

QUESTION 2  
BIG ISSUES

---

*What are the most pressing problems in the foundations of quantum mechanics today?*

WHILE SCIENCE CHURNS OUT a relentless series of quantum leaps within a matter of years (if not months), philosophy is accustomed to a much more leisurely ride. As a philosopher friend of mine recently remarked, “Major advances in philosophy happen in units of centuries, and even that might be an optimistic assessment.” And indeed, by their very nature, many of the questions that perplexed Kant or even Plato continue to engage the contemporary philosopher. Clearly, the pace of progress is a matter of perspective.

The foundations of quantum mechanics occupy a comfortable middle ground between these two extremes. The field is relatively young and dynamic. And because its object of interest is a physical theory, the field is rooted quite firmly in science, despite the host of metaphysical questions quantum mechanics seems to generate. At the same time, the issues that the founders of the theory already agonized over have not visibly aged in the passing decades. Schrödinger’s cat is alive and well fed and not inclined to having its fate decided anytime soon. The ripples of EPR are still felt everywhere. Bohr’s interpretation of quantum mechanics keeps flexing its muscles, inspiring a new generation of epistemic and informational viewpoints while sending other people scrambling for an antidote.

But to say that the time-honored themes of quantum theory’s first generation are on everyone’s lips today as ever is not to suggest that the field of quantum foundations has turned stagnant, or that it has become akin to a dog chasing its tail, or that it has been reduced to little more than an autoerotic enterprise with no hope or desire for escape from bachelorhood. Quite the opposite, actually. As already mentioned in the prologue, there’s been a dramatic refinement over time in the way people think and talk about the central issues. Post-war developments—such as the stream of new interpretations, the various no-go theorems, experiments at the quantum level, and more recently quantum information—have not only put a distinctly new spin on old debates, but have also given rise to a flurry of new questions (and even a few precious answers).

In fact, it is now far from obvious what a contemporary foundationalist would regard as the key issues awaiting resolution. There are no hard-and-fast rules. What one person may experience as a genuine and pivotal difficulty—to be disregard only

at our peril—may be perceived by someone else as a petty concern or mere pseudo-issue. And even once you find two people settling on the same problem, you can bet that they'll hold divergent views of what the problem is really all about and what the best course of action might be.

To get a good sense, then, of a representative range of present-day foundational priorities, let's ask our interviewees to lay out the playing field for us.



GUIDO BACCIAGALUPPI · I think recent progress in various fields within foundations has brought up, or renewed, interest in a number of very important questions—although maybe none are so pressing as to impede further progress pending their resolution.

Hidden-variables programs, that is, pilot-wave theories of the de Broglie-Bohm type, have progressed enough in recent years that the question of direct experimental evidence that might decide between them and quantum mechanics has become meaningful. The central idea is the analogy between pilot-wave theories and classical statistical mechanics, in particular the possibility of observable nonequilibrium effects. The range of application of pilot-wave theories is now large enough that they can be applied to quite exotic phenomena that might reveal systematic violations of the Born rule. Antony Valentini in particular has been pioneering the exploration of these possibilities. Such violations would be the most direct evidence in favor of a revision of quantum mechanics.

Within collapse theories, recent work—especially by Pearle and by Nicosini and Rimini in physics, and by Wayne Myrvold in philosophy—has brought us very close to finally deciding whether a satisfactory relativistic collapse theory is possible. That is a very big question, and it is surprising that so few researchers actively engage in it. (Maybe this is a side effect of an apparent shift in the preoccupations of the community, partially away from more traditional approaches and more toward the new field of quantum information. Indeed, at the Sixteenth U.K. Foundations Meeting just a few months ago, it was quite noticeable that only a handful of talks were in the subject areas of hidden variables, collapse theories, and Everett interpretations.) The experimental question of deciding between collapse theories and quantum mechanics has also made progress, but it is not quite as promising as in the case of pilot-wave theories. This is due to the fact that the appearance of spontaneous collapse can be always mimicked by decoherence induced by some appropriate environment (coupled with one's favorite no-collapse interpretation). What is particularly worrisome is the suspicion that a rival no-collapse theory might not even need to invoke some hitherto unobserved, mysterious environment to do the job, but that once gravitation is quantized, it might provide just the right kind of environment to reproduce some of the currently best candidates for collapse theories (which tend to be mass-density based). A paper by Bernard Kay some twelve years ago or so made this point in a particularly striking manner.

Everett interpretations have also made quite spectacular progress in recent years, principally thanks to work by Simon Saunders in the 1990s, and by David Deutsch, David Wallace, and others in the 2000s. They appear, in fact, to have solved—or to have convincing strategies for solving—all the classic questions that used to trouble them. There are still a few question marks, but I would not say there are very pressing questions for Everett. (Personally, I think there are some questions about the details of relativistic locality and of the various accounts of mentality, which I am exploring with Laura Feline, and some lingering issues about probabilities, as raised, for instance, by Peter Lewis.)

The development of the cluster of approaches around quantum information has brought renewed interest in axiomatic foundations of standard quantum mechanics, and the reconstruction problem of quantum mechanics has seen a sudden flood of very impressive and diverse results from a number of researchers (among others, Hardy, Goyal, and Chiribella–D’Ariano–Perinotti—quoting just the ones I happen to be most familiar with). Among these developments, one particular instance that never ceases to amaze me is Rob Spekkens’s “toy theory,” which reproduces qualitative analogues of scores of quantum effects (excepting computational speedup, Bell-inequality violation, and Kochen–Specker theorems), based purely on a notion of an epistemic limitation on the description of system states. These and similar results carry with them insights into what the truly crucial difference might be between classical and quantum theories, and decisive progress along these lines would be a truly splendid thing.

