letters

Time-symmetric quantum mechanics questioned and defended

In their feature article "A Time-Symmetric Formulation of Quantum Mechanics" (PHYSICS TODAY, November 2010, page 27), Yakir Aharonov, Sandu Popescu, and Jeff Tollaksen state that there exists a "freedom to impose independent initial *and* final conditions on the evolution of a quantum system" without having to modify quantum mechanics "by an iota." The supporting illustrations they give, however, are based on an inadequate analysis of the measurement process in quantum mechanics.

Consider their *gedanken* experiment in which measurements are made at two successive times, t and t_1, after the system has been prepared in a state Ψ at $t_0 < t < t_1$. Now suppose that the experiment is repeated, but without any measurements made at t_1. Then the standard statistical prediction of quantum mechanics for the outcome at the intermediate time t is identical in both experiments, contradicting the authors' claim that "the results at [the intermediary time] t depend not only on what happened earlier at t_0, but also on what happens later at t_1."

To illustrate their arguments, the authors describe some measurements of polarization with spin-½ particles as follows: "We could, for example, start at t_0 with an ensemble of spin-½ particles, each one polarized 'up' in the z-direction. Then at t_1 we measure each spin in the x-direction and select only the particles for which the spin turned out to be up again, but in the new direction. Thus, at any intermediate time t, the spin components in both the z and the x directions—two noncommuting observables—would seem to be completely determined." The rationale given for that strange conclusion is that "if at t we measure the spin along x, we must also find it up [along x], because otherwise the measurement at t_1 wouldn't find it up [along x]." But that claim is incorrect. Selecting, after a measurement, a subset of particles with spin up along a given axis does not imply that before the measurement such particles had spin up along that axis. On the contrary, if some particles at t_1 also emerge with spin down along x, then, according to quantum mechanics, the state Ψ at $t < t_1$ does not represent particles polarized along the x-direction. To find that state, the axis of the measurement device—for example, a Stern–Gerlach magnet—must be rotated until all particles emerge with the same direction of polarization. For the example under consideration, that would be the direction of the z-axis, which corresponds to the polarization at t_0 and t. Hence, a measurement at any later time t_1 is not lost information; instead, it is redundant information about the outcome of a measurement at an intermediate time t when at the initial time t_0 the particles are in a state Ψ.

The claim by Aharonov and coauthors that at various stages of the measurement process ensembles can be separated into subensembles that can be associated with quantum states leads to contradictions with the principles of quantum mechanics, and gives rise to the paradoxes of "impossible ensembles" discussed in the article. Their unphysical description of the measurement process leads them to the false conclusion that "quantum mechanics offers a place to specify both an initial *and* an independent final state," and to such outlandish statements as the idea "that quantum mechanics lets one impose . . . a putative final state of the universe."

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"A Time-Symmetric Formulation of Quantum Mechanics" by Yakir Aharonov, Sandu Popescu, and Jeff Tollaksen is riddled with errors. That is not surprising, given that their starting point is an erroneous "postselection" process. Postselection ignores the measurement postulate of standard quantum physics, according to which a system's quantum state abruptly switches, upon measurement, into an eigenstate of the measured observable.

With that postulate abandoned, it's to be expected that Aharonov and coauthors obtain results that persistently violate the standard quantum uncertainty principle. For example, in their discussion on page 28 of a spin-½ system, they incorrectly state that "the measurement of $S_{\pi/4}$ [the spin component along the diagonal between the x-axis and z-axis] is effectively a simultaneous measurement of S_x and S_z." If that were true, it would violate the uncertainty principle. But the statement is not true. A measurement of $S_{\pi/4}$ could be made by positioning a Stern–Gerlach apparatus with its inhomogeneous magnetic field in the $\pi/4$ direction, without measuring either S_z or S_x. In fact, the measurement would put the system into an eigenstate of $S_{\pi/4}$, leaving both S_z and S_x indeterminate, in agreement with the uncertainty principle.

Aharonov and coauthors continue: "The idea that they [S_z and S_x] are both well defined stems from the fact that measuring *either one* yields +½ with certainty." But that supposed violation of the uncertainty principle is wrong. Using an obvious notation, in their example the system is claimed to be in the eigenstate $|+S_z\rangle$ at time t. This eigenstate can also be written as $(|+S_x\rangle + |-S_x\rangle)/\sqrt{2}$, showing that the spin component S_x is indeterminate whenever the system is in the state $|+S_z\rangle$, in agreement with the uncertainty principle.

