted for years, and how those no-good Copenhageners used it to shut down all discussion of possible alternative positions. His evidence? Well, it's pretty thin on the ground. He offers a passage in a book by Mara Beller, who selectively quotes the autobiography of the philosopher Paul Feyerabend, who (decades after the fact) recalled the aftermath of a lecture by Bohr. Feyerabend, not primarily a physicist or a mathematician, read the room and thought Bohr's supporters were invoking von Neumann to win the post-lecture discussion, while neither they nor their opponents understood the details of the proof itself. Beller leaves out the end of the story: "I found this very strange," Feyerabend wrote, "but was relieved to remember that Bohr himself had never used such tricks" [29].

According to Eugene Wigner, the mathematical proof that von Neumann included in his textbook was *not* the fundamental reason why he himself rejected the possibility of "hidden variables". Instead, von Neumann's conviction was primarily motivated by a more heuristic argument [30]. Heisenberg, too, had a qualitative argument for dismissing any putative hidden-variable completion of quantum theory, one that differed from von Neumann's concerns as well as Hermann's [4]. If he had found von Neumann's theorem conclusive, would he have bothered? I don't know, but Procrusteanizing the interplay of formal and heuristic arguments can't be a good way to do physics history.

In those years when, according to Becker, von Neumann's flawed proof was uncritically accepted, it was (you can tell where this is going) found unsatisfactory by multiple authors, among them Reichenbach. This is all described in Max Jammer's *The Philosophy of Quantum Mechanics* (1974), which Becker has presumably read, since it's listed in his bibliography. So, in fact, is Reichenbach's *Philosophic Foundations of Quantum Mechanics* (1944), which makes the omission of Reichenbach's criticism rather puzzling. Reichenbach's critique is less specific than Hermann's, but both of them hit the note that von Neumann had just assumed too much. Various folks also found von Neumann's axiomatization of quantum mechanics unsatisfying; it's not like the man was deified. When I read Nikolai Sergeevich Krylov's 1950 textbook, he sounded almost *angry* at von Neumann's treatment of "statistical operators", but perhaps translation from the Russian will do that [24]. Of the others who found fault or limitation in von Neumann's work, the writing of Lüders [31] appears to have been more productive than that of Temple [32]. However, the latter case illustrates that the editors of *Nature* were willing to publish a letter attacking one of von Neumann's fundamental axioms. (The root of Temple's concern appears to be that the "canonical quantization" procedure for making a classical theory into a quantum one does not yield a unique answer. This point was hashed out in a rather mundane cycle of publications [33, 34], the sort of day-to-day scientific exchange that makes for much less dramatic copy than cries of *censorship!* and *suppression!*) Von Neumann's flawed theorem was at the frontier of mathematical sophistication for a theory that was not yet fully trusted on physical grounds. While many may have accepted his conclusions too superficially, we should not forget the contributions of those who took greater pains.

Becker accepts hearsay about Bohr and passes it off as Bohr's own position about the philosophy of physics [1], while at the same time, he ignores indirect evidence that Einstein knew about, and was unimpressed by, von Neumann's attempt to rule out explanations of quantum randomness in terms of "hidden variables" [35, p. 286]. I can find no justification for this: It is not an extraordinary claim, requiring an extraordinary standard of support. On the contrary, what would be exceptional is Einstein not knowing about work done, on a problem that deeply concerned him, by a man in the same building.

In his concluding remarks, Becker laments, "hardly any women or people who aren't white appear anywhere in this story at all."

As one of the whitest guys I've ever met, I am the wrong fellow to provide a definitive take upon this point, so I will content myself with a list of people that he could have mentioned, but didn't:

In the early days of quantum mechanics, there were Satyendra Nath Bose, Ishiwara Jun, Nagaoka Hantaro and Hendrika Johanna van Leeuwen. On the philosophical side, Karen Barad offers a refreshingly clear reading of Bohr (sometimes, it takes a stiff dose of feminist theory to clear out the cobwebs that a physics education will leave). Susan Haack, a pragmatist I have gradually grown more radical than, could have illuminated the distinctions among philosophies that Becker lumps together. Mary Hesse is likewise noteworthy in this regard, and Bertha Swirles Jeffreys and Dorothy Maud Wrinch should not be forgotten. Of those who furthered von Neumann's work on quantum logic and quantum probability, more names spring to mind: Araki Huzihiro, Paulette Destouches-Février, Husimi Kôdi, Nakamura Masahiro, Mary Beth Ruskai, Maria Pia Solèr, Umegaki Hisaharu, Yanase Mutsuo, Watanabe Satosi. . . [3]

