

# Annotated Interview with a QBist in the Making

David Mermin

January 28, 2013

# Contents

<b>1</b>	<b>Introduction</b>	<b>1</b>
<b>2</b>	<b>The Seventeen Questions of Maximilian Schlosshauer</b>	<b>2</b>
2.1	Q1. What first stimulated your interest in the foundations of quantum mechanics? . . . . .	2
2.2	Q2. What are the most pressing problems in the foundations of quantum mechanics today? . . . . .	3
2.3	Q3. What interpretive program can make the best sense of quantum mechanics, and why? . . . . .	6
2.4	What are quantum states? . . . . .	7
2.5	Does quantum mechanics imply irreducible randomness in nature? . . . . .	8
2.6	Quantum probabilities: subjective or objective? . . . . .	8
2.7	The quantum measurement problem: serious roadblock or dissolvable pseudo-issue? . . . . .	10
2.8	What do the experimentally observed violations of Bells inequalities tell us about nature? . . . . .	12
2.9	What contributions to the foundations of quantum mechanics have or may come from quantum-information theory? What notion of 'information' could serve as a rigorous basis for progress in foundations? . . . . .	14
2.10	How can the foundations of quantum mechanics benefit from approaches that reconstruct quantum mechanics from fundamental principles? Can reconstruction reduce the need for interpretation? . . . . .	15
2.11	If you could choose one experiment, regardless of its current technical feasibility, to help answer a foundational question, which one would it be? . . . . .	16
2.12	If you have a preferred interpretation of quantum mechanics, what would it take to make you switch sides? . . . . .	17
2.13	How do personal beliefs and values influence one's choice of interpretation? . . . . .	18
2.14	What is the role of philosophy in advancing our understanding of the foundations of quantum mechanics? . . . . .	19

2.15	Q15. What new input and perspectives for the foundations of quantum mechanics may come from the interplay between quantum theory and gravity/relativity, and from the search for a unified theory? . . . . .	20
2.16	Where would you put your money when it comes to predicting the next major development in the foundations of quantum mechanics? . . . . .	20
2.17	What single question about the foundations of quantum mechanics would you put to an omniscient being? . . . . .	23

## Abstract

These are my published answers to the seventeen questions posed by Max Schlosshauer in his 2011 book *Elegance and Enigma: the Quantum Interviews*. I have inserted thirty footnotes into those answers, commenting on them from the perspective of subsequent insights I acquired during six weeks in March and April of 2012 with Chris Fuchs and Ruediger Schack at the Stellenbosch Institute for Advanced Study. Aside from the footnotes and a few introductory paragraphs, the text is identical to my contributions to the Schlosshauer volume.

## 1 Introduction

My title imitates that of arXiv:1207.2141, “Interview with a Quantum Bayesian”, by Christopher A. Fuchs. Mine differs from his in several ways. I prefer Fuchss term “QBist” because Fuchs’s view of quantum mechanics differs from others as radically as cubism differs from renaissance painting, and because I find his term “quantum Bayesianism” too broad. QBism explores the consequences for the interpretation of quantum mechanics of a thorough-going subjective view of probability, as an agent’s measure of her own personal degree of belief. But there are Bayesians who take probabilities to reflect objective facts about an event rather than subjective judgments of an agent. I would prefer to name Fuchs’s perspective not after Thomas Bayes, but after that eloquent pioneer of subjective probability, Bruno de Finetti: Quantum Bruno-de-Finettiism. Still QBism, but with the “B” for Bruno, not Bayes.

Another difference in our titles is that while Fuchs describes himself as a QBist, the author of the text below was not yet there. Max Schlosshauer asked seventeen questions of seventeen people and published the 289 answers, question by question, in his book *Elegance and Enigma: the Quantum Interviews*, Springer, 2011. My answers to Schlosshauer’s questions were finished in April 2011, a year before I spent six weeks in Stellenbosch at STIAS with Fuchs and Ruediger Schack, and finally began to understand what they had been trying to tell me for the past ten years. (I published a summary of my Stellenbosch epiphany as a Commentary in the July 2012 *Physics Today*. That Commentary elicited several letters to the editor, mostly critical, which were published in December 2012, together with my further comments.)

In July 2012, Fuchs posted his own answers to Schlosshauer’s seventeen questions at arXiv and urged me to post mine. I hesitated. By posting nothing at arXiv since 2008, I had ceased to qualify as an endorser, an appropriate loss of status for a retired country gentleman that I was anxious not to reverse. Recently the authorities at arXiv removed this concern by accepting my request to make my dequalification permanent. I was also worried that many of the views I expressed in Schlosshauer’s book had changed since Stellenbosch. But, rather to my surprise, I discovered that few of them had. Fortunately Schlosshauer prohibited footnotes in his book, making it easy for me to maintain verbatim my original 2011 text, while comparing it with my post-Stellenbosch views of early 2013.

My annotated replies to Schlosshauer’s questions are as follows:

## **2 The Seventeen Questions of Maximilian Schlosshauer**

### **2.1 Q1. What first stimulated your interest in the foundations of quantum mechanics?**

I’ve always been more fascinated by physics as a conceptual structure than by physics as a set of rules for calculating the behavior of the natural world - what Suman Seth calls the “physics of principles”, as opposed to the “physics of problems”. My text with Neil Ashcroft on solid-state physics is a success because Neil is as focused on the physics of problems as I am on the physics of principles. Somehow we managed to produce a book that combines both views.

The conceptual structure of quantum mechanics is stranger and lovelier than any perspective on the world that I know of, so I’ve been fascinated and worried about it from the beginning of my career in physics. Indeed I became interested in my early teens in the late 1940s long before I knew enough mathematics to learn the quantum formalism, through the popular writings of George Gamow, Arthur Eddington, and James Jeans. In college, I put these interests on hold, majoring in mathematics and taking only a few courses in (classical) physics.

