An Exchange of Letters in PHYSICS TODAY on *Quantum Theory Without Observers*

February 1999

In "Quantum Theory without Observers—Part One" (PHYSICS TODAY, March 1998, page 42), Sheldon Goldstein discusses our work on the decoherent histories (DH) approach to quantum mechanics and the related work of Robert Griffiths and Roland Omnès. He describes correctly many aspects of the research and makes a number of favorable remarks, such as "it seems likely that the program of DH can be brought successfully to completion." However, he seems to have misunderstood one important point, and as a result he mistakenly attributes certain "inconsistencies" to the program at its present stage. We always consider a "realm"—a set of mutually exclusive decoherent histories with probabilities adding to one—and we typically impose some further conditions on a given realm. (A "family," as discussed by Goldstein, consists of a realm and all its coarse grainings.) It is essential to restrict statements relating the probabilities of occurrence of histories to a given family containing them. (Here, we have in mind statements such as the following: If B happens at time t_2 and C at time t_3, then A must have happened at time t_1.) The restriction is necessary despite the fact that the numerical probability of a given history belonging to more than one family is independent of the family. This point has been stressed very strongly by Griffiths and Omnès.[1] Inconsistencies can arise if statements relating the probabilities of occurrence of histories are made while referring to different families in the course of a given argument. That is true even if the histories involve only a single time.

Goldstein mentions our efforts to understand what is so special about the "usual" realm defined by hydrodynamic variables averaged over small volumes and evaluated at short, albeit discrete, intervals of time. However, he seems to think that we start with the union of many different families (with the possibility of inconsistencies in statements connecting the probabilities of occurrence of various histories) and are trying to find conditions that will shrink this set to a single realm and its associated family, thus eliminating inconsistencies. That is not the case. Rather, we are comparing the properties of different realms or families, while restricting our statements in each case to a single family, thus encountering no inconsistencies along the way.

It is worth mentioning that the figure caption on the last page of the article is misleading. The photograph shows Richard Feynman and one of us (Gell-Mann), and the caption describes Gell-Mann as "one of the most sensible critics of orthodox quantum theory" and Feynman as "one of its most sensible defenders." In fact, both physicists held very similar views of quantum mechanics. Some months before Feynman's death in 1988, Gell-Mann described to a class at Caltech the status of our work on decoherent histories at that time. Feynman was in attendance, and at the end of the class, he stood up, and some of the students expected an exciting argument. But his comment was, "I agree with everything you said."

There is no question that the "orthodox" Copenhagen interpretation works in measurement situations and accurately predicts the outcomes of laboratory experiments. It is not wrong. Rather, it is a special case of the more general interpretation in terms of decoherent histories of the universe. The Copenhagen picture is too special to be fundamental, and it is clearly inadequate for quantum cosmology.

As Goldstein's title suggests, DH is a formulation of quantum mechanics in which observers do not play a fundamental role. We are working to perfect that formulation. However, we are not seeking, nor do we have, a formulation that implements Albert Einstein's idea of attributing "physical reality" to all quantities for which there are situations in which they can be measured with certainty. In DH, if two such quantities at the same time do not commute, measurements of them have to take place in different alternative histories of the universe.[2] Our work is not completely finished, but the research is not plagued by inconsistencies.

References

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2. M. Gell-Mann, The Quark and the Jaguar, W. H. Freeman, New York (1994), p. 172.

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Sheldon Goldstein's two-part article contains much valuable material. Unfortunately, his discussion of consistent histories is, in certain respects, misleading; at the very least, it is out of date. (Goldstein, following Murray Gell-Mann and James Hartle, uses the term "decoherent histories" for what Roland Omnès and I call "consistent histories.")

The logical structure of the consistent histories approach has been worked out in considerable detail by Omnès, and paying serious attention to his "Rule 4" [1] would have prevented Goldstein from making the erroneous assertion that the consistent histories formalism is rendered inconsistent by the results of Andrew Gleason; Simon Kochen and Ernst Specker; John Bell; and Lucien Hardy. My own recent work[2] has led to a quite systematic treatment of the whole subject, in which consistent history "beables" (the physical referents of the mathematical terms) are spelled out in considerable detail, and the formalism is shown to be complete as a fundamental theory, without need of the additional principles that Goldstein seems to think are necessary. Although the "primitive ontology" (to use Goldstein's term) of consistent histories was not presented in the earliest papers in as clear a form as is now possible—a quite common occurrence when important new ideas are introduced into physics—the fundamental ideas have not changed, and more recent work has confirmed the soundness of the basic strategy adopted by Gell-Mann and Hartle, Omnès and myself. (Readers interested in pursuing the subject further may wish to consult reference 3, which contains a response to various criticisms and misunderstandings of consistent histories, as well as simple examples that may make some of the ideas easier to follow.)