Some of the other questions I would be most intrigued to see resolved are those surrounding the relation between standard quantum field theory and the axiomatic approach of algebraic quantum field theory, but I am not sure I am competent enough to comment in detail.

Finally, if I may mention a particular interest of mine, I believe that the relation between quantum mechanics and the direction of time needs to be explored further and may yet have surprises in store. Part of this interest, of course, stems from my period at Huw Price’s Centre for Time in Sydney, but part is rooted in my interest in decoherence, and is related to ideas I am exploring jointly with Max!

ČASLAV BRUKNER · Quantum theory makes the most accurate empirical predictions. Yet it lacks simple, comprehensible physical principles from which it could be uniquely derived. Without such principles, we can have no serious understanding of quantum theory and cannot hope to offer an honest answer—one that’s different from a mere “The world just happens to be that way”—to students’ penetrating questions of why there is indeterminism in quantum physics, or of where Schrödinger’s equation comes from. The standard textbook axioms for the quantum formalism are of a highly abstract nature, involving terms such as “rays in Hilbert space” and “self-adjoint operators.” And a vast majority of alternative approaches that attempt to find a set of physical principles behind quantum theory either fall short of uniquely deriving quantum theory from these principles, or are based on abstract mathematical assumptions that themselves call for a more conclusive physical motivation.

One strategy for progress on this front is to view quantum theory within the context of general theories that conform to reasonable axioms about probabilities, and then to contrast the alternatives. Surprisingly, in the last decade it was found that what one might have expected to be uniquely quantum features—such as probabilistic predictions for individual outcomes (indeterminism), the impossibility of copying unknown states (no cloning), or the violation of “local realism”—are actually highly generic for general probabilistic theories. So, is there any reason why we see phenomena obeying the laws of quantum theory rather than of any other possible probabilistic theory?

Most recently, there have been several approaches to reconstructing quantum theory on the basis of a small set of reasonable physical axioms that demarcate phenomena that are exclusively quantum from those that are common to more general probabilistic theories (see my answer to Question 3, page 66, for my own reconstruction attempt). Typically, however, the proposed axioms partially use abstract mathematical language. One should, in my opinion, insist on reducing this language as far as possible to a phenomenological meaning, and not be afraid to combine these simple elements of everybody’s experience with abstract concepts such as “information” or “knowledge.”

Modern reconstructions of quantum theory partially meet this demand by being entirely developed in terms of primitive laboratory operations, such as preparations, transformations, and measurements. Bohr’s insistence on the usage of classical terms is respected insofar as these operations are classically describable, but they are not linked to the concepts of time, position, momentum, or energy of “traditional” physics. As a result, one derives a finite-dimensional, or countably infinite-dimensional, Hilbert space as an operationally testable, abstract formalism concerned with predictions of future experiments and frequency counts, which are ultimately based on clicks of detectors and nothing more. While I consider the quantum state to be a tool for calculating the probabilities of whatever future measurements we may choose to carry out, I want to make the point that we do appoint physical labels to the states in any particular orthonormal basis, and that we do deal with notions of position, momentum, fields, specific forms of Hamiltonians, and so forth. The abstract quantum formalism, however, tells us nothing about how we should go about building a useful instrument for measuring, say, position, as opposed to any other observable.

In my opinion, the clue for this will not be obtained without an understanding of the concept of distance—or of the more abstract idea of nearness—of points lying in ordinary real space. In the abstract quantum formalism, any two different eigenvalues of the position observable correspond to orthogonal quantum states, without any concept of closeness or distance. The terms “close” and “distant” make sense only in a *classical* context, where those eigenvalues are treated as close when they correspond to neighboring outcomes in real space. Is it possible to arrive at notions of nearness, distance, and space—and, furthermore, at the theories referring to these notions, such as the theory of relativity, quantum field theory, and elementary-particle theory—merely on the basis of clicks in detectors? Or is it necessary to presuppose these

notions, prior to the construction of physical theories? To me, this is one of the most pressing contemporary questions in the foundations of quantum mechanics.

Preferred tensor factorizations, coarse-grained observables, and symmetries might help to indeed demonstrate that all known basic theories of physics are a consequence of abstract quantum theory. The most elementary system, or qubit, lives in an abstract state space with  $SU(2)$  symmetry, which is locally isomorphic to the group  $SO(3)$  of rotations in three-dimensional space. Thinking about directional degrees of freedom—i.e., about spin—this symmetry finds its operational justification in the symmetry of the configuration of macroscopic instruments by which the spin state is prepared and measured. But from where have the macroscopic instruments acquired this symmetry in the first place?

I would like to suggest that under the everyday conditions of coarse-grained measurements, the systems consisting of a large number of elementary systems, such as macroscopic instruments, acquire the symmetry of their elementary constituents. For example, in 2007 Johannes Kofler and I derived the following result. Suppose we mimic restricted measurement precision by bunching together eigenvalues of spin projections into slots. Then the spin coherence states—which are states of many identical elementary spins—acquire an effective description as a classical spin embedded in ordinary three-dimensional space. The orientation of this classical spin requires two angles to be defined, which gives rise, through the relative angle, to the notion of “neighboring” orientations. Thus, the reason for three-dimensional real space being *the* space of the inferred world is offered through a circular but *consistent* movement in the reconstruction, in which it is legitimate to recover the elements with which one started the reconstruction. Von Weizsäcker coined the name *Kreisgang* (“circle walk”) for such movements. The epistemological framework of classical physics and three-dimensional ordinary space are required at the “beginning” of the *Kreisgang* to specify the configuration of macroscopic instruments by which the quantum state is prepared and measured. The *Kreisgang* is “closed” by showing that under the everyday conditions of coarse-grained measurements, a description of macroscopic instruments emerges in the terminology of classical physics, and three-dimensional ordinary space emerges from within quantum theory. I conclude by remarking that this program is not completed—and perhaps not completable.

