On Bohmian Mechanics

Sheldon Goldstein & Steven Weinberg

Physics > Quantum Mechanics / Correspondence

Dissatisfaction with the Copenhagen interpretation of quantum mechanics is, if uncertain, also widespread. In this exchange of letters, a persuasive exponent of David Bohm's views addresses himself to one of the founders of the Standard Model of particle physics. The exchange is, perhaps, less remarkable for what it says than for the fact that it took place at all.

The Editors

From: oldstein@fermat.rutgers.edu
To: WEINBERG@utaphy.ph.utexas.edu
Subject: NYRB
Date: Sun, Sep 22 1996, 17:14:44

Dear Professor Weinberg,

In your recent response in the *NYRB*, you ask George Levine, my colleague here at Rutgers, to "suppose that physicists were to announce the discovery that, beneath the apparently quantum mechanical appearance of atoms, there lies a more fundamental substructure of fields and particles that behave according to the rules of plain old classical mechanics." I agree with your point that this should make little difference to our views about culture or philosophy. More interesting, however, is the possibility that it would make little difference to the views of most physicists about physics!

In fact, a theory rather roughly corresponding to the substructure that you mention, Bohmian mechanics, that describes a deterministic motion of point particles that is governed by the wave function and from which emerges the entire quantum formalism, including an appearance of randomness and operators as observables (for nonrelativistic quantum mechanics), has been around for more than forty years, during which time it has been either ignored or misrepresented by the physics establishment. (The great exception to this behavior is that of John Bell, who was the principal proponent of Bohmian mechanics for most of this period.) This theory completely avoids all the quantum paradoxes, all the mysticism of Bohr and Heisenberg, and replaces it with sharp mathematics.

Here is a thought experiment you might try: ask various physicists that you respect what they think of this theory, and compare their responses with the objective facts of the situation—which you should have no difficulty ascertaining. I think you'll be astonished at what you find. (I always am.)

Best,
Shelly Goldstein

From: WEINBERG@physics.utexas.edu
To: oldstein@fermat.rutgers.edu
Subject: Bohm's quantum mechanics
Date: Tue, Sep 24 1996, 21:01:39

Dear Professor Oldstein,

I have carried out the experiment you requested. At the regular weekly luncheon meeting today of our Theory Group, I asked my colleagues what they think of Bohm's version of quantum mechanics. The answers were pretty uniform, and much what I would have said myself.

First, as we understand it, Bohm's quantum mechanics uses the same formalism as ordinary quantum mechanics, including a wave function that satisfies the Schrödinger equation, but adds an extra element, the particle trajectory. The predictions of the theory are the same as for ordinary quantum mechanics, so there seems little point in the extra complication, except to satisfy some a priori ideas about what a physical theory should be like.

There is also the point that it does not seem possible to extend Bohm's version of quantum mechanics to theories in which particles can be created and destroyed, which include all known relativistic quantum theories.

It is not true that the only alternative to Bohm's version of quantum mechanics is the Copenhagen interpretation, for which I share your reservations. These days most physicists who think about it at all understand quantum mechanics in terms of the Everett many-histories approach. In any case, the basic reason for not paying attention to the Bohm approach is not some sort of ideological rigidity, but much simpler—it is just that we are all too busy with our own work to spend time on something that doesn't seem likely to help us make progress with our real problems.

Now I would like to ask you a question. You are not the only person who has recently raised the issue of Bohm's version of quantum mechanics. Why does this have such appeal for some philosophers and sociologists of science?

Sincerely yours,
Steven Weinberg

From: oldstein@fermat.rutgers.edu
To: WEINBERG@physics.utexas.edu
Subject: Re: Bohm's quantum mechanics
Date: Thu, Oct 3 1996, 11:22:04

Dear Professor Weinberg,

Thank you for your reply. Please forgive my delay in responding, as well as the length of this response. The fact is that there is no living physicist whose writings on the nature of physics and the scientific enterprise I value as much as yours.

Although my username is oldstein, my name is actually Goldstein—the one cited in footnote 13 of the article of Jean Bricmont on Prigogine to which you have referred. I have been working on Bohmian mechanics for more than ten years. I am a physicist.

I must admit that your group there has done you proud and proven me wrong! A few years ago they almost surely would have said that Bohm's theory, like any hidden variables theory, has been demonstrated by Bell's theorem and Aspect's experiment to be impossible, just as a generation ago this was declared so on the basis of von Neumann's theorem. What you report represents a vast improvement: Bohm's theory, while possible after all, is merely irrelevant, in fact because it works so well that it reproduces the standard predictions (for nonrelativistic quantum mechanics).

I shall return later to your statement that

> [t]he predictions of the theory are the same as for ordinary quantum mechanics, so there seems little point in the extra complication,

> except to satisfy some a priori ideas about what a physical theory should be like.

For now, and apropos of the kinds of strange statements I have come to expect but which your group did not supply, I note that there is a superficially similar, but wonderfully idiotic, statement by Timothy Ferris in this past Sunday's (Sept. 29) *NY Times Magazine*, in a article devoted to the problem of quantum weirdness. We find there the following:

> And since Bohm's equations make exactly the same predictions as those of ordinary quantum mechanics, it is not clear what is accomplished by adding the complication of guiding waves, except to restore a sense of sanity to the whole affair.