There is one aspect of consistent histories that was perfectly clear in the very first paper on the topic and in all our subsequent work: a quantum history consists of a sequence of events at successive times, and these events correspond to subspaces of the quantum Hilbert space. In standard quantum theory, a wavefunction is associated with a one-dimensional subspace of the Hilbert space, whereas subspaces of higher dimension correspond to collections of wavefunctions. Thus, wavefunctions are the building blocks out of which histories are constructed, and it is difficult to understand why Goldstein asserts that, in the consistent histories approach, "the wavefunction is by no means the complete description of a quantum system; it is not even the most important part of that description." It is Bohmian mechanics, not consistent histories, that needs ("hidden") variables in addition to the standard Hilbert space of wavefunctions for its beables, and in this respect the approaches are actually quite different, despite Goldstein's efforts to find some parallels.

References

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GOLDSTEIN REPLIES TO GELL-MANN AND HARTLE AND TO GRIFFITHS: The main complaint in these two letters concerns my assertion that the decoherent histories (DH) approach is inconsistent (unless the basic decoherence condition is augmented by additional fundamental set selection principles). Before addressing this complaint, though, I think it helpful to look again at the example I presented in my article (March 1998, page 45) to illustrate the inconsistency.

For a certain quantum state ψ, say at time $t = 0$, for a pair of spin-$\frac{1}{2}$ particles, there are spin components A, B, C and D (also at $t = 0$) for which the DH approach yields the following four statements concerning joint probabilities P:

1. $P(A = 1, B = 1) = 0.09$.
2. $P(A = 1, C \neq 1) = 0$.
3. $P(B = 1, D \neq 1) = 0$.
4. $P(C = 1, D = 1) = 0$.

Corresponding to these four statements are four pairs of commuting observables and four decoherent families (the sort of families to which DH assigns probabilities): the AB family, the AC family, the BD family and the CD family. However, these families cannot be combined into, say, an $ABCD$ family, and thus DH does not supply us with probabilities for simultaneous values of A, B, C and D.

It is important to appreciate that, for orthodox quantum theory (and, in fact, even for Bohmian mechanics), the four statements above, if used properly, are not inconsistent, because they then would refer merely to the outcomes of four *different* experiments, so that the probabilities would refer, in effect, to four different ensembles.

However, the whole point of DH is that such statements refer directly, not to what would happen were certain experimental procedures to be performed, but to the probabilities of occurrence of the histories themselves, regardless of whether any such experiments are performed. Thus, the statements refer to a single ensemble of systems, for about 9% of which, according to the first statement, both A and B are 1; for none of which, according to the second statement, can A be 1 without C also being 1; and so on.

As such, the four statements above are obviously inconsistent, since it follows from statements 1, 2 and 3 that, contrary to statement 4, in at least 9% of the systems in the ensemble, C and D are both 1. This is the inconsistency to which I referred in my article.

Concerning this issue, Murray Gell-Mann and James Hartle complain that I have "misunderstood one important point"—namely, that "it is essential to restrict statements relating the probabilities of occurrence of histories to a given family containing them" because "inconsistencies can arise if statements relating the probabilities of occurrence of histories are made while referring to different families in the course of a given argument." I am puzzled by their response. Each of my four individual statements concerns only probabilities for a single family (with, of course, a different one for each statement). And the fact that "inconsistencies can arise . . . " is precisely the point of the example I used in the article and am using here.

Robert Griffiths is more explicit about the cause of my having made "the erroneous assertion that the consistent histories formalism is . . . inconsistent"—namely, my not "paying serious attention to [Omnès's] Rule 4." Here is the rule, as given on page 163 of the reference Griffiths mentions: "Any description of the properties of an isolated physical system must consist of propositions belonging together to a common consistent logic. Any reasoning to be drawn from the consideration of these properties should be the result of a valid implication or of a chain of implications in this common logic." What Omnès calls a "consistent logic" amounts more or less to a (decoherent) family.