But I returned to physics in graduate school, where my revived curiosity

about quantum foundations was actively discouraged by my teachers. To my disappointment, the Harvard physicists all believed that a preliminary training in the physics of problems was a prerequisite to any understanding of the physics of principles. So, for a quarter of a century, I was deflected full-time into statistical physics, low-temperature physics, and solid-state physics, using (and teaching) quantum mechanics as a beautiful and effective body of rules for manipulating symbols on a page to get answers to questions about experiments in the laboratory. “Shut up and calculate!”

Early in graduate school, Gordon Baym, a fellow student, told me at the Hayes-Bickford cafeteria about Bohm’s spin-1/2 version of Einstein, Podolsky, and Rosen. EPR was never mentioned in any official academic setting. I immediately concluded that the quantum-mechanical description of physical reality was incomplete, and I made a note to think about completing it when I got tired of the serious pursuits my teachers had set me on. (After my oral qualifying exam, Roy Glauber advised me to stop spending so much time with Gordon Baym. The senior members of my committee, Wendell Furry and Julian Schwinger, seemed to agree.)

More than two decades later, in 1979, fifteen years after John Bell’s now-famous paper appeared, I learned about Bell’s theorem through the pages of the *Scientific American*. I believe Tony Leggett had tried to tell me about it a few years earlier, but I was too busy with real physics to pay attention. I realized to my astonishment that the more complete theory that EPR had convinced me would someday be found to underly quantum mechanics, resolving all its mysteries, either did not exist or, if it did, would be at least as mysterious. In the three decades since then, I’ve devoted a significant fraction of my intellectual efforts to pondering such puzzles, mainly trying to boil them down to their simplest possible forms.

## 2.2 Q2. What are the most pressing problems in the foundations of quantum mechanics today?

1. In the words of Chris Fuchs,<sup>1</sup> “quantum states: what the hell are they?” Quantum states are not objective properties of the systems they de-

---

<sup>1</sup>I’m pleased to see that I cite the Master himself, in listing the most pressing of the problems.

scribe, as mass is an objective property of a stone. Given a single stone, about which you know nothing, you can determine its mass to a high precision. Given a single photon, in a pure polarization state about which you know nothing, you can learn very little about what that polarization was. (I say “was”, and not “is”, because the effort to learn the polarization generally results in a new state, but that is not the point here.)

But I also find it implausible that (pure) quantum states are nothing more than provisional guesses for what is likely to happen when the system is appropriately probed. Surely they are constrained by known features of the past history of the system to which the state has been assigned, though I grant there is room for maneuver in deciding what it means to “know” a “feature”.<sup>2</sup>

Consistent historians (see also my answer to Question 16) maintain that the quantum state of a system *is* a real property of that system, though its reality is with respect to an appropriate “framework” of projectors that includes the projector on that state. Since the reality of most other physical properties is also only with respect to suitable frameworks, for consistent historians the quantum state of a system is on a similar conceptual footing to most of its other physical properties.<sup>3</sup> Quantum cosmologists maintain that the entire universe has an objective pure quantum state. I do not share this view. Indeed, I do not believe it has a quantum state in any sense, since there is nothing (nobody) outside the entire universe to make that state assignment.

<sup>4</sup> Well, I suppose it could be God, but why would he want to make state assignments? Einstein has assured us that he doesn’t place bets.

<sup>5</sup> (See also my answer to Question 4.)

2. How clearly and convincingly to exorcise nonlocality from the foundations of physics in spite of the violations of Bell inequalities. Nonlocality has been egregiously oversold.<sup>6</sup> On the other hand, those who

---

<sup>2</sup>This is an anti-QBist sentiment with, however, a tip of the hat to QBism at the end of the sentence.

<sup>3</sup>From a Qbist perspective this is a deficiency.

<sup>4</sup>This is strictly a QBist view, very concisely put.

<sup>5</sup>A direct reference to the Dutch-book view of probability dear to QBists.

<sup>6</sup>Another position of QBists and many others.

briskly dismiss it as a naive error are evading a direct confrontation with one of the central peculiarities of quantum physics. I would put the issue like this: what can one legitimately require of an *explanation* of correlations between the outcomes of independently selected tests performed on systems that no longer interact? (See also my answer to Question 8.)

3. Is the experience of personal consciousness beyond the reach of physical theory as a matter of principle? Is the scope of physics limited to constructing “relations between the manifold aspects of our experience”, as Bohr maintained? While I believe that the answer to both questions is yes, I list them as problems, because most physicists vehemently reject such views, and I am unable to explain to them why they are wrong in a way that satisfies me, let alone them.<sup>7</sup>

I regard this last issue as a problem in the interpretation of quantum mechanics, even though I do not believe that consciousness (as a physical phenomenon) collapses (as a physical process) the wave packet (as an objective physical entity). But because I do believe that physics is a tool to help us find powerful and concise expressions of correlations among features of our experience, it makes no sense to apply quantum mechanics (or any other form of physics) to our very awareness of that experience.<sup>8</sup> Adherents of the many-worlds interpretation make this mistake. So do those who believe that conscious awareness can ultimately be reduced to physics, unless they believe that the reduction will be to a novel form of physics that transcends our current understanding, in which case, as Rudolf Peierls remarked, whether such an explanation should count as “physical” is just a matter of terminology.

I am also intrigued by the view of Schrödinger (in *Nature and the Greeks*) that it was a mistake dating back to the birth of science to exclude us, the perceiving subjects, from our understanding of the external world. This does not mean that our perceptions must be parts of the world external to us, but that those perceptions underlie

---

<sup>7</sup>I would call this QBism. My new QBist explanation of why most physicists are wrong comes closer to satisfying me, but most of them are as dissatisfied as ever.