One sentence later, the authors state, "If we first measure S_z and then S_x, . . . then, given the pre- and postselection, both measurements yield +½ with certainty." That supposed violation of the uncertainty principle is wrong. If we first measure S_z, we'll get +½ because the system was previously prepared in that state. But if we then measure S_x,

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we'll find the result +½ with 0.5 probability and −½ with 0.5 probability, consistent with the uncertainty principle. To use the misleading "ensemble" language of Aharonov and coauthors, every member of the ensemble is in the state $|+S_z\rangle = (|+S_x\rangle + |-S_x\rangle)/\sqrt{2}$. Contrary to the authors' postselection process, it's not true that 50% of the ensemble is in the state $|+S_x\rangle$ (which would violate the uncertainty principle), and 50% is in $|-S_x\rangle$. To misinterpret quantum superpositions such as $(|+S_x\rangle + |-S_x\rangle)/\sqrt{2}$ in this manner is an elementary misconception. It also directly contradicts the experimental facts about measurements of the Stern–Gerlach type.

Contrary to the assertion of Aharonov and coauthors on page 32 that they "have not modified quantum mechanics by one iota," their postselection process would change the foundations of quantum mechanics. The fallacy in that process was pointed out by Asher Peres 16 years ago.[1]

Reference

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The article by Yakir Aharonov, Sandu Popescu, and Jeff Tollaksen is stimulating and raises some interesting and profound issues regarding the foundations of quantum mechanics and quantum measurements. One point the authors make is that a measurement of spin component $\sqrt{2}/2$ of a spin-½ particle is unphysical and can be attributed only to errors in weak measurements performed on a collection of N spins. I offer a more mundane interpretation that does not require the introduction of any error or postselection concepts. The physical, textbook picture of spin-½ is a vector of length $\sqrt{S(S+1)} = \sqrt{3}/2$ with some distribution of orientations (that is, polar coordinates θ and φ). A conventional single measurement of S_z can yield only one of the eigenvalues ±½. This may be interpreted in the classical vector model by envisioning that the spin lies on a cone with θ defined by $\cos\theta = \sqrt{3}/3$. The expectation value of S_x then vanishes due to the uniform distribution of φ. Having some control over φ can yield a finite value of S_x. Thus I would argue that only an expectation value greater than $\sqrt{3}/2$ is unphysical; a value of $\sqrt{2}/2$ is quite physical and can even be interpreted classically in terms of some distribution of φ and θ.

I would phrase the important observation of Aharonov and coauthors in a different way. While spin-½ has a magnitude of $\sqrt{3}/2$, a fundamental limitation of conventional *single-point* quantum measurements is that they can yield only the values +½ and −½ and any expectation value must therefore lie between those two extremes. Classically, therefore, the spin may not be fully aligned along the z-axis with $\theta = 0$. Such alignment is impossible since it would yield well-defined values of the three noncommuting variables $S_z = \sqrt{3}/2$ and $S_x = S_y = 0$. However, once multiple time measurements are performed, one can define a reduced distribution of some of the measurements that is conditional on the outcome in the other measurements; such a distribution can be interpreted in terms of S_z greater than ½ but less than or equal to $\sqrt{3}/2$. That interpretation does not violate any of quantum mechanics' fundamental rules, which do not usually consider such quantities.

Rather than a new, time-symmetric formulation of quantum mechanics that involves pre- and postselection, one can simply treat the scheme of the authors' figure 1b as a three-point correlation function, whereas figures 2a and 2b show a four-point correlation function, each panel having its own set of conditional probabilities. Error, time reversal, and postselection need not be invoked. Instead, one may think in multiple dimensions and develop the right language for the interpretation of the observables.

The combination of pre- and postselection is an attempt to create an artificial ensemble that reproduces the results of multiple-point measurements in a single one-point measurement. As shown by Aharonov and coauthors, that is possible by introducing errors. An alternative physical picture is obtained by retaining the multipoint analysis. Multidimensional thinking is well developed in coherent nonlinear spectroscopy, where a system of spins or optical chromophores are subjected to sequences of short impulsive pulses.[1,2] Similar ideas may be applied for the interpretation of multiple measurements. One can think of various types of n-point observables obtained by combining $n - m$ perturbations and m measurements. The nonlinear n-point response functions in spectroscopy represent $n - 1$ impulsive perturbations followed by a single measurement. The objects Aharonov and colleagues considered correspond to $n = m$. Proper multiple-distribution functions could then be naturally used for the interpretation of such generalized measurements.

continued on page 62

letters

continued from page 9

References

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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).

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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.

References

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3. Y. Aharonov, D. Rohrlich, *Quantum Paradoxes: Quantum Theory for the Perplexed*, Wiley, Weinheim, Germany (2005).

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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