I have always had great difficulty with this rule. I don't understand what it actually means, in terms of both detail and basic meaning. Does the description provided by the four statements in my example, which requires reference to four families, violate this rule because the four statements are on adjacent lines? What if they were on different pages, or were made by different people? It can hardly be expected that, when thinking about the same system, all people at all times will—by some peculiar harmony—formulate statements concerning only the same common family.

Besides, why are my four statements not a counterexample? They *are* a description of precisely the sort that Rule 4 informs us "must" not be. This raises the question as to exactly what is meant in the rule by "must," and, in its next sentence, by "should."

The real problem, I believe, is this: If we "must" or "should" restrict our descriptions and reasoning in the manner described by Rule 4, it must be because of the meanings of the statements under consideration and the way the language expressing them is intended to function. For example, if (as would be appropriate in orthodox quantum theory) we were to use the four statements above as an elliptical way of talking about results of possible experiments, then it is apparent that we could get into trouble by considering, at one time, several of these statements, should we slide into the mistake of thinking that the several statements refer to a common experiment.

However, if, for DH, descriptions such as those provided by the statements above are to be understood with their usual meanings, then Rule 4 is simply false, to the extent that it has any meaning at all. And if the proponents of DH have some other meaning in mind for such statements, they should so inform us and supply this meaning—something that, as far as I am able to tell, they nowhere do.

It may be argued that Rule 4 should be regarded as merely a rule—that is, as merely defining a certain game. But then why must I play this game when analyzing the implications of DH?

It is true that, to deduce or recognize that the four statements above are inconsistent, we must consider a collection of statements involving more than a single family. If we obey Rule 4 in our analyses, we will encounter, as Gell-Mann and Hartle say, "no inconsistencies along the way." But the statements will remain inconsistent even if we invoke and adhere to rules that demand, in effect, that we ignore the inconsistency.

In my article, I tried to present the DH approach in what I deemed the best possible manner. Whatever its vices, this version, based on an augmented decoherence condition, has the virtue of consistency.

Griffiths finds it "difficult to understand" why I say that, for DH, the wavefunction of a physical system does not provide a complete description of the system. Let's focus, therefore, on the simplest possible example to illustrate my point. Suppose that at, say, $t = 0$, a single spin–$\frac{1}{2}$ particle is in a quantum state ψ with $\sigma_z = 1$, and suppose we consider the single-time (hence, decoherent) family corresponding to the value of σ_x at this time. Then, for about half of the members of a large ensemble of systems in this state, the value of σ_x is 1, and for these individual systems the quantum state ψ, which is a superposition of the eigenstates of σ_x, provides only partial information.

Griffiths claims that "wavefunctions are the building blocks out of which histories are constructed." In the preceding example, the history $\sigma_x = 1$ can be regarded (ignoring the other degrees of freedom) as corresponding to a wavefunction—namely, the associated eigenstate. But this wavefunction is by no means the quantum state ψ of the system, which remains, for DH, an incomplete description of that system. Insisting that histories be regarded as constructed out of wavefunctions makes it more difficult to appreciate this fact and obscures the dynamical character of the role played for DH by the quantum state ψ of a system.

Why, indeed, does Griffiths insist upon so playing with words? Are there any good reasons for doing so, beyond supporting the insinuation that DH involves only pure quantum concepts, and beyond sustaining the illusion that, unlike Bohmian mechanics, it involves no additional "hidden" variables? **—SG**

Sheldon Goldstein conjectures that "hardly anybody truly believes . . . anymore" in the Copenhagen interpretation of quantum mechanics, especially in "the notion that quantum mechanics is about observation or results of measurement" (March 1998, page 42). From discussions with a number of colleagues, I know that I am not the only person to whom the Copenhagen interpretation remains one of the most significant intellectual achievements of our century. Therefore, Goldstein's conjecture is certainly incorrect.

I suggest that the very austerity of the Copenhagen interpretation, unsurpassed by that of any other interpretation of quantum mechanics, speaks very much in its favor. Indeed, its basic attitude toward the fundamental role of observation represents a major intellectual step forward over naive classical realism. In classical physics, observation is often regarded as a secondary concept, with the elements of the real world being primary. Yet, it is obvious that any statement about nature has to be based on observation. What could then be more natural than a theory in which observation plays a more fundamental role than in a classical worldview? What could be more sensible than the theory itself acknowledging that any statement about the world has to make reference to observation?