JEFFREY BUB · We don’t really understand the notion of a quantum state, in particular an entangled quantum state, and the peculiar role of measurement in taking the description of events from the quantum level, where you have interference and entanglement, to an effectively classical level where you don’t. In a 1935 article responding to the EPR argument, Schrödinger characterized entanglement as “*the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought.” I would say that understanding the nonlocality associated with entangled quantum states, and understanding measurement, in a deep sense, are still the most pressing problems in the foundations of quantum mechanics today.

Having said that, I don’t think we are going to get anywhere by sitting back and reflecting on the meaning of measurement or the notion of state in physics, or in try-

ing to “solve the measurement problem.” It’s not that we don’t know how to solve the measurement problem: Bohm’s theory is a solution, so-called modal interpretations provide formal solutions, the Everett interpretation is another solution, the Ghirardi–Rimini–Weber theory is a rival theory that avoids the measurement problem. It’s rather that there’s nothing like a general consensus that any of these proposals are getting it right. Einstein commented in a letter to Max Born that Bohm’s theory “seems too cheap to me.” He was referring to the deterministic character of Bohm’s theory. My feeling is that all these ways of thinking about quantum mechanics are “too cheap,” because they all attempt to explain away the irreducible indeterminism of quantum mechanics—rather than providing a conceptual framework for thinking about a universe in which, to put it somewhat anthropomorphically, a particle is free to choose its own response to a measurement, subject only to probabilistic constraints, which might be nonlocal.

I think the way forward is to consider the sort of question raised by Wheeler: why the quantum? Or, the more focused question posed by Popescu and Rohrlich in their 1994 article, in which they introduced the notion of a nonlocal box: why is quantum theory not more nonlocal, given that you can have more nonlocality without thereby allowing the possibility of instantaneous signaling between the parties? This question has been extraordinarily fruitful in leading to new insights about quantum nonlocality and seems to me the most promising route to advancing our understanding of what is really involved in the transition from classical to quantum physics.

ARTHUR FINE · My general attitude toward science is pluralistic, in the sense that I regard every major theory in science as open to reasonable interpretations that differ from one another over some essentials. This is certainly true in the case of quantum theory, where interpretations differ over collapse and the need for an external observer, over determinism and indeterminism, over whether Lorentz invariance is merely phenomenological, over realism and instrumentalism, and so on. Faced with this array, one might experience a pressing need to sort things out so as to narrow the options, hopefully, to the one “correct” interpretation. I do not share that attitude. Rather, I see the interpretive array as part of a healthy freedom of choice whose payoff comes from the different heuristic paths suggested by the differing interpretations. So I don’t think that finding the “right” interpretation of quantum mechanics is a pressing problem at all.

Still, there are problems that we would all like to understand better. One is the whole question of locality. Reflections that stem from the Bell theorem have suggested that quantum phenomena exemplify nonlocality: acting here can immediately influence happenings way over there. I have never seen an argument for this conclusion that does not involve assumptions that go well beyond reliable theory and data. Indeed, several generations now of excellent experimental investigations have not yet produced a conclusive verdict concerning the violation of the Bell inequalities themselves. The problem remains as to whether one can satisfy efficiency requirements (both on detection and on synchronization of coincidence) and, in the same experiment, manage to rule out communication between the two (or more) wings where

the measurements are made. Although there are plans for experiments that claim to do this, none seem to work. It may be that none can work, since modern simulation techniques suggest that statistics in violation of the Bell inequalities can be generated classically in a wide range of circumstances, including the conditions proposed in most experimental designs. Thus, entanglement may turn out to be a significant resource in quantum information theory, but not of such significance foundationally as has been supposed.

One general issue raised by the debates over locality is to understand the connection between stochastic independence (probabilities multiply) and genuine physical independence (no mutual influence). It is the latter that is at issue in “locality,” but it is the former that goes proxy for it in the Bell-like calculations. We need to press harder and deeper in our analysis here.

CHRISTOPHER FUCHS · John Wheeler would ask, “Why the quantum?” To him, that was the single most pressing question in all of physics. You can guess that with the high regard I have for him, it would be the most pressing question for me as well. And it is. But it’s not a case of hero worship; it’s a case of it just being the right question. The quantum stands up and says, “I am different!” If you really want to get to the depths of physics, then that’s the place to look.

Where I see almost all the other interpretive efforts for quantum theory at an impasse is that despite all the posturing and grimacing over the “measurement problem” and the “mysteries of nonlocality” and what have you, none of them ask in any serious way, “Why do we have this theory in the first place?” They see the task as one of patching a leaking boat, not one of seeking the principle that has kept the boat floating this long (for at least this well). My guess is that if we can understand what has kept the theory afloat, we’ll understand that it was never leaky to begin with. The only source of leaks was the strategy of trying to tack a preconception onto the theory that shouldn’t have been there.

What is this preconception? It almost feels like cheating to say anything about it before Question 4 ... but I have to, or I can’t answer the rest of Question 2! The preconception is that a quantum state is a *real thing*—that there were quantum states before there were observers; that quantum states will remain even if all observation is snuffed out by nuclear holocaust. It is that if quantum states are the currency of quantum theory, the world had better have some in the bank. Take the Everett interpretation(s)—the world as a whole has its wave function, darned be it if observership or probability is never actually reconstructed within the theory. The Bohmian interpretation(s)? The wave function is the particle’s guiding field; observers never mentioned at all. GRW interpretation(s)? Collapse is what happens when wave functions get too big; of course they’re real. Zurek’s “let quantum be quantum”? It is, as far as I can tell, a view that starts and ends with the wave function. There is no possibility that two observers might have two distinct (contradicting) wave functions for a system, for the observers are already *in* a big, giant wave function themselves.