<sup>8</sup>QBism. Today I would sharpen it to “It makes no sense for anybody to apply quantum mechanics to his or her very own awareness of that experience”. You can apply it to what you believe about the experience of somebody else.

everything we can know about that world. (See also my answer to Question 14.) Until the arrival of quantum mechanics, physics made good sense in spite of this historic exclusion. Quantum mechanics has (or should have) forced us to rethink the importance of the relation between subject and object.<sup>9</sup>

### 2.3 Q3. What interpretive program can make the best sense of quantum mechanics, and why?

My sympathies are with those, going all the way back to Heisenberg and Peierls, who maintain that quantum mechanics is a set of rules for organizing our knowledge with a view to improving our ability to anticipate subsequently acquired knowledge. By “our knowledge”, I mean my own knowledge combined with whatever other people are able to communicate to me of their own knowledge. I take this commonality of scientific knowledge to be one of the reasons why Bohr placed such emphasis on what can be expressed in ordinary language.<sup>10</sup>

To John Bell’s “Knowledge about what?” I would say knowledge about our perceptions - ultimately our direct, irreducible mental perceptions, which can, of course, be refined by the use of instruments devised for that purpose. To his “Whose knowledge?” I would say knowledge of whoever is making use of quantum mechanics. Different users with different perceptions may well assign different quantum states to the same physical system. What consistency requirements, if any, can be imposed on such descriptions, is an entertaining question.<sup>11</sup> I have had some disagreements with some of my friends about this,<sup>12</sup> as described in “Compatibility of state assignments”, which I cite here because it cannot be found in the primary repository, arXiv, but only in the Journal of Mathematical Physics (43, 4560-66 (2002)).

---

<sup>9</sup>I’m pleased and surprised to discover myself writing this very QBist paragraph back in my dark pre-QBist days.

<sup>10</sup>Replace “knowledge” with “belief” and this becomes a very QBistic paragraph

<sup>11</sup>QBism again, if you replace “perceptions” by “experience” and “knowledge” by “belief”.

<sup>12</sup>Actually I elicited a ferocious QBist attack, which I flatter myself had an impact on the subsequent development of QBism. See Caves, Fuchs, and Schack, “Conditions for compatibility of quantum state assignments”, quant-ph:0206110, published as Phys. Rev. A66, 062111 (2002). I hereby make a note to reexamine what, if anything, of my original point survives my conversion.

My answer to “Why?” has to be inferred from my answers to most of the other sixteen questions.

## 2.4 What are quantum states?

The first of my answers to Question 2 primarily says what quantum states are not. It is harder to say what they are. I am intrigued by the fact that if quantum mechanics applied only to digital quantum computers, then the answer would be entirely straightforward. Quantum states are mathematical symbols. The symbols enable us to calculate, from the (explicit, unproblematic) prior history of a collection of Qbits - I commend to the reader this attractive abbreviation of “qubit” - the probabilities of the readings (0 or 1) of a collection of one-Qbit measurement gates to which the Qbits are then subjected. This procedure is made unambiguous by the rule that a Qbit emerging from a one-Qbit measurement gate reading 0 (or 1) is assigned the state  $|0\rangle$  (or  $|1\rangle$ ). This makes it possible to assign initial states with the help of one-Qbit measurement gates. Additional rules associate specific unitary transformations of the states of the Qbit(s) with the action of the other subsequent gates that appear in a computation.

Quantum states, in other words, are bookkeeping tools that enable one to calculate, from a knowledge of the initial preparation and the fields acting on a system, the probability of the outcomes of measurements on that system.<sup>13</sup> This is what I take to be the Copenhagen interpretation of quantum mechanics. (I hereby renounce my earlier summary of Copenhagen, widely misattributed to Richard Feynman, as “shut up and calculate”). If the only application of quantum mechanics were to the operation of digital quantum computers, there would be no ambiguity or controversy about Copenhagen.

The Copenhagen view fits quantum computation so well that I am persuaded that quantum states are, even in broader physical contexts, calculational tools, invented and used by physicists to enable them to predict correlations among their perceptions.<sup>14</sup> I realize that others have used their experience with quantum computation to make similar arguments on behalf of

---

<sup>13</sup>My QBist friends don’t like this. At a minimum they would replace “knowledge of” with “belief about”.

<sup>14</sup>My QBist friends would not object to this, particularly if “perceptions” was changed to “experience”.

many worlds (David Deutsch) and consistent histories (Bob Griffiths). I would challenge them to make their preferred points of view the basis for a quick practical pedagogical approach to quantum computation for computer scientists who know no physics, as I have done with Copenhagen in my quantum-computation book. The approach to quantum mechanics via consistent histories in Griffiths's book, while something of a tour de force, does not strike me as either quick or practical.

## 2.5 Does quantum mechanics imply irreducible randomness in nature?

Yes. But “in nature” requires expansion. A more precise formulation would be that quantum mechanics implies irreducible randomness in the answers to most of the questions that we can put to nature. The probability of a photon that has emerged from a vertically oriented sheet of polaroid getting through one oriented at forty-five degrees from the vertical is irreducibly one half, as is the probability of a slow-moving mu meson turning into an electron and a pair of neutrinos in the next microsecond-and-a-half. “Irreducible” means there is nothing we can condition the probabilities on that would sharpen them up.<sup>15</sup>

Can you exploit quantum physics to make an ideal random-number generator? <sup>16</sup> A distinguished Cornell computer scientist once made the long trek from the Engineering Quad to my physics-department office in the heart of the Arts College to ask me this question. He had been told this by a student, and didn't believe him. I said the student was right. I don't think he believed me either.