Schrödinger's cat is paradoxical only if one insists on pressing orthodox quantum theory into service—as many naively do—to imply that (in Goldstein's words) "the cat is somehow both dead and alive until an observer checks to see" (March 1998, page 43). Doing that reflects a serious misunderstanding. All the quantum state is meant to be is a representation of the catalog of our knowledge of the system. It is precisely that catalog that is necessary to arrive at the maximum possible set of predictions for all possible future observations of the system.

The revolutionary new feature of quantum physics arises whenever there is no way, not even in principle, to tell which of various possibilities is the case. Then, instead of just having to acknowledge our ignorance, as we would have to in classical physics, quantum superposition comes in as a qualitatively new property. If the condition above should ever be realizable for the dead and live states of a cat, its quantum state has to be a superposition of these states. That clearly does not mean the cat is both alive and dead. It means only that no definite statement can be made concerning the question of whether the poor animal is alive or dead. Upon observation, we will find it in either state and thus the state assigned to the cat collapses into either possibility.

It is not at all surprising that we have to change the representation of our knowledge if that knowledge changes because of information obtained by observation of the cat. The collapse of the state vector can be seen only as a "measurement paradox" if one views this change of the quantum state as a real physical process. In the extreme, it is often even claimed that something happens to the cat because it is being observed. There is no basis for any such claim. In contrast, what can be more natural than to change the representation of our knowledge—that is, the quantum state—if we gain new knowledge from a measurement performed on the system? Any statement about what is the case in the world can then be obtained only with explicit reference to observation. Indeed, as in the case of the measurement paradox, the paradoxes constructed by opponents of the Copenhagen interpretation are always based on some realistic pre-quantum notions about how the world ought to be brought into the discussion through the backdoor. In fact, there is never a paradox if we realize that quantum mechanics is about information. Actually, such a view also leads to a most natural understanding of new phenomena in quantum computation and quantum communication, such as quantum entanglement, quantum nonlocality or quantum teleportation. Then, no need whatsoever arises to allude to such notions as superluminal or instantaneous transmission of information.

It is very much to the credit of proponents of alternative approaches such as Goldstein that, beginning with Albert Einstein in the early 20th century, they have realized—often in a much deeper way than have the adherents of the orthodox view—how novel and counterintuitive some features of quantum theory are. Progress, however, will not be made by trying to return to pre-quantum notions.

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GOLDSTEIN REPLIES TO ZEILINGER: Because of the strong disagreement expressed by Anton Zeilinger in the first two paragraphs of his letter, I was surprised to find myself agreeing with much, if not most, of what he said after that—namely, that Schrödinger's cat paradox is indeed a consequence of what Zeilinger terms "a serious misunderstanding" of the role of the quantum state; that when the quantum state of the cat is in a superposition of dead and alive, then (in Zeilinger's words) "that clearly does not mean the cat is both alive and dead [but] only that no definite statement can be made" on the question, because we are, and must be, ignorant as to the fact; and that what he calls a "natural understanding" of the new quantum phenomena based on entanglement neither requires nor involves any "superluminal or instantaneous transmission of information." These things are as true for Bohmian mechanics—to take my favorite quantum theory without observers—as they are for orthodox quantum theory as understood by Zeilinger.

Even the last sentence of his letter, while a bit too dogmatic for my taste, is one for which I have qualified sympathy. However, his denigration of "pre-quantum notions,"—which can mean anything from what he calls "naive classical realism," to variables not definable (or defined) in terms of Hilbert space structure, to the possibility of any sort of observer-independent reality—must be taken with a grain of salt as being merely an appeal to prevailing prejudices.

But does Zeilinger truly believe that "quantum mechanics is about information"? Information is always information about something. Therefore, shouldn't quantum mechanics then be regarded as being about that something? Quantum mechanics tells us about atoms and chemical bonding and high-temperature superconductivity. Of course, it also provides us with information about these things. But it does so precisely because it is about the things themselves.

And does Zeilinger really wish to deny that the change of the state vector that occurs during the measurement process is "a real physical process," even when it leads to the destruction of the possibility of interference? Can quantum interference be genuinely understood by invoking a wavefunction that is nothing more than "a representation of our knowledge"?