So when I say “Why the quantum?” is the most pressing question, I mean this specifically within an interpretive background in which quantum states aren’t real



in the first place. I mean it within a background where quantum states represent observers' *personal* information, expectations, degrees of belief.

"But that's *just* instrumentalism," the philosopher of science says snidely (see my answer to Question 14, page 253). "You give up the game before you start." Believe me, you've got to stand your ground with these guys when their label guns fly from their holsters! I say this because if one asks "Why the quantum?" in this context, it can only mean that one is being *realist* about the *reasons* for one's instrumentalities. In other words, even if quantum theory is purely a theory for apportioning and structuring degrees of belief, the question of "Why the quantum?" is nonetheless a question of what it is about the actual, real, objective character of the world that compels us to use this framework for reasoning rather than another. We observers are floating in the world, making decisions on all that we experience around us: why are we well-advised to use the formalism of quantum theory for that purpose and not some other formalism? Surely it connotes something about the general character of the world—something that is contingent, something that might have been otherwise, something that goes deeper than our decision-making itself.

With this one gets at the real flavor of this *most pressing problem in the foundations of quantum mechanics* from the point of view of QBism. It takes on two stages. The first is to find a crisp, convincing way to pose quantum theory in such a way that it gets rid of these trouble-making quantum states in the first place. What I mean by this is, if quantum theory is actually about how to structure one's degrees of belief, it should become conceptually the clearest when written in its own native terms. To give an example of how this might go, consider the Born probability rule as it is usually represented: one starts with a quantum state  $\hat{\rho}$ , say for some  $d$ -level system, and some orthogonal set of projection operators  $\hat{D}_j$  representing the outcomes of some nondegenerate observable. The rule is that the classical value  $D_j$  registered by the measuring device (no hat this time) will occur with probability

$$p(D_j) = \text{tr}(\hat{\rho}\hat{D}_j).$$

A recent result of QBism, however, is that if a certain mathematical structure always exists in Hilbert space (we know it does for  $d = 2$  to 67 already), then in place of the operator  $\hat{\rho}$  one can always identify a *single* probability distribution  $p(H_i)$ , and in place of the operators  $\hat{D}_j$  one can always identify a set of conditional probability distributions  $p(D_j | H_i)$ , such that

$$p(D_j) = (d+1) \sum_i p(H_i) p(D_j | H_i) - 1.$$

The similarity between this formula and the usual Bayesian sum rule (law of total probability) is uncanny. It says that the Born rule is about degrees of belief going in, and degrees of belief coming out. The use of quantum states in the usual way of stating the rule (that is, rather than degrees of belief directly) would then simply be a relic of an initial bad choice in formalism.



If this program of rewriting quantum theory becomes fully successful (working for all  $d$ , for instance), thereafter there should be no room for the distracting debates on the substantiality of quantum states—they're not even in the theory now—nor the tired discussions of nonlocality and the “measurement problem” the faulty preconception inevitably engendered. At this point, a second stage of the pressing question would kick in: it will be time to take a hard look at the new equations expressing quantum theory and ask how it is that *they* are mounted onto the world. What about the world compels this kind of structuring for our beliefs? To get at that is to really get at “Why the quantum?” And my guess is, when the answer is in hand, physics will be ready to explore worlds the faulty preconception of quantum states couldn't dream of.

GIANCARLO GHIRARDI · I believe that the most pressing problems are still those that have been debated for more than eighty years by some of the brightest scientists and deepest thinkers of the past century: Niels Bohr, Werner Heisenberg, John von Neumann, Albert Einstein, Erwin Schrödinger, John Bell. To characterize these problems in a nutshell, I cannot do better than stressing the totally unsatisfactory conceptual status of our best theory by reporting the famous sentence by Bell: “Nobody knows what quantum mechanics says exactly about any situation, for nobody knows where the boundary really is between wavy quantum systems and the world of particular events.”

I also share Bell's opinion that the fact that this wonderful and extremely successful theory is radically incapable of accounting for our definite perceptions does not matter in practice, at least not presently. But I cannot accept that the basic theoretical construction for our understanding of natural phenomena is internally inconsistent, and that it is not able to account for the way it postulates measuring processes to take place. I will repeatedly come back to this point in my subsequent comments. But from the very beginning, I want to emphasize with great strength that science, this wonderful and unbelievable creation of the human mind, finds its real reason of existence in its ability to allow for an objective and always-growing understanding of reality. As such, an internally inconsistent theoretical scheme—one that becomes acceptable only by resorting to vague, not well-defined, imprecise, and fundamentally contradictory verbal assertions—cannot be taken as real progress in our grasping *God's thoughts*.

In this spirit, and given that theoretical schemes exist that are logically consistent and predictively equivalent—or even identical—to standard quantum mechanics (here I have in mind particularly the spontaneous-collapse theories and Bohmian mechanics), I am naturally led to share another position of Bell's, which he expressed with great clarity in *Against Measurement* and in his Tauschek Lectures. Namely, the great problem now is which one of the existing “exact” theories admits a fully satisfactory relativistic generalization. Here it is useful to recall that Bell used the term “exact” to denote a theory that “neither needs nor is embarrassed by an observer.”