## 2.6 Quantum probabilities: subjective or objective?

<sup>17</sup> In a message in a bottle that I tossed into the sea about fifteen years ago - the “Ithaca Interpretation of Quantum Mechanics” (IIQM) - I firmly declared quantum probabilities to be objective properties of the physical world. The

---

<sup>15</sup>I seem here to be flirting with objective probabilities.

<sup>16</sup>The QBist answer to this question is not obvious. It is not even clear what, if anything, my question means to one who takes a subjective view of probability.

<sup>17</sup>My answer is an interesting (to me) pre-Stellenbosch attempt to reconcile my IIQM with Fuchs's early QBism.

bottle was noticed by Chris Fuchs, who introduced me to subjective probabilities and to his collaborators Carl Caves and Rüdiger Schack. I found their point of view so intriguing that I have left the bottle adrift ever since, but in thinking about it today, I wonder why I was so readily persuaded that their view of probability was incompatible with mine.

In declaring quantum probabilities to be objective, I had in mind two things. First, that the role of probability in quantum mechanics is fundamental and irreducible. Probability is not there just as a way of coping with our ignorance of the underlying details, as in classical statistical mechanics. It is an inherent part of how we can understand and deal with the world. Second, that probabilistic assertions are meaningful for individual systems, and not just, as many physicists would maintain, for ensembles of “identically prepared” systems. I believe Fuchs *et al.* would agree with both propositions.

I also explicitly rejected Karl Popper’s promotion of “propensities” into objective properties of the systems they describe. It was not my intent to reify probability, or if it was - fifteen years later it is hard to be sure - I hereby disassociate myself from the foolish person I might then have been. Admittedly, my IIQM motto that “correlations have physical reality” (though correlata do not) sounds dangerously like a Popperian reification of probability. But it is not. In my two IIQM papers, I used the phrase “has physical reality” to mean “can be accounted for in a physical theory”, particularly when I insisted that conscious experience has reality, but not physical reality.

Thinking about this today, I see that to be compatible with the point of view of Fuchs *et al.*, I should also have maintained that correlations have physical reality but not reality. “Physical reality” is not, as I seem to have implicitly maintained fifteen years ago, just a subset of “reality”. Neither is contained in the other. Conscious awareness belongs to reality and not to physical reality, but correlation belongs to physical reality and not to reality. Putting it like that, I now see that this goes a way toward reconciling the IIQM not only with Fuchs *et al.*, but also with Adán Cabello’s demonstration that whatever the sense in which correlations have physical reality, it cannot be that their values are EPR “elements of reality”,

So I would say that quantum probabilities are objective in the sense that they are unavoidable. They are intrinsic features of the quantum formalism

- not just an expression of our ignorance. And they apply to individual systems and are not just bookkeeping devices for cataloguing the behavior of ensembles of identically prepared systems.

But because quantum mechanics is our best strategy for organizing our perceptions of the world, quantum probabilities have a strategic aspect. Strategy implies a strategist, and in that sense quantum probabilities are subjective.  
18

Strategic as the use of probability may be, the fact that a free neutron has a slightly less than fifty-fifty chance of decaying within the next ten minutes strikes me as just as objective a property of the neutron as the fact that its mass is a little less than 1,839 times the mass of an electron. Of course, one can, and some of my friends do, conclude from this that dynamics itself (in which mass is a parameter, and out of which emerges the half-life) is as subjective a matter as probability. Wary as I am of reification, I'm not ready to take that step.<sup>19</sup>

## **2.7 The quantum measurement problem: serious roadblock or dissolvable pseudo-issue?**

It's a pseudoissue. But I have not dissolved it entirely to my satisfaction. So while I see no roadblock, I do feel the need to drive slowly past some unfinished construction, attending to signals from the people with flags.

Today "the quantum measurement problem" has almost as many meanings as "the Copenhagen interpretation". I mention only two of them. The first is how to account for an objective physical process called the collapse of the wave function, which supersedes the normal unitary time evolution of the quantum state in special physical processes known as measurements. I believe that this version of the problem is based on an inappropriate reification of the quantum state. So are efforts to eliminate the special role of measurement through dynamical modifications in standard quantum mechanics that make an appropriate rate of collapse an ongoing physical process under all conditions.

---

<sup>18</sup>I would like to think this is a QBist sentiment.

<sup>19</sup>A rare anti-QBist position.

The quantum state is a calculational device, enabling us to compute the probabilities of our subsequent experience on the basis of earlier experiences. Collapse is nothing more than the updating of that calculational device on the basis of additional experience.<sup>20</sup> This point of view is the key to resolving this form of the quantum measurement problem. I look forward to the day when some clear-headed gifted writer has spelled it out so lucidly that everybody is completely convinced that there is no such problem. (I'm convinced. But I'm not completely convinced. <sup>21</sup>)

A second question going under the name “quantum measurement problem” is whether there can be quantum interference between quantum states that describe macroscopically distinct physical conditions (sometimes called “cat states”). If such interference is not just hard to observe but strictly absent, then quantum mechanics must break down in its answers to questions of sufficient complexity, asked of systems of sufficient size. Size alone is not the issue, since quantum mechanics works brilliantly in accounting for all kinds of classically inexplicable behavior in the gross behavior of bulk materials. Indeed, the appropriate definition of “macroscopic” in this setting is far from obvious.

The fact that the unavoidable entanglement of a macroscopic system with its environment renders manifestations of quantum interference effectively unobservable is a good practical rejoinder to those who seek an answer from a macroscopic breakdown of quantum mechanics. But decoherence does not directly address the question of whether anything actually changes when the superposition is replaced by a mixed state, beyond an abstract representation of our practical ability to acquire knowledge. And it is subject to the same kinds of time-reversal problems that plague statistical-mechanical derivations of the second law.