I am not mentioning this because of the fact that Ferris couldn't get straight exactly what it is that Bohmian mechanics in fact adds (though it is of course very important that it adds the actual configuration and not the guiding wave, which is after all just the wave function, as you well know). People can easily make factual errors, even whoppers. But the last quoted phrase ("except to restore a sense of sanity to the whole affair") is I think extraordinary, particularly in an article on the *problem* of quantum weirdness.

Now you refer instead to "some a priori ideas about what a physical theory should be like," and a great deal hinges on which a priori ideas you have in mind. Certainly we both agree with the a priori idea that a physical theory should be sane. And as a matter of fact I myself do not accept the idea that a physical theory must be deterministic—and my support for Bohmian mechanics in no way rests on a desire to restore determinism to physics.

For me, the main a priori idea is a demand for coherence: physical theories should be clearly formulated, in sharp mathematical terms. In particular, it should be clear what the theory is about. Secondarily—and one might say as a consequence of the requirement of coherence—the theory should not involve any subjective notions in its very formulation, nor should it involve axioms concerned with measurement, since the notion of measurement is much too vague. (Theorems having implications for measurements are of course fine—and presumably necessary if the consequences of the theory are to be explored). Finally, the theory should in some sense be simple, since otherwise it does not provide us with much of an explanation of what we wish to understand.

What I like about Bohmian mechanics is that it is by far the simplest formulation of nonrelativistic quantum mechanics satisfying the requirements of coherence and objectivity (in the above sense). Two equations, Schrödinger's and the de Broglie-Bohm guiding equation, completely express the theory. From these simple equations the rest of the quantum formalism flows. Thus to the extent that "Bohm's quantum mechanics uses the same formalism as ordinary quantum mechanics, including a wave function that satisfies the Schrödinger equation," it is only the Schrödinger equation part of that formalism that (along with the guiding equation) is fundamental in Bohmian mechanics, with the rest of the formalism arising as a consequence.

Now if Schrödinger's equation alone were adequate as an expression of a physical theory I would completely agree with you. But from Schrödinger's equation alone no physical consequences whatsoever flow, Sidney Coleman to the contrary notwithstanding. No textbook of which I am aware on the subject omits the measurement postulates, and if one indeed takes these as independent axioms the increase in complexity is far greater than we would have with the mere addition of the guiding equation. (It is true that in his text Gottfried tries to derive measurement theory from something more basic, but he merely repeats the familiar mistakes traditionally made in connection with the measurement problem.)

Now all this has assumed the adequacy of the Copenhagen interpretation, so that we could compare two adequate formulations—Bohmian mechanics and the Copenhagen interpretation—with respect to simplicity and other criteria for judging theories. But you seem to agree with me that the Copenhagen interpretation is not adequate. You should therefore appreciate why others who agree with us on this, and who are not aware of any other adequate alternatives to the Copenhagen interpretation, might be attracted to Bohmian mechanics: they want to make sense of quantum mechanics, something that the Copenhagen interpretation manifestly does not do and that Bohmian mechanics manifestly does.

Now I in fact assume that when you said that "there seems little point in the extra complication" you had in mind what you consider to be a sensible alternative—the Everett many-histories approach—which you believe involves no complications such as the particle trajectory in Bohmian mechanics. I'm not entirely sure what you have in mind by the Everett many-histories approach, but my guess is that you mean the decoherent histories approach of Gell-Mann and Hartle, since it is my impression that it is this approach that is currently most popular among "physicists who think about it at all."

But whatever you have in mind, whether it be decoherent histories, or the original Everett many-worlds (whatever that is—see J.S. Bell, *Speakable and Unspeakable*,[1] where incidentally you will also find Bell saying that "It is always interesting to find that solipsists and positivists, when they have children, have life insurance"),[2] or whatever, you did say a few years ago about your favorite approach that "I am not convinced that this program has been entirely successful yet, but I think in the end it may be." Since, insofar as the conceptual problems of quantum mechanics are concerned, Bohmian mechanics as it presently exists is entirely successful, I can't understand why, if you are aware of this fact, you should be at all surprised by its appeal. This is particularly puzzling because you go on to say that "we really do need to understand quantum mechanics better in quantum cosmology … where no outside observer is even imaginable." After all, from a Bohmian perspective, this presents no conceptual problem whatsoever, so that at the very least Bohmian mechanics might give us important clues for quantum cosmology.

As a matter of fact, I have a certain amount of sympathy for the decoherent histories approach, in the sense that I believe that it is just possible that something sensible could come of it. (In fact, I find that I get into a good deal of trouble with people working in foundations of quantum mechanics for making such a modest claim on behalf of decoherent histories, since most people working in foundations seem to believe that the approach is of no value whatsoever.)