SHELLY GOLDSTEIN · If I were to take this question to be concerned only with the most pressing problems in the foundations of quantum mechanics *today*, then I suppose I would point to the tension between quantum nonlocality and relativity. Relativity is widely regarded both as a fundamental physical principle and as being incompatible with any sort of genuine action-at-a-distance. Quantum nonlocality is arguably (correctly, I believe) an experimentally verified consequence of quantum mechanics that would clearly seem to involve genuine action-at-a-distance. Does relativity then have to be abandoned, or can it be reconciled with quantum nonlocality, appearances to the contrary notwithstanding?

I think it would be better, however, to respond to the following question: what *have been* the most pressing problems in the foundations of quantum mechanics? And to this I suppose the standard answer is the measurement problem, or, more or less equivalently, Schrödinger's cat paradox.

The problem here is that the usual description of the state of a system in a quantum-mechanical universe is of a rather unusual sort. It is given by a rather abstract mathematical object, called the wave function or the quantum state vector (or maybe the density matrix) of the system, an object whose physical meaning is rather obscure in traditional presentations of quantum theory. Moreover, in these presentations we are usually rather emphatically discouraged from supplementing our description of a quantum system with further—possibly more familiar but maybe exotic and elusive—variables, or even from contemplating such a possibility.

If one accepts, however, that the usual quantum-mechanical description of the state of a quantum system is indeed the complete description of that system, it seems hard to avoid the conclusion that quantum measurements typically fail to have results: pointers on measurement devices typically fail to point, computer printouts typically fail to have anything definite written on them, and so on. More generally, macroscopic states of affairs tend to be grotesquely indefinite, with cats seemingly both dead and alive at the same time, and the like. This is not good!

These difficulties can be avoided by invoking the measurement axioms of quantum theory, in particular the collapse postulate. According to this postulate, the usual quantum-mechanical dynamics of the state vector of a system (given by Schrödinger's equation)—the fundamental dynamical equation of quantum theory—is abrogated whenever measurements are performed. The deterministic Schrödinger evolution of the state vector is then replaced by a random collapse to a state vector that can be regarded as corresponding to a definite macroscopic state of affairs: to a pointer pointing in a definite direction, to a cat that is definitely dead or definitely alive, and so on.

But doing so comes at a price: one then has to accept that quantum theory involves special rules for what happens during measurement, rules that are in addition to, and not derivable from, the quantum rules governing all other situations. One has to accept that the notions of measurement and observation play a fundamental role in the very formulation of quantum theory, in sharp conflict with the much more plausible view that what happens during measurement and observation in a quantum universe, like everything else that happens in such a universe, is a consequence of the laws governing the behavior of the constituents of that universe—say the elementary

particles and fields. These laws apply directly to the microscopic level of description, and they say nothing directly about measurement and observation, notions that arise and make sense on an entirely different level of description, the macroscopic level.

I believe, however, that the measurement problem, as important as it is, is nonetheless but a symptom of a more basic difficulty with standard quantum mechanics: it is not at all clear what quantum theory is about. Indeed, it is not at all clear what quantum theory actually says. Is quantum mechanics fundamentally about measurement and observation? Is it about the behavior of macroscopic variables? Or is it about our mental states? Is it about the behavior of wave functions? Or is it about the behavior of suitable fundamental microscopic entities, elementary particles and/or fields? Quantum mechanics provides us with formulas for lots of probabilities. What are these probabilities of? Of results of measurements? Or are they the probabilities for certain unknown details about the state of a system, details that exist and are meaningful prior to measurement?

It is often said that such questions are the concern of the foundations of quantum mechanics, or of the interpretation of quantum mechanics—but not, somehow, of quantum mechanics itself, of quantum mechanics simpliciter. I think this is wrong. I think these, and similar, questions are a reflection of the fact that quantum mechanics, in the words of John Bell, is “unprofessionally vague and ambiguous.”

What is usually regarded as a fundamental problem in the *foundations* of quantum mechanics, a problem often described as that of *interpreting* quantum mechanics, is, I believe, better described as the problem of finding a sufficiently precise *formulation* of quantum mechanics: a *version* of quantum mechanics that, while expressed in precise mathematical terms, is also clear as physics.

And it is hard for me to imagine how this can be achieved, in any fundamental physical theory, unless that theory involves, as part of its description of the state of a system, an explicit space-time ontology (for a relativistic version, and a spatial ontology whose state changes with time for the nonrelativistic version). This ontology might be a particle ontology, involving world lines in space-time, or a field ontology, involving a field on space-time, or perhaps both, or perhaps neither but something else. In any case, the space-time ontology amounts to a certain kind of decoration of space-time, to the specification of what Bell has called the local beables of the theory.

Theories involving different local beables, or involving the same local beables but different laws for the local beables, would be different theories—for example, different *versions* rather than merely different *interpretations* of quantum theory.

DANIEL GREENBERGER · For reasons I'll explain in my answer to Question 7 (see page 152), I don't think the measurement problem will be solvable soon, or possibly ever. We will probably have to know more about nature for that. But there are other questions that are intriguing, such as whether a single particle has a wave function, or whether we have to talk about ensembles, and whether the wave function represents solidly observable probabilities, or just subjective information that we have about the system.

I myself have been worrying along different lines. I don't think we treat mass properly in quantum theory. It enters as a parameter, while energy enters as an operator. If  $E = mc^2$ , then I don't think that's consistent, and there is much evidence for that. In the same vein, the concept of proper time is much more subtle in quantum theory than it is in classical physics. For example, if you send a particle wave packet through a beam splitter, each part has its own proper time. If the two parts then get accelerated differently, their proper times run at different rates. If now the two parts get recombined, say at another beam splitter, what exactly is the proper time of the recombined particle? This is a practical question because the particle can be unstable, and its decay time will be controlled by the proper time that has elapsed. Surely the two parts cannot remember their separate histories. That would violate the essence of how quantum theory works.

