# letters

continued from page 9

## References

1. R. R. Ernst, G. Bodenhausen, A. Wokaun, *Principles of Nuclear Magnetic Resonance in One and Two Dimensions*, Clarendon Press, New York (1978).
2. S. Mukamel, *Annu. Rev. Phys. Chem.* **51**, 691 (2000); *Phys. Rev. A.* **61**, 21804 (2000); *Principles of Nonlinear Optical Spectroscopy*, Oxford U. Press, New York (1995).

**Shaul Mukamel**
*(smukamel@uci.edu)*
*University of California, Irvine*

**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.

## References

1. M. Gell-Mann, J. B. Hartle, in *Complexity, Entropy and the Physics of Information*, W. H. Zurek, ed., Addison-Wesley, Redwood City, CA (1990), p. 425.
2. R. B. Griffiths, *Consistent Quantum Theory*, Cambridge U. Press, New York (2002); available online at http://quantum.phys.cmu.edu/CQT.
3. Y. Aharonov, D. Rohrlich, *Quantum Paradoxes: Quantum Theory for the Perplexed*, Wiley, Weinheim, Germany (2005).

**Robert B. Griffiths**
*(rgrif@cmu.edu)*
*Carnegie Mellon University*
*Pittsburgh, Pennsylvania*

**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