And that's just me checking the references I had close at hand — including the sources that Becker himself invokes, like Max Jammer. Rather incredibly, while mourning the lack of diversity in physics, he manages to portray it as an even whiter shade of male than it really was.

To paraphrase the teenagers I meet on the subway: I doubt your commitment to the Revolution, *tovarisch.*

Lee Tsung-Dao came close to deriving Bell's inequality, four years before Bell, but apparently he got tied up in extra complications due to kaon physics and never took his ideas to their full conclusion.[4]

[3] I myself am convinced that the work of Maryna Viazovska [36] is relevant to quantum foundations, but that is a point I am still working to make [37, 38].

[4] As recounted by Jammer [39, p. 308]. Oddly enough, kaon physics was the setting where I had my first real encounter with Bell's inequality, as an undergraduate. I think my physics education was fairly ordinary,

Together with Yang Zhenning, Lee had won the 1957 Nobel Prize in Physics for predicting that the weak nuclear force does not conserve parity. The *experimental* confirmation of parity violation was by Wu Chien-Shiung, but *she* didn't get a share of the Nobel. Wu also did work relevant to quantum foundations, essentially implementing the setup for the thought-experiment of Einstein, Podolsky and Rosen. Becker mentions this — believe me, by this point, I was almost expecting him *not* to. He says, rightly, that she was "renowned for her work in nuclear physics". Fine. But not indicating just *how* important her work was, and not mentioning her being slighted for a Nobel when he makes pious complaints about the hazards of bias in physics? Throughout *What Is Real?*, the Nobel Prize is a signifier of greatness. Never does Becker challenge the glamor — and the problems of the Nobel are low-hanging fruit in the study of science's cultural biases [40].

Similarly, Becker drops in the name of Melba Phillips, who did important work in nuclear physics and helped to introduce group theory into the practical application of quantum mechanics [41], only mentioning her as a friend of Bohm.[5] Phillips lost her professorship after invoking the Fifth Amendment before a McCarthy-era committee, a point that is surely relevant to Becker's observation that "political considerations" play into science. For all the time that Becker spends on Bohm's Marxism and his troubles with the red scare, does the five-year blacklisting of Phillips not merit a line? I know that it is an easy trap for a reviewer to complain that an author did not produce the book that the reviewer would have written, but here it seems that Becker did not write the book that he himself wanted to.

Another name belongs here: Subrahmanyan Chandrasekhar. At one point, Becker goes off on a tangent about gravitational physics, where he mentions some work that Oppenheimer and colleagues did on the gravitational collapse of dead stars (the "Tolman–Oppenheimer–Volkoff limit"). This struck me as a weird thing to mention. It wasn't even the only work that Oppenheimer did on the problem in that year! Why select it for special attention?

By 1939, the year of the Tolman–Oppenheimer–Volkoff limit, gravitational collapse was already a well-established controversy. Arthur Eddington was troubled by the possibility of endless collapse as early as 1924. In 1935, he said, "The star has to go on radiating and radiating and contracting and contracting until … gravity becomes strong enough to hold in the radiation, and the star can at last find peace." He loathed this possibility so much that he wanted "a law of Nature to prevent a star from behaving in this absurd way!"

Eddington rejected Chandrasekhar's conclusion that too-massive white dwarfs would collapse under their own gravity, and his influence delayed the acceptance of Chandrasekhar's work by some years. This is, I'd say, a far better example of a cult of personality holding back scientific progress than any of the events that Becker makes much hay of. Why not include it in your book about the flaws of physics?

Well (you can tell where this is going), Bohr was on Chandrasekhar's side [42].

in that the appearance of philosophy was avoided; the closest approach we had was screening Feynman's Messenger Lectures over the January break. Thus, at first I found Bohr's philosophical opinions irrelevant. At a second encounter, they were obscure; at a third, subtle yet unsatisfying.