Connected to this problem is the serious disconnect between quantum theory and general relativity. Quantum theory works with position and momentum, which intrinsically brings in the mass of the particle, while relativity works with particle trajectories, position and velocity, purely geometrical concepts, and independent of the mass. As a consequence, the weak equivalence principle breaks down in quantum mechanics. I think that these problems are the essence of why we don't have a theory of quantum gravity. It goes way beyond the mathematical complications of a non-linear theory. I think we don't understand gravity at the simple physical level of the equivalence principle. We don't know nearly enough to even begin to make a theory of quantum gravity. (If someone succeeded in making such a theory mathematically, which certainly could happen, I think it would be a serious step backward—everyone would believe it, and it would probably win a Nobel prize. Nobody could test it, and in my opinion, it would be almost guaranteed to be wrong, since it would be based on ideas that do not fit together on the simplest level.) I'll have more to say about this in my answer to Question 15 (see page 265).

LUCIEN HARDY · The most well-known problem in quantum foundations is the measurement problem—our basic conception of reality depends on how we resolve this. I will address this problem in my answer to Question 7 (see page 153). The measurement problem is tremendously important. But there is another problem that is even more important—and that may well lead to the solution of the measurement problem. This is to find a theory of quantum gravity. The problem of quantum gravity is easy to state: find a theory that reduces to quantum theory and to general relativity in appropriate limits. It is not so easy to solve. The two main approaches are string theory and loop quantum gravity. Both are deeply conservative, in the sense that they assume it will be possible to formulate a theory of quantum gravity within the quantum formalism as it stands. I do not believe this is the right approach. Quantum theory and general relativity are each deeply conservative, and deeply radical, but in complementary respects. Quantum theory is conservative in that it works on a fixed space-time background, but it is radical in that probabilities play an indispensable role. General relativity is conservative in that it is deterministic (probabilities are not necessary), but it is radical in that the space-time background is not fixed but rather

depends on the distribution of matter. In my opinion, a theory of quantum gravity will have to take the radical road in each case. It will be probabilistic, and it will have nonfixed causal structure. In fact, we can expect it to be a bit more radical still. It will, most likely, have indefinite causal structure. The reason for this is that in quantum theory, when we have a physical quantity that can vary, we will typically have situations where there is fundamental indefiniteness as to the value of the quantity. Since causal structure is dynamical in general relativity, we therefore expect it to be subject to fundamental indefiniteness in quantum gravity. This means that it will sometimes be the case that there is no matter of fact as to whether a given interval is spacelike or timelike. The basic mathematical apparatus of quantum theory needs a fixed space-time background (at least it requires a background time with respect to which the state evolves), and the basic mathematical apparatus of general relativity is deterministic. Neither framework is likely to be capable of accommodating a theory of quantum gravity, since neither possesses the radical feature of the other, and neither has indefinite causal structure. Hence, we require a deeper framework with new conceptual and mathematical apparatuses.

It is instructive to look at the transition from Newton's theory of gravitation to Einstein's theory of general relativity. We can take a limit to get from Einstein's theory back to Newton's theory. The mathematical apparatus of general relativity, however, is very different from that of Newton's theory. Newtonian gravity suffers from a deep conceptual problem: the force of gravity is not local. In general relativity, locality is restored, because the gravitational force is propagated locally through the space-time continuum (through matter-induced curvature of this very continuum). Even though Newton's theory turned out not to be fundamental, it is interesting to ask what the best interpretation of it is. One reasonable answer is that it should be regarded as a theory of curved space rather than of curved space-time. Such an interpretation of Newton's theory (as formalized by Cartan) only became evident after Einstein had formulated his theory of general relativity in terms of the curvature of space-time. This point, which is due to Wayne Myrvold, raises the possibility that we will best understand quantum theory—which suffers from its own deep conceptual problems—in retrospect as a limiting case of a deeper theory, such as a theory of quantum gravity. If this is true, then we need to work on quantum gravity to have a hope of properly solving the measurement problem.

The problem of quantum gravity requires, in my opinion, the development of a new mathematical framework. This could be as radical a departure from the frameworks of quantum theory (Hilbert spaces) and general relativity (tensor calculus) as the tensor calculus for general relativity is from the mathematics of Newtonian mechanics. The problem of quantum gravity is, I believe, a foundational problem, and the tools and methods of foundational thinking need to be brought to bear on it.

ANTHONY LEGGETT · To my mind, within the boundaries of “foundations of quantum mechanics” strictly defined, there is really only one overarching problem: is quantum mechanics the whole truth about the physical world? That is, will the textbook application of the formalism—including the use of the measurement axiom,

possibly at a very late stage—continue to describe experimental results adequately for the indefinite future? If the answer should turn out to be no, then, of course, there would be any number of further questions to be raised, but they would no longer be about quantum mechanics. If the answer is yes, then I believe there is really not much left to be asked (see also my answer to Question 3, page 79).

I think that there is, however, one question that—while in some sense more general than being about quantum mechanics as such—may be relevant to our future perceptions of the meaning of the formalism. This is the issue of the basis and status of the conventional viewpoint on the arrow of time. To be more specific, if it were to become accepted in a more general context that this arrow could, as it were, reverse itself locally and temporarily—as has in effect been suggested by a number of thinkers—then I believe this might recolor our thinking about the measurement problem and about other aspects of the formalism.