But one point seems clear: insofar as "the extra complication" of "the particle trajectory" is concerned, Bohmian mechanics and decoherent histories are pretty much on the same footing. They both have trajectories (or histories—in one case deterministic, and in one case stochastic)—and an essential innovation of the decoherent histories approach is that it provides us with a probability formula for the histories (analogous to the guiding equation for trajectories), as a fundamental element of the formulation of the theory and not as any sort of consequence of Schrödinger's equation or of standard quantum mechanics (whatever that means). This probability formula is just the Wigner formula for the probability distribution for the results of a sequence of ideal measurements but it is regarded in the histories approach as referring to histories of objective events, corresponding to such-and-such observables having such-and-such values at such-and-such times, regardless of whether or not any measurement is involved. In other words the histories approach describes the probabilities of observables *having* such-and-such values and not merely *finding* them with those values.

The basic difference between decoherent histories and Bohmian mechanics is a simple one: Bohmian mechanics exists, trivially, while decoherent histories remains merely a research program. The point is that in trying to assign probabilities to histories using the Wigner formula (which is more or less just the diagonal of the decoherence functional of Gell-Mann and Hartle) one runs into very serious problems of consistency, problems that the various decoherence conditions that are formulated in terms of the decoherence functional don't entirely resolve. Many complications ensue. Gell-Mann and Hartle have proposed various conditions which conceivably could overcome all the difficulties and provide us with a well-defined theory that could then be judged for simplicity and what have you. But I believe that as of yet no one has proposed conditions which have been shown to resolve all difficulties.

So when all is said and done, Bohmian mechanics seems incredibly simpler than the decoherent histories program, a fact that is at present causing me severe difficulties. This is because I am writing a review article, for a broad audience, on three sensible formulations of quantum mechanics, specifically decoherent histories, spontaneous localization, and Bohmian mechanics, and there are some length restrictions that I must adhere to. Now while Bohmian mechanics can be completely described quite briefly, and spontaneous localization with a bit more space, it seems to me that to even begin to convey adequately what is involved in the decoherent histories approach requires considerable length. (I haven't yet figured out how to manage.)

Unfortunately, Bohmian mechanics is a nonrelativistic theory, and so it is of value primarily for the lessons it conveys about finding a sensible interpretation of quantum mechanics that is relativistic, rather than for the specific details of the theory itself. Now the question you raised about pair creation is, of course, very important. Bohmian mechanics itself is not a theory with particle creation or annihilation. However, I see no reason why some Bohm-type theory should not permit these things.

Here are two ways they could arise: What might be called the Bohm motion for the Klein-Gordon equation permits trajectories that can reverse time direction, which can naturally be regarded as describing pair annihilation or creation depending on the sense of the reversal. Also, if the basic variables were field configurations, then a Bohm-type evolution for such configurations that permits the number of particle-like elements (solitons or what have you) of the field configuration to change during the course of the evolution might well exist.

However, I don't have any compelling candidates to put forward as relativistic Bohmian theories. I think any such formulations should integrate the structure of modern gauge theories into their formulation in a natural and illuminating way. This is something that I am too ignorant to do, and its probably not too easy in any case—certainly nothing so trivial as Bohmian mechanics. (I wish there were something so trivial for relativistic physics.)

I think the virtues of Bohmian mechanics that I've described easily account for its appeal for philosophers of science. As far as sociologists of science are concerned, I wasn't aware that it had much appeal for them. (I know that Norton Wise refers to it, but that was because he knew about it—from a philosophy of quantum mechanics seminar at Princeton that he occasionally attended, if from nothing else—and because he could use it to criticize you. In general there is nothing in Bohmian mechanics that should be congenial to the postmodern sensibility. Quite the contrary, since Bohmian mechanics trivially restores to quantum mechanics the rationality that postmodernists were so happy to think had been precluded by the quantum revolution, a rationality to which the advocates of the Copenhagen interpretation seem none too sympathetic.

Incidentally, with regard to the matter of scientific realism, the American and British philosophers of science are pretty much on your side, more so I would imagine than the typical physicist. In fact, positivism was abandoned by American philosophers several decades ago, with physicists lagging one or two decades behind in this regard. It is hard to find American philosophers nowadays who will defend positivism, but it is not at all that difficult to find physicists mouthing positivistic slogans, particularly when in deep quantum mode.

Sincerely yours,
Shelly Goldstein

From: WEINBERG@physics.utexas.edu
To: oldstein@fermat.rutgers.edu
Subject: Bohm, etc.
Date: Thu, Oct 3 1996, 22:08:35

Dear Professor Goldstein,

A few days after I sent my previous e-mail message to you, I realized that

“Oldstein” is the physicist Shelly Goldstein. I was going to write you to apologize, so I was especially glad to see from your message that I was already forgiven.

You are pretty persuasive about Bohmism. Is there a succinct (emphasis on succinct) clear statement of Bohm’s theory that I can read?

Best,
Steven Weinberg

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1. J.S. Bell, *Speakable and Unspeakable in Quantum Mechanics* (Cambridge: Cambridge University Press, 1988), 133–37.
2. J.S. Bell, *Speakable and Unspeakable in Quantum Mechanics* (Cambridge: Cambridge University Press, 1988), 136.