Seeing quantum interference effects with carbon-60 molecules is an experimental tour de force. But I would have been astonished if interference had been demonstrably absent. My impression is that those who did the experiment did not expect it to reveal a breakdown of quantum mechanics. They did it because it was there, like Mount Everest, challenging somebody to take it on.

---

<sup>20</sup>Quintessential QBism!

<sup>21</sup>Now I am

## 2.8 What do the experimentally observed violations of Bells inequalities tell us about nature?

They tell us something strange about correlations in the outcomes of certain sets of local tests, independently chosen to be performed on far-apart noninteracting physical systems, which may have interacted in the past but no longer do. Prior to Bell's analysis of such quantum-theoretic correlations (and the experimental confirmation of those theoretic predictions), it seemed reasonable to assume that correlations in the outcomes of such tests could find an explanation in correlations in the conditions prevailing at the sites of the tests. Such local conditions can include individual features of the locally tested system, acquired at the time of its past interaction with the other systems; the conditions can also include the weather at the place of the test, the time of each local test, and so on.

Such local explanations can indeed be constructed for any single choice of which local test to perform on each system. But if there is more than one choice of test for each system, then there can be circumstances (revealed by a violation of an appropriate Bell inequality) in which no single explanation, based on correlation in the locally prevailing conditions, works for all possible choices of local test, even if the choices of local test are made randomly and independently in each local region. This is strange, because the local conditions prevailing at the site of any particular test cannot depend on a random choice of what test to perform far away from that site.

Failure of a Bell inequality fatally undermines the view that all the correlations in all the possible tests can find a single explanation in terms of correlations in conditions at the sites of the tests. The conclusions people draw from this vary widely. Those who conclude that the choice of what test to perform in one region does affect the prevailing conditions in the other regions (as it does explicitly in the de Broglie-Bohm pilot-wave interpretation) have embraced nonlocality.

A more conservative conclusion is that it is unreasonable to demand a single explanation that works not only for the choices of test that were actually made in each region, but also for the choices of test that might have been made but were not. This is the conclusion of that subset of the quantum-information community with which I sympathize. It is also the conclusion of consistent

historians (see my answer to Question 16), but their apparent conservatism hides their ontologically radical insistence that all the explanations give correct accounts of the tests to which they apply, subject to the proviso that you cannot combine ingredients of one explanation with those of any other, since their validity is in general relative to different “frameworks”.

I like Asher Peres’s conclusion that unperformed tests have no outcomes: it is wrong to try to account for the outcomes of all the tests you might have performed but didn’t. This too is more radical than it appears, since recent versions of Bell’s theorem (inspired by Danny Greenberger, Mike Horne, and Anton Zeilinger) show that the outcome of the test you actually performed is incompatible with each and every possible set of outcomes for all the tests you might have performed but didn’t. This adds a word to Asher’s famous title: “Unperformed experiments have no conceivable results”.

That addition makes his point just a little harder to swallow. But swallowing becomes easier again if I expand Asher’s title further to “Many different sets of unperformed experiments have no conceivable sets of results, *if* the result for each local test has to be exactly the same in every set of results in which that particular local test appears”. (The expanded title itself, however, is harder to swallow.) What can it mean to impose such consistency on sets of conceivable data associated with different choices of sets of local tests, when only one set of tests was actually performed?

So for me, nonlocality is too unsubtle a conclusion to draw from the violation of Bell inequalities. My preference is for conclusions that focus on the impropriety of seeking explanations for what might have happened but didn’t. Evolution has hard-wired us to demand such explanations, since it was crucial for our ancestors to anticipate the consequences of all possible contingencies in their (classical) struggles. (See also the second of my answers to Question 2.)

## 2.9 What contributions to the foundations of quantum mechanics have or may come from quantum-information theory? What notion of 'information' could serve as a rigorous basis for progress in foundations?

I agree with Heisenberg and Peierls that the quantum formalism is a tool we have discovered to express the information we have acquired and the consequences of that information for the content of our subsequent acquisition of information. To the extent that it sharpens and systematizes this point of view, I believe that quantum information theory is the most promising and fruitful foundational approach.<sup>22</sup>

Beyond this, applying the quantum formalism directly to the processing of information itself may get us closer to the heart of what quantum mechanics is all about, than can the informationally less subtle problems addressed in more traditional physical applications of quantum mechanics. At the very least, it provides a refreshingly different set of examples of quantum phenomena.

I am not expert enough in quantum (or classical) information theory to have an opinion on the definition of information most likely to shed light on foundational questions. Slogans like “It from bit” are fun, but don’t tell me much without considerable (yet to be provided) expansion. It seems to me that any foundationally illuminating concept of information must be explicit about both the possessors of the information and the content of that information.<sup>23</sup> As John Bell put it, “Whose information?” and “Information about what?” (See also my answer to Question 16.)

---

<sup>22</sup>Replace “information” by “belief” and this is QBism.

<sup>23</sup>QBism is explicit: The possessor of the belief is the agent using quantum mechanics to assign probabilities; the content of the belief comes from the experience of the agent.

## 2.10 How can the foundations of quantum mechanics benefit from approaches that reconstruct quantum mechanics from fundamental principles? Can reconstruction reduce the need for interpretation?

It is wonderful that all of special relativity follows from the principle that no physical behavior can distinguish among frames of reference in different states of uniform motion, combined with the realization that the simultaneity of events in different places is a convention that can differ from one frame of reference to another. Can the rest of physics - in particular quantum mechanics - be reduced to so economical a set of assumptions?

I doubt it. Even the foundations of special relativity are not captured as compactly as I just claimed. I failed, for example, to mention the assumptions of spatial and temporal homogeneity, and of spatial isotropy. And the fundamental notion of an “event” - a phenomenon whose spatial and temporal extent we can ignore for purposes of the topic currently under discussion - might strike some as irritatingly vague, bringing “us” into the story in a way physics traditionally (and, I increasingly believe, wrongly <sup>24</sup>) tries to avoid. And just what are these human artifacts called “clocks” that play so fundamental a role in the story? In short, it’s not as simple as advertised.