**TIM MAUDLIN** · The most pressing problem today is the same as ever it was: to clearly articulate the exact physical content of all proposed “interpretations” of the quantum formalism. This is commonly called the measurement problem, although, as Philip Pearle has rightly noted, it is rather a “reality problem.” Physics should aspire to tell us what exists (John Bell’s “beables”), and the laws that govern the behavior of what exists. “Observations,” “measurements,” “macroscopic objects,” and “Alice” and “Bob” are all somehow constituted of beables, and the physical characteristics of all things should be determined by that constitution and the fundamental laws.

What are commonly called different “interpretations” of quantum theory are really different theories—or sometimes, no clear theory at all. Accounts that differ in the beables they postulate are different physical theories of the universe, and accounts that are vague or noncommittal about their beables are not precise physical theories at all. Until one understands exactly what is being proposed as the physical structure of the universe, no other foundational problem, however intriguing, can even be raised in a sharp way.

**DAVID MERMIN** · Here are three.

*One:* In the words of Chris Fuchs, “quantum states: what the hell are they?” Quantum states are not objective properties of the systems they describe, 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.”

Consistent historians (see also my answer to Question 16, page 279) 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. 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. 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. (See also my answer to Question 4, page 102.)

*Two:* 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. On the other hand, those who 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, page 176.)

*Three:* 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.

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. 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 everything we can know about that world. (See also my answer to Question 14, page 256.) 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.



LEE SMOLIN · The measurement problem—that is to say, the fact that there are two evolution processes, and which one applies depends on whether a measurement is being made. Related to this is the fact that quantum mechanics does not give us a description of what happens in an individual experiment.

To put it differently, the only interpretations of quantum mechanics that make sense to me are those that treat quantum mechanics as a theory of the information that observers in one subsystem of the universe can have about another subsystem. This makes it seem likely that quantum mechanics is an approximation of another theory, which might apply to the whole universe and not just to subsystems of it. The most pressing problem is then to discover this deeper theory and level of description.

ANTONY VALENTINI · The interpretation of quantum mechanics is a wide open question, so we can't say in advance what the most pressing problems are. As the history of physics shows, it's only in hindsight that one can say who was looking in the right direction. What's important is that we leave the smoke screen of the Copenhagen interpretation well behind us, and that talented and knowledgeable people think hard about this subject from a realist perspective.

Instead of answering the question, I can offer a list of things I'd like to see done in the near future, as they seem important as far as I can tell.

It would be good if the ongoing controversy over the consistency of the Everett interpretation could be settled. It would be helpful to know if that theory really makes sense (on its own terms) or not. It would also be good to see further experiments searching for wave-function collapse. More generally, I'd like to see more experiments that test quantum theory in genuinely new domains—as in the recent three-slit experiment.

In modern theoretical physics, there are a number of important issues that deserve more attention from a foundations perspective, such as the question of Hawking information loss in black holes, and the problem of time in quantum gravity. The description of the quantum-to-classical transition in the early universe also deserves more foundational scrutiny.

As for my own current line of research—which focuses on the possibility of nonequilibrium violations of quantum theory, in de Broglie-Bohm theory and in deterministic hidden-variables theories generally—there are some outstanding issues that need a lot more work. One is the need for more detailed calculations and numerical simulations of relaxation to quantum equilibrium in the early universe, with the aim of obtaining precise predictions of where residual nonequilibrium violations of quantum theory might be found today—for example, in the cosmic microwave background or in relic cosmological particles. My work so far points in the direction of super-Hubble wavelengths as the area to look at, but much more remains to be done. I have also made some proposals to the effect that Hawking radiation could consist of nonequilibrium particles that violate the Born rule in a way that might avoid information loss, and there are a host of theoretical questions to be investigated to develop that proposal further.

Finally, there is the important general question of whether it's possible to construct a reasonable hidden-variables theory without an ontological wave function.

De Broglie–Bohm theory has several features that have been shown to be common to all hidden-variables theories (under some reasonable assumptions): nonlocality, contextuality, and nonequilibrium superluminal signaling. De Broglie–Bohm theory also has the feature of an ontological wave function, and it would be good to know if this is another common feature of hidden-variables theories or not. Alberto Montina has worked on this recently, but more needs to be done.

DAVID WALLACE · I think anyone’s answer to this is going to depend above all on what they think of the quantum measurement problem. After all, the measurement problem threatens to make quantum mechanics incoherent as a scientific theory—to reduce it, at best, to a collection of algorithms to predict measurement results. So the only reason anyone could have not to put the measurement problem right at the top of the list would be if they think it’s solvable within ordinary quantum mechanics. (Someone who thinks it’s solvable in some modified version of quantum mechanics—in a dynamical-collapse or hidden-variables theory, say—ought to think that the most pressing problem is generalizing that modified version to account for all of quantum phenomena, including the phenomena of relativistic field theory.)

As it happens, though, I *do* think the measurement problem is solvable within ordinary quantum mechanics: I think the Everett (“many worlds”) interpretation solves it in a fully satisfactory way, and while I think there are some philosophical puzzles thrown up by that solution—mostly concerned with probability and with emergence—that would benefit from more thought, I wouldn’t call them *pressing*. Not from the point of view of physics, at any rate.

So from my point of view, the “most pressing problems” aren’t going to be ultra-broad problems like, “What does quantum mechanics as a whole mean?” They’re going to be a bit more detailed, a bit more concerned with particular puzzling features of the conceptual and mathematical structure of quantum mechanics. (The advantage of the Everett interpretation—the main *scientific* benefit it’s brought, I’d say—is that it allows us to ask those questions without getting tangled up in worries about whether there are hidden variables or dynamical collapses or whatever not included in our equations, and without all sorts of doubletalk about “experimental contexts” and “the role of observers” and “subjective quantum states” and so on.)