[5]Bohm's letters to Phillips are reproduced in C. Talbot's *David Bohm: Causality and Chance, Letters to Three Women* (Springer, 2017). I do not know of letters from Phillips to Bohm being published; it is possible that they have not been preserved.

Becker writes that Oppenheimer and colleagues "used a very early forerunner of a computer" to do their calculations. I guess it's not *wrong* to call a Marchant mechanical desk calculator an "early forerunner of a computer", but it's not exactly the image that sprang to my mind when I read that passage. More importantly, he says that it was "difficult for other physicists to take the idea of collapsed stars seriously". The portrayal in Becker's source, Thorne's *Black Holes & Time Warps* (1994), hits a rather different tone. Physicists and astronomers took the possibility seriously in that they accepted Oppenhemer et al.'s calculations as correct, but they generally figured that some as-yet-unknown physical effect would prevent collapsing stars from going all the way to the black-hole stage.

So, the excursion into gravitation manages to blend "huh? why did you choose to write about *that*?" with "are you trying to do *anything* except carry out character assassination on Niels Bohr?". It is also an example of the genre convention where an author adds to the Feynman legend. (In this book, Feynman is literally "a legend", complete with the requisite Nobel Prize.) Becker credits the "ironclad arguments that finally convinced the physics community that gravitational waves must be real" to Feynman and Bondi, neglecting the contribution of Felix Pirani [43]. Elsewhere, his portrayal of Feynman's role in the field of quantum computation neglects to mention that Paul Benioff had discussed implementing Turing machines with quantum time evolution two years before Feynman's lecture on "Simulating Physics with Computers" [44]. But nobody ever went broke over-estimating the appetite of science-loving boys for Richard Feynman.

One famous Feynmanism that Becker carries over uncritically is that the double-slit experiment "contains the *only* mystery" of quantum mechanics. This was fine in 1964, but it is simply no longer a viable claim. We now know that it is quite simple to construct a theory based on local hidden variables which displays interference in a double-slit-type scenario. (The explanation turns out to be rather pretty: In order to be consistent, the "vacuum state" has to be a statistical distribution over the hidden variables, meaning that it carries an unobservable phase factor which can convey a signal, like hearing the pitch at which an instrument plays a rest.) Simply put, to find the real mystery, we have to dig deeper [45,46].

Becker notes that Feynman's reception of Hugh Everett III's interpretation of quantum mechanics was rather frosty: "The concept of a 'universal wave function' has serious difficulties," he said at the Chapel Hill conference in 1957. Thus it is interesting that when Becker describes the "deep obscurity" in which Everett's imagery languished through the 1960s, he neglects the portrayal that Feynman gave in his 1962 course on gravitation [47]:

> The traditional description of the total quantum mechanics of the world by a complete Monster Wavefunction (which includes all observers) obeying a Schrödinger equation
>
> $$i\frac{\partial\Psi}{\partial t} = \mathbf{H}\Psi$$
>
> implies an incredibly complex infinity of amplitudes. If I am gambling in Las Vegas, and am about to put some money into number twenty-two at roulette, and the girl next to me spills her drink because she sees someone she knows, so that I stop before betting, and twenty-two comes up, I can see that the whole

course of the universe for me has hung on the fact that some little photon hit the nerve ends of her retina. Thus the whole universe bifurcates at each atomic event. Now some people who insist on taking all quantum mechanics to the letter are satisfied with such a picture; since there is no outside observer for a wavefunction describing the whole universe, they maintain that the proper description of the world includes all the amplitudes that thus bifurcate from each atomic event.[6]

One can hardly find a more straightforward presentation of an Everettian view, a view that Feynman apparently takes seriously but does not find compelling. (Feynman's commentary in this lecture about the vagueness of some traditional language about "observers" borders on the caustic; one feels that Becker would approve.) Later in life, Feynman said,

> In fact the physicists have no good point of view. Somebody mumbled something about a many-world picture, and that many-world picture says that the wave function ψ is what's real, and damn the torpedoes if there are so many variables, N^R. All these different worlds and every arrangement of configurations are all there just like our arrangement of configurations, we just happen to be sitting in this one. It's possible, but I'm not very happy with it.