Moreover, it would not be at all sensible for a theory to acknowledge that "any statement about the world has to make reference to observation," since Zeilinger's assertion is plainly false. Statements about history are not statements about history books, and statements about dinosaurs are not statements about fossilized dinosaur bones. And even statements concerned with the present, though they are typically based rather directly on observations—if not our own, then somebody else's—are usually not *about* those observations. Although it is presumably true that the *justification* of any statement about the world must be based, at least in part, on experience or observation, there is nothing in Zeilinger's assertion that "any statement about nature has to be based on observation" to suggest, or even make plausible the idea, that observation has a fundamental role to play in the formulation, as opposed to the justification, of physical theory.

What Zeilinger terms "the austerity of the Copenhagen interpretation" is very much like the austerity of solipsism, and it suffers from similar defects. What results from this austerity is not merely implausible, but also deficient in the theoretical simplicity afforded by an appeal to something outside ourselves. **—SG**

Sheldon Goldstein invokes the Einstein-Podolsky-Rosen (EPR) paradox to suggest that "the quantum mechanical description is not the whole story ..." (March 1998, page 43).

Although every college physics student has heard of the EPR paradox, and most have read the original paper in *Physical Review*,[1] far fewer students of quantum mechanics have read the rebuttal to EPR that was printed in *Physical Review* a year later by Yale University professor Henry Margenau.[2] Margenau showed that Einstein's objections to quantum mechanics relied on the von Neumann projection postulate, which says that the measurement procedure "collapses" the state of the system to the eigenstate of the measured observable with the recorded eigenvalue. As Margenau made plain, the EPR paradox vanishes if the von Neumann projection postulate is abandoned.

Margenau and his student James Park proved that the von Neumann projection postulate is simultaneously "absurd, false, and useless."[3]

1. Why the postulate is "absurd." The concept of a quantum state is inherently statistical. "Quantum state" refers to the identical preparation of an ensemble of identical systems. One determines the state by recording the eigenvalues (in principle an infinite number of times) to get expectation values of a "quorum" of observables. Doing so in effect describes the preparation that yielded the state of the systems in the ensemble (before the measurement).

However, the projection postulate says that a measurement on a single system determines the state of the system (after the measurement). But this contradicts the statistical nature of the quantum state. By itself, a single measurement in quantum mechanics cannot disclose a state unless one has additional information not provided by that single measurement. Park gave the example of a school where one knows that all the students are of the same gender; a measurement on a single student determines the gender of the whole student population.

2. Why the postulate is "false." Realistic measurement procedures exist that violate the projection postulate.[4] For example, some measurements destroy the state completely, such as a photon hitting a phosphorescent screen (measuring position), or a silver atom hitting a plate in a Stern-Gerlach experiment (measuring spin and position).

Other measurements could not possibly yield an eigenstate of the observable since the eigenstate would contradict certain aspects of the experimental setup. For example, if recording an eigenvalue of the momentum operator of a one-dimensional particle-in-a-box left the particle in a momentum eigenstate, e^{ikx}, the particle would no longer be in the box, even if the walls were infinitely high; the particle would be distributed uniformly over all space. As another example, if recording an eigenvalue of the position operator of a free particle left the particle in a position eigenstate, $\delta(\mathbf{x}-\mathbf{x_0})$, the particle would

subsequently suffer infinitely quick diffusion.
3. Why the postulate is "useless." No quantum mechanical calculations require the invocation of the von Neumann projection postulate. Axiomatic quantum mechanics works just fine with the standard axioms: (a) definition of the quantum state, represented by a density operator in Hilbert space (unit trace, Hermitian, nonnegative and so on); (b) representation of most observables by linear, Hermitian operators in Hilbert space; and (3) some form of equation of motion to evolve the state in time.

Abandoning the von Neumann projection postulate resolves the EPR paradox without having to concoct a new quantum mechanics.

References

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GOLDSTEIN REPLIES TO NACHTRIEB: Whereas the projection postulate is, for Zeilinger, a triviality, for Robert Nachtrieb it is an absurdity. The truth probably lies somewhere between these extremes. However, regardless of the status of the projection postulate, it is not true that, as Nachtrieb claims, abandoning it "resolves the EPR paradox." This is because the EPR argument does not require the invocation of this postulate. What it does require, in addition to the assumption of locality, is merely the quantum mechanical predictions for the correlations between the outcomes of certain experiments—predictions that have repeatedly been confirmed. Recent formulations of the EPR argument rarely, if ever, appeal directly to the projection postulate. And even in the original EPR paper, a version of the argument that made no such appeal was presented (alongside one that did).