All that said, here’s the problem that leaps out for me. Just how are we to understand the apparently greater efficiency of quantum computers over classical ones? When I started as a physics grad student in the late 1990s, we had two really great quantum algorithms—Shor’s algorithm, which factorizes large numbers, and Grover’s algorithm, which finds the biggest number in a list—and both of them were dramatically more efficient than the best-known classical algorithms. Shor’s algorithm in particular had had a huge impact, because the problem of factorizing large numbers *both* is one of the standard examples of a difficult computational problem, *and* is crucial in decoding a lot of codes that were and are thought to be basically undecodable by classical computers. So everyone who was working in quantum information—including me at the time—was very excited by this, and pretty much all of us thought that Shor’s and Grover’s algorithms were going to be the tip of

the iceberg, that there were going to be dozens or hundreds of these amazing quantum algorithms. But actually, ten years and more later, and those algorithms are still pretty much all we've got. Even if you could solve the technical problems involved in making a quantum computer that would fit on your desktop, at the moment there's not much you could do with it that you can't do with your existing classical desktop.

Now that's embarrassing for people writing grant applications. But it's also bizarre from a foundational point of view. It's one thing to discover that quantum mechanics has a completely different computer-complexity theory from classical mechanics. It's quite another to discover that it's almost identical but *not quite*. My hunch is that we're missing something pretty profound here.

The second problem I'd identify is a bit easier to attack, and indeed we've got quite a long way with it already, but there's further to go. It's fairly clear now that the really big mysteries in quantum theory come not so much from superposition as from entanglement (after all, classical electromagnetism admits superpositions). But getting a detailed quantitative grasp of what's going on in multipartite entanglement is really hard. We've got a variety of tools, and a variety of results, but it feels as if we still haven't found the right way of thinking about it, or maybe the right mathematical framework to use, such that it all becomes less opaque and less mysterious. (I think the very graphical "language" that Bob Coecke and his coworkers are developing is really promising here, but it's early days.)

I'll mention one more thing, which might not normally be classified as "quantum foundations"—and which I guess isn't exactly "pressing," because we've been stuck with it for decades. The last twenty or thirty years have made it really clear that quantum mechanics is way, way different from classical mechanics, and that it's possible to understand why the world looks classical without having to keep classical concepts as basic. (I'm thinking, in particular, of the role of decoherence theory, and the way we've basically managed to wean ourselves of the correspondence principle.) But the way we construct quantum theories, particularly in quantum field theory, is still almost invariably to start with a classical theory and then "quantize" it. That really, really shouldn't be necessary, but it seems to be. We need to find some way of thinking about quantum fields that doesn't require this link to classical fields.

ANTON ZEILINGER · We have learned from quantum mechanics that naive realism is not tenable anymore. That is, it is not always possible to assume that the results of observation are always given prior to and independent of observation. To me, the most important question is to find out what exactly the limitations are. This can only be found out by carefully exploring quantum phenomena in more complex situations than we do today.

A deep reanalysis of the fundamental concepts underlying quantum mechanics is also necessary, analogous to the careful analysis of the notions of space and time by the Viennese philosopher–physicist Ernst Mach. Mach's analysis paved the way for the abandonment of the notions of absolute space and time, and for their replacement by the modern notions in special and general relativity.

WOJCIECH ZUREK · Understanding the role of information; or, to be more precise, clarifying the relation between information and existence. I think that this was always—that is to say, since about 1925—the key. It is the essence of the measurement problem.

When you read Bohr, von Neumann, Wigner, Everett, or Wheeler, it is clear that they were aware of this. Bohr may not have had information theory at hand when he was thinking about matters of interpretation, but his insistence on the communicability of the measurement outcomes in everyday language points in that direction. Von Neumann and Wigner worried about the role of the conscious observer in the process, and the precondition for (and maybe even the essence of) consciousness is information acquisition and processing. Everett has long passages on information and quantum theory in his thesis, and he even devises an information-theoretic version of Heisenberg's indeterminacy principle. Wheeler's "It from Bit" goes further, by turning tables on the usual understanding of information as representing what exists and proposing that it might be the material that reality—the "It"—is made out of.

In a sense, the interplay between information and existence—between what is known and what exists—is older than quantum theory: it was central to physics since at least Boltzmann and Maxwell. The origin of the second law and the threat posed by Maxwell's demon are a premonition of the problems that are central in quantum theory. Indeed, one may defend the thesis that the quantum discoveries of Planck and Einstein (for example, stimulated emission) that paved the way for modern quantum theory happened because thermodynamics "knew" that information plays a central role in physics. One of the best illustrations of this interdependence is the famous (classical and thermodynamic) discussion of Szilárd, who in effect deduced—years before Shannon—some of the key ideas of information theory. It also puts the observer (the demon) squarely in the center of the action. This theme of the physical significance of information persists in quantum measurements.

So, already thermodynamics made it clear that "information is physical." Newtonian mechanics, however, allowed for a separation of what *is*—what exists—from what *is known*: a point in phase space is a legal representation of the state of a classical system, and it need not be altered by the observation aimed at making its location precise.

This separation of information from states was tenable in classical physics, but it breaks down in quantum theory—it breaks down in our universe. I think that by now many people recognize how central information is to quantum physics. On a technical level, this started with Heisenberg and his indeterminacy principle. But even with all that we know now about the interplay of quantum physics and information (including Bell's theorem, the no-cloning theorem, quantum error correction, and so on), I sense that the real mystery is still barely touched.



<http://www.springer.com/978-3-642-20879-9>

Elegance and Enigma  
The Quantum Interviews  
Schlosshauer, M. (Ed.)  
2011, XIII, 312 p., Hardcover  
ISBN: 978-3-642-20879-9