

able 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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Journal's History and Peer Review Process Were Misrepresented

I am surprised to find myself misquoted—and to see certain other errors—in Paul Moran's response to two letters to the editor (December 1997, page 102) commenting on a book review he had written for your magazine. The following are five reasons for my surprise.

First, although Moran attributes to me two quotes about Raymond Damadian and alleges that they come from a casual conversation he and I had back in the early 1970s, I do not remember any such conversation taking place.

Second, Moran quotes me as referring to Damadian as "Ray," but that is simply not something I would do, because I know that nickname to be offensive to Damadian. Thus, I question that Moran's conversation was with me.

Third, I don't recall that, as Moran alleges, Damadian published primarily in *Physiological Chemistry and Physics* (the journal's name in the 1970s; Moran got that wrong too). Rather, I remember his publishing in such journals as *Science*, the *Biophysical Journal* and the *Proceedings of the National Academy of Sciences*, as well as in what I'll call *PC&P* for short. I believe you will find that Damadian's first publication in *PC&P* did not occur until 1975.

Fourth, contrary to Moran's assertions, all manuscripts submitted to *PC&P* were reviewed using orthodox reviewing procedures.

Fifth, although Moran claims that I told him in the early 1970s that Damadian had already acquired the rights to publish *PC&P*, Damadian's acquisition did not occur until later in that decade. Furthermore, although the journal's name was changed at that time ("and *Medical NMR*" was added), no change was made in the journal's editorial policy.

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The existence of the English word "peerless" points out that approval by peers as the sole condition for acceptance for publication of a scientific manuscript is at best a risky compromise. On the one hand, the peer review system may provide an editor with an easy way to turn down truly undeserving writings. On the other hand, it may result in the throwing out of the very best on

which to base major scientific progress of the future. This risk is especially serious for the science of cell physiology, a field that is still in its infancy and in which revolutionary upheavals are ongoing.

Recognizing all this, the editors of *Physiological Chemistry and Physics* and *Medical NMR* (formerly *Physiological Chemistry and Physics*) have long held to an official policy based on the belief that scientific issues should be settled by investigations and open debate, not by appeals to anonymous judges. To achieve this goal, the editors have established over time a set procedure for evaluating submissions to the journal. It includes giving the author of a rejected article the right to (1) rebut the reasons given by the reviewers for rejection, (2) recommend to us a list of alternative competent reviewers and (3) in the case of ultimate rejection, have us publish a brief priority note describing the article's key points and its date of receipt by the journal. The initial step in this procedure, however, remains the obligatory use of the orthodox peer review system. The full procedure is described on the front pages of each issue.

It was thus with astonishment and dismay that we discovered that *PHYSICS TODAY* has been made into a tool to publicize a vilifying statement to the effect that our journal does not use the orthodox review system. The statement appears in Paul Moran's reply to a couple of letters to the editor. In making such a spurious statement, Moran—who evidently knows so little about our journal that he cannot even get the name right, let alone our evaluation procedure—defames not only those of us who have run the publication (I am the current editor-in-chief) but also all the scientists who have published their work in our pages over the last three decades.

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Correction

December, page 54—The setting of the fictional dinner at the University of Cambridge presented in *The Cambridge Quintet: A Scientific Speculation* was misidentified in the review of the book; its correct name is Christ's College. ■