In my article, moreover, I referred to the EPR paper merely to quote its conclusion, that "the wave function does not provide a complete description of the physical reality." To support that conclusion, I neither relied upon nor mentioned the EPR argument, but rather invoked entirely different considerations.

I note that if it could be argued convincingly that the projection postulate is "absurd," it would hardly be necessary to argue that it is also "false and useless." But it is none of those things. Rather, it is merely limited, an idealization useful for the analysis of a restricted class of experimental situations. **—SG**

Sheldon Goldstein describes several reformulations of quantum mechanics that attempt to do away with the notion of an observer. He has, I would argue, overlooked a much simpler formulation than any of those he discussed—namely, what I shall call Bohmian quantum mechanics I (BMQI), to distinguish it from the Bohmian mechanics (BMQII) discussed in the second part of his article (April, page 38).

The fundamental feature of BMQI, described in David Bohm's great book *Quantum Theory*,[1] is the analysis of the measurement process as a physical process. In this analysis, Bohm recognized that a measuring apparatus is itself a physical system whose dynamics is also described by the laws of quantum mechanics. A measurement is then considered to be an interaction, again described by the laws of quantum mechanics, between the observed system and the apparatus. No additional postulates are needed to determine the consequences of the measurement process, such as an assumed collapse of the wavefunction. In particular, much of the mystery of the Copenhagen interpretation is avoided by using this approach. Bohm summarizes his analysis by concluding that "we are able to obtain a complete objective description of the process of the measurement, which does not involve human observers in any way at all" (page 607).

The notion that a measuring apparatus is itself a physical system is not unique to quantum mechanics. Many of the difficulties one encounters in, for example, both special and general relativity—such as the so-called clock paradox—can be avoided by dispensing with the assumption that ideal clocks measure proper time and by introducing simple physical models for clocks. I would argue, in fact, that no physical theory is complete unless it contains within itself a complete description of the measurements it describes.[2]

One puzzle remains: Why did Bohm in effect renounce BQMI by introducing BQMII, given that BQMII requires postulates—such as *ad hoc* restrictions on initial conditions—that are not needed in BQMI? Furthermore, what problem was solved by BQMII that could not be dealt with by BQMI? To the best of my knowledge, there is no way to distinguish between the two. Thus, a judicious application of Occam's razor would surely favor BQMI over BMQII.

As a postscript, I urge anyone interested in the measurement problem (and even those who are not) to read chapter 22 of Bohm's book.

References

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GOLDSTEIN REPLIES TO ANDERSON: I very much agree with James Anderson's assertion that "no physical theory is complete unless it contains within itself a complete description of the measurements it describes," at least insofar as potentially fundamental physical theories are concerned. However, in the final chapter of his great book (page 625), Bohm concluded that "without an appeal to a classical level, quantum theory would have no meaning. *We conclude then that quantum theory presupposes the classical level and the general correctness of classical concepts in describing this level; it does not deduce classical concepts as limiting cases of quantum concepts*" (emphasis in original).

Bohm's comment points to an important distinction between BQMI and BQMII, and it helps us understand why, little more than a year after finishing his book, Bohm did in fact "renounce BQMI by introducing BQMII." Morover, the abstract of the paper in which Bohm presented BQMII concludes as follows: "In any case, the mere possibility of such an interpretation proves that it is not necessary for us to give up a precise, rational, and objective description of individual systems at a quantum level of accuracy."[1]

Bohm's idea that, in Anderson's words, "a measuring apparatus is itself a physical system whose dynamics is also described by the laws of quantum mechanics"—a notion that goes back at least to John von Neumann or Nevill Mott and certainly did not originate with Bohm—is required for the very formulation of the measurement problem, not its resolution. As far as the measurement problem is concerned, this idea, with which I agree, is part of the question, not part of the answer.

Anderson implies that I wish "to do away with the notion of an observer." I do not.

Reference

1. D. Bohm, Phys. Rev. 85, 166 (1952).

—SG

Sheldon Goldstein should realize that standard quantum mechanics in itself is technically observer-free: As Goldstein notes, it predicts, for an observable, "the distribution of the value that would be found were the appropriate measurement performed" (March 1998, page 45). Thus, no observers are actually required.