This we read in the very "Simulating Physics with Computers" lecture that Becker describes.[7] I find Feynman's statements over his career difficult to square with Becker's description of a man who "had few qualms about the Copenhagen interpretation" (even leaving aside the ill-definedness of "the Copenhagen interpretation"). If Feynman had "few qualms", they were very precisely calibrated ones that leave no popular interpretive tradition entirely unscathed.

Turning to the topic of Feynman's doctoral advisor, John Archibald Wheeler, we find that according to Becker, Wheeler eventually disdained the ideas of Everett for political reasons:

> Ever the scientific diplomat, Wheeler had tried to maintain his commitment to the ideas of Bohr, his late mentor, without explicitly denouncing the ideas of Everett, his former student. This wasn't a difficult position for him to hold while Everett's work languished in obscurity and remained cloaked in the language of the "relative-state formulation." But now DeWitt was calling Everett's view the "many-worlds" interpretation and saying that Wheeler was partly responsible for it—and the fact that it was showing up in science-fiction magazines didn't help either. So Wheeler publicly distanced himself from Everett's work and DeWitt's spin on it. "[Everett's] infinitely many unobservable worlds make a heavy load of metaphysical baggage," Wheeler wrote in 1979.

According to Peter Byrne's biography of Everett [48], the events happened the other way round:

[6] Because of *course* Feynman's roulette wheel would be in Las Vegas with "a girl".

[7] Erroneously locating it at Caltech, rather than MIT's Endicott House conference center.

> Everett ordered multiple copies of the [December 1976] magazine, sending them to friends, including Wheeler, who had already begun to publicly dissociate himself from the many worlds interpretation. Being praised in *Analog* probably added salt to Wheeler's festering wound.

This may simply be a careless exchange, but I can't help but think that it introduces a minus sign, making Everett look better and Wheeler worse. Whatever his personal motivations, Wheeler made a lengthier case than that to "leave the observer out of the wave function", and he did so in a paper published in 1977, for the proceedings of a conference that took place in 1974 [49]. Already in 1974, Wheeler had published an article in *American Scientist* which lists Everett's relative-state interpretation as only one "discovery about the quantum principle", in a lineage with Planck's quantization of energy and a notch less sophisticated than Birkhoff and von Neumann's quantum logic [50].

Becker writes, "After his failure to reconcile his student Everett's work with the ideas of his mentor Bohr, Wheeler had set aside his interest in the foundations of quantum theory. But the Bell experiments, as well as long talks with Eugene Wigner, his colleague at Princeton, had brought Wheeler back to his former interest in the subject." I find the causation here implausible. Everything I have read of Wheeler's papers suggests that it was not the Bell experiments that brought his attention back to quantum theory, but the prevalence of singularities in his gravitation research, undoing the calm image of a world built from clean spacetime geometry, and leading him to the notion of "law without law". Becker cites Freire's *The Quantum Dissidents* (2015) for this historical claim, but Freire gives only an association, not a claim of causality:

> In the early 1970s the quantum foundations illness that had once inflicted Wheeler came back. The buzz around Bell's theorem experiments was brought to him by his colleague at Princeton, Wigner, and he got involved with Edward Fry's experiment (Misner et al. 2009, p. 45).

Chasing down Misner, Thorne and Zurek's 2009 feature in *Physics Today* [51], we find that "got involved with" does not quite summarize what they write:

> There was great excitement in Wheeler's group when the violation [of Bell's inequality] was indeed reported in an experiment by Edward Fry of Texas A&M University, and even more so when Alain Aspect confirmed its violation in measurements that are spacelike separated and hence causally independent.

Fry's result was published in 1976 [52], too late to be considered a causal contributor to the relapse of "quantum foundations illness" in the "early 1970s". Moreover, Misner, Thorne and Zurek only mention Wigner as a participant in Wheeler's graduate course on quantum measurement, which he first announced in the fall of 1977.

What we have here is rather remarkable: a myth generated by citation telephone, with the chain beginning in a widely-read professional magazine only a decade ago. (Myths are empirically part of the weaving of history, insofar as we cannot understand its course without accounting for their consequences.)

