letters

Consistent treatments of quantum mechanics

Letters to the editor from Michael Nauenberg and Art Hobson (PHYSICS TODAY, May 2011, page 8) both maintain that Yakir Aharonov, Sandu Popescu, and Jeff Tollaksen make elementary mistakes in quantum mechanics in their November 2010 article (page 27). The authors reply that they have made no mistakes but do not otherwise answer the questions Nauenberg and Hobson raise. The authors are correct. Nauenberg and Hobson have misunderstood them. But I believe the misunderstandings were induced by the rather peculiar language used in the article.

The experiment at issue is elementary. At an initial time t_0 an ensemble of spin-½ particles is prepared, all with spin up along the z-axis. At a later intermediate time t all spins are measured along a single direction, either z or x—we do not know which. At a still later final time t_1 the spin of each particle is measured along x and a subensemble is formed of those particles with spin up along x. All statements that follow apply to the particles in that "postselected" subensemble.

No magnetic fields act on the spins between measurements, so no spin states change except as the result of measurements. It follows that if the intermediate measurements at t had been along z, then their results would have been up, because the initial state was up along z. But if the intermediate measurements had been along x, then their results would also have been up, because if any had been down, the result of the final measurement at t_1 along x would also have been down, and such a spin would not be in the postselected subensemble. (If you still doubt that nothing is needed but plain ordinary quantum mechanics [and Bayes's theorem for conditional probabilities] to determine the probabilities of intermediate measurements on the postselected ensemble, see the homework assignment in the box at right.)

I have no doubt Nauenberg and Hobson would both have agreed with that description of the results at the intermediate time t, had they not been distracted by the strange way the authors prefer to talk about it. The language led them astray, as it led me astray the first time I encountered these entertaining games by Aharonov and his collaborators 16 years ago. I think I finally got that aspect straight in "Limits to quantum mechanics as a source of magic tricks."[1]

For example, Aharonov and coauthors say, "The results at t depend . . . on what happens later at t_1." Nauenberg says, and it sounds right, that this contradicts standard quantum mechanics. He surely would not have objected if the authors had instead said, "What we can learn about the results at t depends . . . on what we learn later at t_1." But the way they do state it suggests causality acting backwards in time, which would indeed contradict quantum orthodoxy. Similarly, they say, "If at t we measure the spin along x, we must also find it up, because otherwise the measurement at t_1 wouldn't find it up." Nauenberg says that this is incorrect, and he sounds right again. But surely he would not have objected if Aharonov and coauthors had instead said, "If the spin measurement at t were along x and not along z, then it would have given spin up because if it had given spin down, then the measurement at t_1 would also have given spin down, and that particle would not have been in the postselected ensemble." By putting everything in the historical present tense and refusing to use the subjunctive mood to indicate counterfactual statements, Aharonov and coauthors encourage such misunderstandings.

In the same vein, the authors say "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," and Hobson objects. I doubt he would have objected if the authors had said, "The idea that they are both well defined in the postselected ensemble stems from the fact that *no matter which one* was actually measured at the intermediate time, the result of that measurement would have been +½." Here again, the choice of tense and mood seem designed to mislead.

Homework assignment

At an initial time a spin-½ particle is prepared with spin up along direction *a*. At a later intermediate time the spin is measured along direction *b*. At a still later final time the spin is measured along direction *c*. Show that if the final measurement gives spin up, then the probability that the intermediate measurement gave spin up is

$$\frac{1}{1 + \tan^2(\theta_{ab}/2)\tan^2(\theta_{bc}/2)}.$$

Why has the Aharonov group been confusing some of us in this way for two decades? There is a clue in the final section of the authors' PHYSICS TODAY article, where they talk about the flow of time. There they suggest taking seriously the idea that time "propagates forward from the past boundary condition and backward from the future boundary condition." If you believe that, then conventional use of tense is indeed obsolete and distracting, and using the subjunctive mood in counterfactual statements becomes problematic.

Aharonov, Popescu, Tollaksen, and their collaborators have uncovered some intriguingly provocative features of plain old ordinary quantum mechanics. Those features suggest to them a radical new interpretation. If they want to get the prerevolutionary masses to pay due attention, they might stop addressing us only in postrevolutionary language, or at least accompany their

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Newspeak with translations into the Oldspeak that most of us still use.

Reference

1. N. D. Mermin, *Phys. Rev. Lett.* **74**, 831 (1995).

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The Letters reply by Yakir Aharonov, Sandu Popescu, and Jeff Tollaksen gives a misleading impression of the consistent histories interpretation of quantum mechanics. As I pointed out in the same issue, consistent histories is an alternative to their time-symmetric formulation. It resolves the "collapse problem" of quantum foundations, which Aharonov and coauthors admit they have failed to do. However, that is not its only or even its principal virtue. It also solves what I call the second quantum measurement problem: how to infer from the measurement outcome the state of the measured system at an earlier time before the measurement took place. Thus the consistent histories approach, without any fuzzy references to "ensembles," provides a proper probabilistic framework for inferring that S_x for a spin-½ particle actually was $-\frac{1}{2}$ before the measurement took place. This approach evades the criticisms expressed by Michael Nauenberg and Art Hobson in their letters in the same issue.

Furthermore, consistent histories, unlike certain aspects of the approach of Aharonov and coauthors, is fully consistent with the Hilbert-space structure of quantum mechanics introduced by John von Neumann and now taught to all students of the subject. Indeed, with a consistent histories analysis one can locate fairly precisely[1] the error in reasoning that leads to claims such as that the measured spin of a spin-½ particle can equal 100.[2] Claims about what weak measurements actually measure, when made by those who have not yet solved either the collapse problem or the second measurement problem, should be treated with great caution.

The claim that the consistent histories approach is controversial was true back in the 1990s, and the controversy led to a clearer formulation of the interpretation. However, no significant problems have come to light in the decade since the publication of my book.[3] The fact that something has been ignored does not imply it is controversial. Admirers of Richard Feynman, some of whose writings were cited by Aharonov and coauthors in their reply, may be interested to know that shortly before his death he was quite sympathetic to an early form of the consistent histories or decoherent histories approach to understanding quantum mechanics (see PHYSICS TODAY, February 1999, page 11)—an open-mindedness worth imitating.

References

1. R. E. Kastner, *Stud. Hist. Phil. Mod. Phys.* **35**, 57 (2004), http://arxiv.org/abs/quant-ph/0207182.
2. Y. Aharonov, D. Z. Albert, L. Vaidman, *Phys. Rev. Lett.* **60**, 1351 (1988).
3. R. B. Griffiths, *Consistent Quantum Theory*, Cambridge U. Press, New York (2002), http://quantum.phys.cmu.edu/CQT/.

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Aharonov, Popescu, and Tollaksen reply: We are grateful to David Mermin for stating loud and clear, in his unmistakable style, that "the authors are correct." As for the rest of his letter, a lesson on clear writing from a great master of the subject is always welcome. We did strive for clarity in our article, but there is always space for improvement, and Freudian slips of Newspeak often creep in—that's their very nature.