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Readers of the article by Yakir Aharonov, Sandu Popescu, and Jeff Tollaksen might be interested in an alternative time-symmetric formulation of quantum mechanics, known as "consistent histories," that was developed over roughly the same time period as Aharonov's work (see the article by Robert Griffiths and Roland Omnès, PHYSICS TODAY, August 1999, page 26). Closely related is the "decoherent histories" approach of Murray Gell-Mann and James Hartle,[1] but as that is not usually formulated in a way that is transparently time-symmetric, the following remarks refer to the consistent histories approach; see reference 2 for an up-to-date formulation.

Both the consistent histories approach and that of Aharonov and coworkers pay attention to events at several different times, are formulated in a way that is time-symmetrical, and address a number of quantum paradoxes. Both are consistent with the calculational procedures taught to students in a typical quantum mechanics course, so they are "standard quantum mechanics," without the additional variables of de Broglie–Bohm or the additional collapses of Ghirardi-Rimini-Weber. And both approaches do not accept the "shut up and calculate" mentality that alas continues to dominate much classroom instruction. So far as I can tell, all the results mentioned by Aharonov and coauthors and in the earlier work they cite are fairly readily translated into the language of consistent histories, though the reverse is not true (see below); therefore, the consistent histories view is more general.

In the treatment by Aharonov and coauthors, *measurement*, as in textbook quantum theory, remains a black box: It collapses the wavefunction, but nothing more can be said. And for good reason: The textbook approach of introducing probabilities by reference to measurement yields what appear to be insoluble difficulties if one attempts to apply quantum theory to the measurement process itself—that is, to actual apparatus constructed out of entities that are quantum mechanical. In the consistent histories approach, that difficulty does not arise, because it treats quantum dynamics as fundamentally probabilistic, not deterministic, and the same rules apply to measurements as to all other physical processes. Speaking metaphorically, the probabilistic approach used in consistent histories allows one to open the black measurement box and watch the quantum gears turn.

The other major difference between the two approaches is their treatment of quantum paradoxes. We owe many of the most striking and delightful paradoxes of quantum theory to Aharonov and his coworkers, and he and Daniel Rohrlich have written a book on the topic.[3] But he leaves the paradoxes largely unresolved; the reader is encouraged to study but not unravel them. The consistent histories approach is exactly opposite: Paradoxes should be—and a large number of them have been—resolved by the correct application of well-formulated and fully consistent quantum principles (see reference 2, chapters 19–25).

Students new to quantum theory are often confused and deserve reasoned responses to their queries. Although paradoxes are valuable illustrations of how the quantum world differs from our everyday experience, I prefer to provide students with the conceptual tools needed to resolve and make sense of them. In particular, students benefit from learning a fully consistent approach to probabilities in the quantum domain, one not based on measurements but on general quantum principles. A colleague and I have just finished using that approach in teaching the first term of our introductory graduate quantum mechanics course. Although it requires extra time and effort to learn how to think about quantum processes rather than just do calculations, the reward comes in a deeper understanding of how the real (quantum) world works.

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Aharonov, Popescu, and Tollaksen reply: We thank the letter writers for their interest and for the opportunity to better clarify our ideas.

Michael Nauenberg and Art Hobson make essentially the same point—namely, that our ideas are completely wrong. To put their criticism in the right context, we point out that the outcome of our research program is twofold. First, we have discovered an *entirely new class of quantum effects*; second, we present a *new way of thinking* about quantum mechanics.

The fact that quantum mechanics predicts the effects we discovered is just that, a fact. The effects are computed using standard quantum mechanics, without additions or modifications. As such, their prediction by quantum mechanics is beyond doubt (unless one suspects algebraic mistakes). Furthermore, many of our effects have been verified experimentally; in particular, different versions of our amplification method have been used as novel technological tools. Both Nauenberg and Hobson completely ignore our effects. But one should not ignore them. They are novel and they are strange. Even more, they don't appear in isolation, but they form a well-structured pattern. Surely there is a lesson here that quantum mechanics wants to teach us; one ignores it at one's peril.