Naming the winner of the debate between Niels Bohr and Albert Einstein is still at issue, despite Goldstein's assertion favoring Einstein. As Goldstein notes, Einstein believed that quantum mechanics might be superseded some day by a more complete theory. However, it can be argued that the current evidence, including that stemming from the work of John Bell and Lucien Hardy, is most simply explained by some form of Bohr's conception of quantum states. In that conception, dynamical variables that characterize a quantum state are defined in connection with specific experimental arrangements, rather than as elements of reality associated solely with the object, as Einstein would have wanted.

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GOLDSTEIN REPLIES TO DOTSON: Concerning Allen Dotson's quibble about measurement versus observation, I could have called my article "Quantum Theory without Observers or Measurements"; however, the title I chose is awkward enough.

The work of Bell and Hardy to which Dotson refers is very interesting, particularly with regard to nonlocality. But it is not very relevant to deciding between, say, Bohmian mechanics and orthodox quantum theory, which account for the phenomena discussed by Bell and Hardy in a surprisingly similar manner: For both of those theories, the "dynamical variables" (corresponding to the outcomes of the experiments) that Bell and Hardy discuss should be regarded as "defined in connection with specific experimental arrangements," to use Dotson's phrase. **—SG**

At first glance, Bohmian mechanics and its "explanation" of the two-slit experiment (April 1998, page 40) look marvelous. But isn't there some difficulty in applying these ideas to problems involving tunneling, where the (quantum) kinetic energy is negative?

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Regarding part two of Sheldon Goldstein's article (April 1998, page 38), it should be noted that the de Broglie-Bohm theory derives hidden particle trajectories and velocities that are compatible with Schrödinger's equation by identifying them with the quantum mechanical probability current and flow velocity, respectively.

This identification provides an intuitively satisfying description of open systems with more or less unidirectional particle flow—two-slit interference, for example—in which the fringes are formed by objectively real particles following deterministic trajectories. However, it does not produce a satisfactory description of hidden trajectories in bound systems such as atoms, in which some or all of the components of the probability current are zero (under such circumstances, flow velocities reveal little about hypothetical particle velocities). The Bohm interpretation ascribes this condition to electrons that are stationary under the influence of counterbalancing gradients of the classical potential and a quantum potential—at least in some directions. Although this picture is self-consistent, a static model of atomic electrons does not have the intuitive appeal of a dynamic one, and intuitive appeal is the point of such models, since we cannot observe the trajectories.

Eventually, a dynamic model of the atom may be discovered that has hidden trajectories that are both computable and compatible with quantum mechanics. Meanwhile, it may be better to just say that we are at liberty to postulate hidden deterministic particle trajectories that more or less follow the probability current in open systems (such as the two-slit interference experiment), and that form closed orbits in bound systems (such as atoms) consistent with some or all of the components of the probability current being zero—and to leave it at that.

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GOLDSTEIN REPLIES TO HOOVER AND BRADFORD: My answer to William Hoover's question is, no. A particle moving according to the Bohmian equations of motion can do things that would be impossible classically. That is because Bohmian mechanics is not classical mechanics. Tunneling is a prediction of—not a problem for—Bohmian mechanics. It should be regarded as a virtue that such seemingly paradoxical behavior is explained with so little difficulty—as in fact it is in Bohmian mechanics. Moreover, the explanation does not involve any appeal to imaginary velocities.

Henry Bradford faults the Bohm interpretation because what it yields is sometimes too simple. Atomic electrons in certain stationary states are, for Bohmian mechanics, at rest. Bradford complains that this is nonintuitive. What this presumably means is that it conflicts with our classical intuitions, as well as with the Solar System model of the (Bohr) atom that we first learn. In other words, the behavior is unfamiliar. But why should a new theory predict only familiar behavior?

By suitably complicating its defining dynamical equations, we could transform Bohmian mechanics into a theory in which atomic electrons move in a manner more consistent with our prejudices. But such consistency would be of far less value to me than the simplicity sacrificed to obtain it.

I would not say that the point of models like Bohmian mechanics is what Bradford calls "intuitive appeal." Nor is the problem with quantum theory that it is nonintuitive. Rather, the problem is that quantum theory is unprofessionally subjective and vague—if not downright incoherent. And the root of that problem is that it is not at all clear what quantum theory is really about.

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