Yet quantum mechanics does seem to be floating in the air, in a way that makes relativity seem quite anchored. At least the basic conceptual ingredients of relativity have at first glance a direct intuitive correspondence with familiar phenomena in our immediate experience. The complicating issues for relativity emerge only when one insists on sharpening up these intuitions. In contrast, the basic ingredients of quantum mechanics - states, superpositions, and their linear evolution in time - bear not even a vague relation to anything in our direct experience, while measurement - the only thing that ties the subject to the ground - seems to introduce what John Bell derided as “piddling laboratory operations” at too fundamental a level.

I’m glad people are attempting to reconstruct quantum mechanics from (a few) fundamental principles, but I’m skeptical that they’ll succeed without slipping into at least one of their principles something just as much in need

---

<sup>24</sup>Definitely a QBist sentiment.

of interpretation. The reason I'm nevertheless glad is that having a new and strikingly different formulation of the really puzzling stuff can sometimes be a useful step toward untangling the puzzle.

### **2.11 If you could choose one experiment, regardless of its current technical feasibility, to help answer a foundational question, which one would it be?**

The foundational issues about quantum mechanics that perplex me are all predicated on the assumption that the theory is correct. I would like to be able to make better sense of what it says. I am not persuaded that my perplexity is so acute that I should seek the answer in a breakdown of the theory. Therefore I would not expect any experimental test to shed light on a foundational question.

I exclude here the possibility that a crucial foundational issue might be associated with an application of the theory so intricate that the relevant calculation might be too difficult to perform, thereby requiring an experimental test. It does seem to me that all the puzzling features of the theory emerge full-blown in its most computationally elementary applications.

This is not to say that the breakdowns of quantum mechanics suggested by some interpretations are not worth exploring through experiment. (See also my answer to Question 7.) I expect quantum mechanics to break down at some scale. Indeed, I find it amazing, in view of the body of data that gave rise to it, that it seems to be working perfectly well within the atomic nucleus and even within the nucleon. This lends support to viewing quantum mechanics as a “mode of thought”, as Chris Fuchs and Rüdiger Schack once put it, rather than as a description of the world.<sup>25</sup>

So I would be surprised (and rather disappointed) if foundational issues were settled by observing a breakdown of quantum mechanics. I would expect them to be settled by our acquiring a deeper understanding of the existing theory, within its domain of validity.

---

<sup>25</sup>A tip of the hat to the two leading QBists.

## 2.12 If you have a preferred interpretation of quantum mechanics, what would it take to make you switch sides?

My intuitions about the nature of quantum mechanics are not coherent enough to add up to anything I would dignify with the term “interpretation”. Admittedly, shortly after turning sixty, I did write a few papers setting out what I called the Ithaca Interpretation (see also my answer to Question 6). But I was young then, innocent, and overly willing to sacrifice an accurate phrase for an entertaining one.

One of those papers made its argument under the banner of Bohr’s statement that the purpose of our description of nature is “only to track down, so far as it is possible, relations between the manifold aspects of our experience”. When I wrote the paper, the crucial word for me was *relations*. My motto was *correlations without correlata*. What led me to stop giving physics colloquia on the IIQM after only a year was the obvious question: “Correlations between what?” Abner Shimony aptly complained that the Ithaca Interpretation “had no foreign policy”. Exchanges with Chris Fuchs persuaded me that just as important as *relations* was *our experience*, which I was too ready to hide beneath the same rug under which I had (correctly) swept the problem of consciousness.

So insofar as I had a preferred interpretation in 1998, what persuaded me that it was, at best, insufficiently developed was somebody making me aware of some interesting ideas that hadn’t occurred to me. It remains entirely possible that some wise, imaginative, and readable person may in the future lure me away from the position I am trying to sketch in my answers to these questions.<sup>26</sup>

To make me switch to some interpretations I now reject would require a breakdown of quantum mechanics along lines suggested by these currently unpalatable points of view. Of course, at that point they would no longer be interpretations of existing theory, but alternative theories. To convert me to Bohmian mechanics, for example, I would have to see clear evidence of particles that were not in “quantum equilibrium”. Without that breakdown

---

<sup>26</sup>That position is so close to QBism that I’m not sure that in Stellenbosch my QBist friends lured me very far.

of orthodox quantum mechanics, the reintroduction of particle trajectories seems an unnecessary complication that raises questions at least as vexing as those raised by the orthodox theory. To convert me to the view that “wave-function collapse” was a real physical process and not just an updating of expectations on the basis of new information, I would have to see convincing evidence of deviations from quantum probabilities produced by Ghirardi-Rimini-Weber-Pearle “hits”.

A simple nontrivial example of a history containing many different times that exactly satisfied the consistency conditions might persuade me to take another look at consistent histories (see my answer to Question 16).

### **2.13 How do personal beliefs and values influence one’s choice of interpretation?**

The belief that physics is, or ought to be, the whole story surely plays a role. Those who believe that physics describes the external world *as it relates to us* have an interpretive flexibility unavailable to those who insist that “we” have no place in the story except as complex physical systems.<sup>27</sup> (See also the third of my answers to Question 2.)

I have the impression that those physicists who believe in God tend, perhaps unsurprisingly, to take a more strongly realistic view of the abstractions that make up the quantum formalism than do many of us who take an atheistic view of the world.

There are also those who maintain that while God does not exist, Physical Law does. Since I agree with the first half of this proposition, I would not call them idolatrous. But others might.