Becker portrays Dirac rather favorably. As mentioned above, Dirac criticized the vagueness of "complementarity"; his prediction of antimatter is credited as a "great success" of quantum field theory, and his Nobel Prize is duly mentioned.[8] Nowhere does Becker hint that historians and philosophers of physics have seen in Dirac a positivist or instrumentalist inclination [53]. If the assertion of scattered philosophers is enough warrant to tag Bohr with the p-word, why is the same not true for Dirac? True, the charges against Dirac have been disputed [54], but so too are those for Bohr. Likewise, Becker gives Schrödinger a bit of a hero treatment: He was the one who could have opposed Bohr's vagueness at the Como conference, had he only been there, etc. But read Schrödinger's *Mind and Matter* and *My View of the World* — Erwin was *weird*. People call Bohr opaque and confused, but never admit that Schrödinger is out there finding inspiration in the Upanishads.[9]

And speaking of the 1927 Como conference: despite Becker's portrayal, Bohr probably did *not* discuss the thought-experiment known as the "gamma-ray microscope". He added that to the written version of his talk, months later [56]. Becker claims that Bohr using the gamma-ray microscope to support his view is strange, because the thought-experiment, due to Heisenberg, presumes that position and momentum exist before they are measured, and are just disrupted by the measurement process. But if you actually read Bohr's lecture, it's clear that he is using the gamma-ray microscope as a warm-up example at the beginning of a section. After explaining it, Bohr goes on to say that a "closer investigation of the possibilities of definition would still seem necessary in order to bring out" the real conceptual issues [57]. He's perfectly aware of the shortcomings; he's the one who told Heisenberg about them.

This bit of science history is so well known, it's in a play.

In Michael Frayn's *Copenhagen*, the ghost of Heisenberg and those of Niels and Margrethe Bohr look back over their lives and try to make sense of them. This thought-experiment comes up, and Bohr explains how Heisenberg's analysis fell short. Heisenberg whines, "I know — I put it in a postscript to my paper."

Niels Bohr deadpans, "Everyone remembers the paper — no one remembers the postscript."

When I saw *Copenhagen*, Margrethe Bohr was played by Mariette Hartley, whose character Zarabeth taught Spock to eat meat in "All Our Yesterdays". She was also in *Columbo* twice. So, [very Peter Falk voice] just one more thing:

Have you ever known something obscure that you wanted the world to share — like a book of poetry that moved you, one you found in the dustiest corner of a secondhand store — and then seen someone promote it widely while getting it horribly wrong? That, I am afraid, was my reaction to Becker's treatment of the remarkable mathematical achievement known as Gleason's theorem.

Becker's description of Andrew Gleason's work is an example of "bait in the text and switch in the notes" [1]. In the main text, he portrays Gleason's theorem as a no-go proof

[8] Should a prediction based on the "Dirac sea" of negative-energy electrons be considered a triumph of QFT per se? Should Dirac be called the one who "originally pioneered" QFT when the quantization of free electromagnetic fields predates the Dirac equation? You see the paranoia into which one descends while reading *What Is Real?*, where the smallest detail soon provokes distrust.

[9] According to Helge Kragh, Bohr nominated Schrödinger for the Nobel Prize, and it was the weight of his recommendation that secured the award [55].

for hidden variables, part of the "ilk" of von Neumann's flawed result. In the endnotes, he clarifies,

> Gleason's proof actually didn't mention hidden variables. Gleason was a mathematician, not a physicist, and his proof had to do with certain features of Hilbert space, the mathematical structure underlying quantum physics. But Jauch and his colleague Piron pointed out to Bell that Gleason's proof had an obvious corollary that ruled out hidden variables — and that this corollary seemed far stronger than von Neumann's proof, or their own.

Presenting the corollary as the result is, in this case, a serious error.

Gleason's theorem begins with a brief list of postulates, which are conditions for expressing what "measurements" are, in terms of Hilbert spaces. To each physical system we associate a Hilbert space, and each possible measurement we can perform on that system corresponds to a particular mathematical gadget — in Gleason's original version, to an orthonormal basis [58]. The conclusion of Gleason's argument is that any mapping from measurements to probabilities that satisfies his assumptions must take the form of the Born rule applied to some density operator. In other words, the theorem gives the set of valid quantum states and the rule for calculating probabilities given a state.