On the other hand, our way of looking at quantum mechanics is certainly unconventional; it introduces new concepts, and it approaches old concepts in a new way. That is essentially what the two letter writers point out, Hobson most emphatically when he writes that our article "is riddled with errors." We are criticized for thinking in a different way and for asking new questions. But our way of thinking leads to the same predictions as the conventional way, so as far as experiments are concerned they are completely equivalent. As Richard Feynman says in his book *The Character of Physical Law* (Modern Library, 1994), suppose we have "two theories" that "have all the consequences . . . exactly the same. . . . How are we going to decide which one is right? There is no way by science, because they both agree with the experiment to the same extent." So the criticism is baseless.

At the same time, if our approach is completely equivalent to the standard one, why bother? Again Feynman gives the best answer: "For psychological reasons . . . , these two things may be far from equivalent, because one gives a man different ideas from the other. . . . There will be something, for instance, in theory A that talks about something, . . . but to find out what the corresponding thing is . . . in [theory] B may be very complicated—it may not be a very simple idea at all." As a consequence, a new way of thinking allows one to ask new questions that, although they could be asked in the old theory as well, would have been very difficult to even envisage. That is precisely what we did. First, we raised the issue of the physics in pre- and postselected ensembles. And to reply to Hobson, no, there is nothing "erroneous" in the process of postselection. Postselection is a question about results of experiments, and every question about the results of actual measurements is legitimate. Subsequently, we discovered the concept of weak measurements, which in turn led us to discover the various effects we presented. Since the power of any new approach is given by its ability to predict new effects, one should conclude that ours is strong indeed.

Furthermore, as many physicists agree, an intuitive understanding of quantum mechanics is still missing. That is why quantum physicists are surprised over and over again by the discovery of strange and unexpected fundamental effects. We hope that our new way of thinking is a step toward the long-sought intuition. Even more important, the new way of thinking may give us new ideas about what to change, if experiments ever turn out to contradict quantum mechanics and therefore require its modification. In particular, since we tinker with the idea of time—one of the most important concepts in physics—starting the change from there may be a very potent method.

Shaul Mukamel refers to our experiment in which the component along some given axis of a spin-½ particle is found, by a weak measurement, to have the value $\sqrt{2}/2$ which is $\sqrt{2}$ times larger than the largest eigenvalue. He suggests an alternative explanation based on a classical vector model of spin. According to his explanation, values up to $\sqrt{3}/2$ should be possible. However, we presented the experiment showing $\sqrt{2}/2$ only because it was mathematically simple; by choosing a different postselection, we could have obtained, as results of weak measurements, values as large as we wanted. Hence the above simple classical-vector view doesn't work.

Robert Griffiths points out that there are two other time-symmetric formulations of quantum mechanics besides ours—"consistent histories" and "decoherent histories." In particular, Griffiths is right when he emphasizes a major difference in spirit between our theory and consistent histories. The main goal of consistent histories is to find an explanation for the (apparent?) collapse of the wavefunction during a quantum measurement; although that solution is hotly disputed, as are all other solutions to the collapse problem, it is certainly a very ingenious one. However, an answer to the collapse problem is not our primary interest (though we are starting to see glimmers of an alternative answer to it using our approach).

The letter by Griffiths, however, runs the risk of being misread as implying that solving the collapse problem will by itself clarify most or all of the counterintuitive aspects of quantum mechanics. That conclusion would be wrong. Quantum mechanics is strange and unusual and defies intuition in many ways; solving the collapse problem is by no means its only interesting fundamental issue. Nor can the solution of that one problem lead to a complete understanding of quantum phenomena. That particles tunnel in the first place is surprising by itself; even more so is the fact that, as we showed, perfectly good measuring devices, working with arbitrarily high precision, indicate consistently that the tunneling particles have negative kinetic energy. Equally surprising is that we can arrange a situation in which perfectly good measurements, made with as high a precision as we want, indicate that spin-½ particles have arbitrarily large spin. And these are only two examples. As far as we are aware, none of the many proposed solutions to the collapse problem make these effects seem less surprising, let alone predict them.

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On the reuse of US Navy reactors

Several years ago I worked at Naval Reactors (NR), the US government office