Values (as opposed to beliefs) are harder to identify. I sometimes detect them in the attitudes of those who believe in, or search for, hidden-variables models of quantum mechanics. I have heard ringing declarations about the nature of science, exhortations not to give up the good fight, and expressions of scorn for contemporary obscurity. (See also my answer to Question 15.)

---

<sup>27</sup>QBists!

## 2.14 What is the role of philosophy in advancing our understanding of the foundations of quantum mechanics?

If quantum mechanics is correct, or even if it is only correct to a high degree of accuracy in some yet-to-be-delimited domain, then everything in quantum foundations counts as philosophy. Let me rephrase the question: what role have professional philosophers played in advancing our understanding of the foundations of quantum mechanics? I do not count as “philosophers” professional philosophers who are also professional physicists, and I count as “professional” anybody with a Ph.D. in the field.

When I got into this business thirty years ago, I had hoped that philosophers would bring to the conversation their historical expertise in the Big Questions. What is the nature of human knowledge? How do people construct a model of the world external to themselves? How does our mental organization limit our ability to picture phenomena? How does our need to communicate with each other constrain the kinds of science we can develop? Those kinds of questions.

To my disappointment, it seems to me that professional philosophers prefer to behave as amateur physicists. They don’t try to view the formalism as part of a Bigger Picture. On the contrary, they seem to prefer to interpret it more literally and less imaginatively than many professional physicists. Because they are less proficient than physicists in using the tools of physics, they tend not to do as good a job on these narrower matters. They often come through as naive and unsophisticated.

So I would say that up to now, professional philosophers have not played a significant role in advancing our understanding of quantum foundations. I would not (and could not) discourage them from working in quantum foundations. But I would urge them to keep their eyes on the Big Questions. (See also my answer to Question 15.)

## 2.15 Q15. What new input and perspectives for the foundations of quantum mechanics may come from the interplay between quantum theory and gravity/relativity, and from the search for a unified theory?

My guess is that an understanding of the connection between gravity and quantum mechanics will have to await new input and perspectives from the foundations of both disciplines. Space and time in quantum field theory are classical parameters. They're on *our* side of the subject-object boundary.<sup>28</sup> Extrapolating them down to sub-nucleon levels - let alone to the Planck scale - strikes me as unwarranted and even arrogant. (I note with interest a hint of some personal values here. Compare my answer to Question 13.) Spatial and temporal coordinates describe the readings of our instruments.

I'm just as skeptical about quantum cosmologists applying quantum mechanics to the universe as a whole. For quantum mechanics to make sense, there has to be an inside ("the system") and an outside ("us").<sup>29</sup>

So insofar as gravity is a theory of the structure of space-time, I'd be surprised if real progress were made in incorporating it into quantum theory without a more thoughtful and (dare I say it?) *philosophical* examination of the foundations of both fields.

## 2.16 Where would you put your money when it comes to predicting the next major development in the foundations of quantum mechanics?

Let me put the question in a more manageable form: what was the last major development in the foundations of quantum mechanics? (It remains basically the same question, since none of the developments that follow have been broadly accepted as the most illuminating way to look at the subject.)

---

<sup>28</sup>The distinction between subject (agent) and object (world external to the agent) is a fundamental part of QBism. Schrödinger also makes much of it, particularly (but not exclusively) in *Nature and the Greeks*. See also my *Commentary* in the July 2012 *Physics Today* and my remarks in the December 2012 issue about the letters it elicited.

<sup>29</sup>More QBism.

I would nominate for the most important recent development the application of quantum mechanics to the processing of information, starting with the invention of quantum cryptography by Bennett and Brassard in 1984, continuing with the development of quantum computation, and the fascinating efforts of Chris Fuchs to make a coherent whole out of it all.<sup>30</sup> As runner-up, I would cite the study of pre- and postselected ensembles by Aharonov and his collaborators, and (perhaps - I still lack a good feeling for it) the ensuing notion of weak measurement. In third place, I would put the consistent-histories point of view, as put forth by Bob Griffiths.

What all three of these developments have in common is that they are standard quantum mechanics applied in highly nonstandard settings. In this respect, they are all conservative approaches to quantum foundations. They use the orthodox theory to answer simple questions that it had never before occurred to anybody to ask. The answers provide intriguing new perspectives on the theory.

Because the last of the three seems to have been widely ignored in the quantum-foundations community and is unrepresented among the authors of this volume, I'll say a little about it. (My old friend Pierre Hohenberg has tried valiantly to get me to take this stuff seriously.<sup>31</sup> Pierre and I were in both college and graduate school together, but in all those years nobody ever warned me to stay away from him; see my answer to Question 1. Maybe somebody should have.)

Consistent historians offer an unusual fusion of collapse and no-collapse points of view. Underlying their *weltanschauung* is an old formula of Aharonov, Bergmann, and Lebowitz (ABL), which compactly gives the probabilities of the outcomes of a whole sequence of (von Neumann) measurements carried out at different times on a system in a given initial state.<sup>32</sup> Prior to its reinterpretation<sup>33</sup> by consistent historians, the ABL formula was understood to be an expression of the fact that immediately after any particular

---

<sup>30</sup>QBism is at the top of the list.

<sup>31</sup>He disapproves of my recent interest in QBism.

<sup>32</sup>Bob Griffiths tells me that the formula was published by Wigner a year before ABL.

<sup>33</sup>Griffiths also made me realize that I should have said "drastic reinterpretation". I believe that I did in an earlier draft, since I was surprised to discover, when he complained, that "drastic" was not in the book.

measurement, the state of the system collapses according to the standard Born rule; this postcollapse state then evolves under the unitary dynamics until the next measurement in the sequence produces another collapse. Unitary evolution, followed by measurement and collapse, followed by more unitary evolution, followed by more measurement and collapse, and so on.