Now, one corollary of this result is a statement that intrinsic hidden variables don't work as an explanation for quantum probabilities. But that's not how Gleason set up the problem, and it's not the primary reason that the theorem has been invoked since then. Instead, the motivation is to *re-derive the mathematical apparatus of quantum theory* given only a portion of it. Specifically, we start in the setting of Hilbert space, and we *make a presumption about what means "measurement".* From this starting point, Gleason tells us how to rebuild the rest of the quantum-mechanical formalism. Crucially, measurement is primary, and the set of legitimate quantum states is secondary. Notice how this runs spiritually counter to the Everettian approaches, in which the Monster Wavefunction is all, and measurements are convenient illusions (the details of the justification varying with the particular brand of Everettism). Becker seems almost allergic to the idea of taking "measurement" as a conceptual primitive, an attitude that, to be fair, I expect that many physicists share. But a clear-cut presentation of Gleason's theorem shows that it is, at the very least, mathematically fruitful to embrace this heterodoxy.

In setting up the premises of Gleason's theorem, we invoked the notion of Hilbert space. It is quite natural to ask whether the need for Hilbert space can be deduced from a still earlier starting point. This has been a major concern for the "quantum logic" community, and one of the most significant discoveries in the project of getting to the place where Gleason can take over is a 1995 theorem by Maria Pia Solèr [59].

I could go on. For example, in multiple places, Becker mixes up who was a student and who was a postdoctoral researcher or other type of colleague. If you can't explain the academic career ladder, how can you hope to explain the mysteries of quantum mechanics?

In summary: Errors pervade this book like the smell of turpentine. It is possible that the vaguenesses, the omissions and the misleading portrayals are no worse than many other books. Perhaps, when stacked up against the sins physicists normally commit when brushing past the history to get to the harmonic oscillator, this book is only typical [10]. But usually physicists serve up the folk tales as an incidental part of something else, and this isn't section 1.1 of a textbook. It is packaged and sold as a serious investigation into the history and philosophy of quantum physics.

> I tried to argue that we did not understand the status of the superposition principle. Why are pure states described as [rays] in a complex linear space? Approximation or deep principle? Niels Bohr did not understand why I should worry about this. Aage Bohr tried to explain to his father that I hoped to get inspiration about the direction for the development of the theory by analyzing the existing formal structure. Niels Bohr retorted: "But this is very foolish. There is no inspiration besides the results of the experiments." I guess he did not mean that so absolutely but he was just annoyed. [...] Five years later I met Niels Bohr in Princeton at a dinner in the house of Eugene Wigner. When I drove him afterwards to his hotel I apologized for my precocious behaviour in Copenhagen. He just waved it away saying: "We all have our opinions."
>
> — Rudolf Haag [60]

3 The literature of prior reviews

All that said, I wouldn't say that I agree with every position taken by those who have critiqued Becker. For example, while I think Peter Woit makes several good points in his review [61], the roughly Zurekian position where he ends up is not one I find satisfying. Or, rather, it is one I grew increasingly discontented with as my interest in quantum foundations intensified. While we could get into technical considerations here [62], there is a simpler emotional core. Ultimately, like Bohr's pronouncements, the Zurekian zeitgeist assumes what I would like to see explained [63].

For a second example, I'm not satisfied with the "shut up and calculate" attitude that Sheldon Glashow endorses in his review [64]. As I wrote in a commentary upon another book [65]:

> "Shut up and calculate!" is not a stable position. Even the most ascetic claim — the assertion to shut up and calculate with one mathematical formalism rather than another — is in some way a claim about the character of the world. Perhaps bound up with historical happenstance and social convention, but a claim about Nature nonetheless: Were the world a different way, would we not, after we shut up, calculate in a different fashion?

The teachers I recall fondly were not the ones who declared, "You'll learn it because it will be on the test, that's why!" Why do physicists calculate in the particular manner that our profession learns to do, instead of any other? Why, out of all the mental contrivances that the mathematicians have thought up or stumbled across, do we use this specific set of arcana?