Consistent historians eliminate measurement and collapse from the story by reinterpreting these probabilities to be probabilities of what I would call *actual states of being* - called histories. These histories (or, more accurately, the subset of them deemed “consistent”, as noted below) have nothing to do with measurement outcomes. For consistent historians the ABL formula is thus more fundamental and broader in scope than the Born rule. The Born rule can be extracted from the consistent historians’ version of ABL in some very special cases, but measurement vanishes from ABL in the general case, which according to consistent historians gives probabilities not of measurement outcomes but of actual states of being.

How can they get away with this vast extension of actuality to entities whose nonexistence lies at the very heart of conventional quantum mechanics? Easily! They do it by forbidding the extension whenever it gets you into trouble; they impose stringent consistency conditions on the probabilities appearing in any candidate for a valid history. Any history that meets these consistency conditions can describe the probabilities of an actual state of being, and not the mere outcomes of a set of piddling laboratory operations. Any history that violates the consistency conditions is utter nonsense - not a history at all, and certainly not a description of actual states of being.

As one might expect, there can be many distinct histories, all of which meet their own internal consistency conditions, although the state of being that combines the actual states of being associated with more than one of those histories need not satisfy its own internal consistency conditions. When this happens, the combination of the two actual states of being is not an actual state of being.

Rather than concluding from this that the project is dead in the water, the consistent historians elevate it to a fundamental ontological principle. Reality is multifaceted. There can be this reality or there can be that reality, and provided you refrain from combining actualities from mutually incon-

sistent realities, all of the incompatible realities have an equally valid claim to actuality. This tangle of mutually incompatible candidates for actuality (associated with different “frameworks”) constitutes the no-collapse side of consistent histories. The collapse side lies in the fact that each of these peacefully coexisting mutually exclusive actualities is associated with what from the orthodox point of view (which consistent historians reject) would be a sequence of measurements and Born-rule collapses.

This multiplicity of incompatible realities reminds me of special relativity, where there is time in this frame of reference and time in that frame of reference, and provided only that you do not combine temporal statements valid in two different frames of reference, one set of temporal statements is as valid a description of reality as the other.

But I am disconcerted by the reluctance of some consistent historians to acknowledge the utterly radical nature of what they are proposing. The relativity of time was a pretty big pill to swallow, but the relativity of reality itself is to the relativity of time as an elephant is to a gnat. (Murray Gell Mann, in his talk of “demon worlds”, comes close to acknowledging this, yet he dismisses much less extravagant examples of quantum mysteries as so much “flapdoodle”).

## **2.17 What single question about the foundations of quantum mechanics would you put to an omniscient being?**

I’d ask, “How has the uncertainty principle altered the ‘omni’ of your omniscience?”

Joking aside - but it wasn’t really a joke - I have trouble imagining an omniscient being. Let me rephrase the question: if you could be frozen for 150 years and revived intact, what question would you ask physicists when you woke up?

I’d ask something like this:

Is the fundamental physics of a system still described in terms of quantum states that evolve linearly in time and that specify probabilities of the out-

comes of tests that we can perform on that system? If so, is anybody puzzled by the meaning of this conceptual structure? If not, is there general agreement on the meaning of the structure that replaced it?

In early twenty-first-century terms: has the structure of quantum mechanics survived intact for a century and a half? If so, are there still foundational problems? If not, are there still foundational problems?

I chose 150 years because a century might not be long enough to get an interesting answer. But I also worry that physicists two centuries from now, no matter how I phrased the question, might not understand it. It might elicit only polite bewilderment, just as a pressing aether-theoretic query at the end of the nineteenth century might seem not only irrelevant but downright incomprehensible to a physicist of the early twenty-first.

There are two possible grounds for future bewilderment at my question. One is that quantum mechanics will have been discovered, as Einstein always hoped, to be a phenomenology based on a more fundamental view of the world, which is more detailed and more intuitively accessible. This strikes me as unlikely, because John Bell showed that any theory detailed enough to satisfy certain common-sense yearnings would also have to contain instantaneous action-at-a-distance. (See my answer to Question 8.) So while the discovery of a more fundamental view of the world during the next century and a half seems entirely possible, I'd be surprised if the new theory turned out to be more intuitive than our current understanding.

An appropriate timescale for the survival of quantum mechanics is set by the fact that its basic conceptual machinery has suffered no alterations whatever, beyond a little tidying-up, for over eighty years. Not a bad run when you compare what happened to fundamental knowledge between 1860 and 1940, though not close to the more than two centuries that classical mechanics remained the fundamental theory. So the persistence of the same basic formalism for another 150 years seems at least plausible.

Even so, my question might elicit mid-twenty-second-century bewilderment, because after several more generations of physicists, chemists, biologists, engineers, and computer scientists had worked with the theory, it might finally, in Feynman's words, have become obvious to everybody that there's no real

problem. We early twenty-first-century people, who believed there ought to be a better way to understand the theory, will then have been consigned to the same dustbin of history as the early twentieth-century aether theorists.

I hope that's not how it works out. It is, for example, now possible to articulate the nature of the wrong thinking that made relativity seem shockingly counterintuitive to many people during its early years. People had simply deluded themselves into believing that there was something called "time" that clocks recorded, rather than recognizing that "time" was a remarkably convenient abstraction - that enables us to talk efficiently and even-handedly about the correlation among many different kinds of clocks.

There is now no generally agreed-upon key to dissolving the puzzlement that quantum mechanics engenders today in many of us. (For that matter, I have encountered otherwise sensible physicists who disagree with the above resolution of the puzzles of relativity.) I would hope that within the next 150 years, such a key might be found that almost everybody would agree clarifies the character of the theory, in contrast to today's state of affairs, where no school of thought commands more than ten percent of the population, except for those who maintain - but can they really mean it? - that there is nothing to be puzzled about.