Some statements in the opening chapters of Quantum 101 go unchallenged as we progress onward, through quantum field theory into Higgs-boson territory, right up to the frontier, where people are trying to devise quantum gravity. Think on it: Physicists are trying to make a quantum mechanics of space and time themselves, using rules invented for hot gases in glass tubes! Is this merely a failure of imagination, or are there good reasons — physics reasons — why the mathematical alternatives should be kept on the sidelines? This is not an idle question, but a matter of increasingly active research.[10] I regret to say that Becker elides this. For example, he dings Anton Zeilinger for offering "positivist" vagueness about quantum measurement. Yet he ignores the actual mathematics that Zeilinger and Brukner did to try and start making those intuitions precise [68]. Overall, this elision provides a skewed picture of how the physics community actually regards the founders of quantum mechanics.

Glashow is right to raise the question, "What's so remarkable about radioactive half-lives?" We should in fact be willing to carry this line of questioning further. For example, is Pauli exclusion really so mysterious as all that? Chemistry students take on board the filling of electron shells as one more rule, with no more mystique than a table of electronegativities. To identify what is truly enigmatic about quantum mechanics, to pinpoint those "quantum mysteries" that genuinely deserve the name, we have to define in a robust way what we mean by "classical", and then we have to prove a theorem that no model which satisfies that criterion can reproduce the predictions of quantum theory.

Glashow points out the prominence of Schrödinger's cat in Becker's book. Like Glashow, I'm not incredibly impressed by this thought-experiment, but my sense that it says nothing terribly deep about quantum physics may stem from a different source. Schrödinger's cat is full of red herring. It is a deceptive little fiction that does great violence to the words from which it is built. A living cat breathes. Indeed, this is a vital part of what it means to be a living cat. By definition, a living cat is a system that can exchange 100 billion billion molecules with its environment and still be considered "the same cat". This is simply not a system for which any physicist can have expectations so precise that they call for the full apparatus of quantum theory to manage.

Another problem with Schrödinger's cat, on top of its flagrant disregard for the meaning of "cat", is that it relies upon a single question that admits of only a yes-or-no answer. You could pose the same "paradox" for any theory expressed in probabilistic terms. It says nothing substantial about the particular features of quantum theory that make it truly special. In order to get at those aspects of the physics, we must have more flexibility in the ways that we can interrogate a physical system. We need to be able to ask a variety of questions that have at least three possible answers apiece. If we are restricted to binary experiments, we have to be able to combine them, choosing one binary interrogation for one system and another for a second. (These are statements of the Kochen–Specker and Bell theorems, respectively, with the equations stripped out to protect the innocent [69].) And when we do get at those deeper mysteries, what do they tell us about nature? Well, put four rabbis in a room and you might get five opinions, but this is where my colleagues and

[10] For one perspective, see [66]; for another, try [67].

I have arrived, after many physicist-years on the question [70–72]:

It is not always the case that facts are there on the ground, waiting for us to come along and read them off. In order to be satisfactory, science also has to encompass the situations when the "outcome of a measurement" is a thing a scientist elicits, an event that is a consequence of an active intervention upon the world. This goes deeper than the possibility of disturbing what was already there: At root, the choice of performing one experiment over another is the choice of instantiating one or another arena for fact creation. What a world to have evolved in! But we have a handbook for managing our expectations as we interact with it, a handbook that any individual among us can pick up and use: It is called quantum theory.

One of the great problems of philosophy is how we, any of us, can obtain something that can play the role of solid truth, starting from anything so personal as experience. Quantum physics suggests something remarkable: that there is a gradation to the answer, and that it is quantifiable. In plenty of situations, it would be folly to act in any way other than as though a measurement simply tallied what was already present — the number of people standing on the National Mall, for example. Yet the poets have something legitimate, too, when they ask whether the tree that one of us sees is the same tree seen by any other. What happens to Alice, is an event for Alice. Should Bob try to share in Alice's personal experience, what he gets is an experience of his own, standing in the same relation to the original, we might say, as a sonnet does to love. Science does not have to collapse in the face of this. On the contrary, it might even prosper. While hardly knowing it, we have been developing the tools to handle exactly this eventuality — and it started with hot gases in glass tubes.

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