The Quantum Revolution: An Orthodox Perspective

When I was young I was fascinated by the quantum revolution: the transition from classical definiteness and determinism to quantum indeterminacy and uncertainty, from classical laws that are indifferent, if not hostile, to the human presence, to quantum laws that fundamentally depend upon an observer for their very meaning. I was intrigued by the radical subjectivity, as expressed by Heisenberg’s assertion [3] that “The idea of an objective real world whose smallest parts exist objectively in the same sense as stones or trees exist, independently of whether or not we observe them … is impossible …” It is true that I did not really understand what the quantum side of this transition in fact entailed, but that very fact made quantum mechanics seem to me all the more exciting. I was eager to learn precisely what the alluring quantum mysteries might mean, what kind of world they describe, as well as exactly what evidence could compel—or at least support—such radical conclusions.

The subjectivity and indeterminism of quantum theory were accompanied by the view that quantum particles don’t really have trajectories and that, by Heisenberg’s uncertainty principle, any talk of such things is meaningless. While the conclusion of meaninglessness seemed a bit forced, I was reassured by one of the best quantum mechanics textbooks, that of Landau and Lifshitz, which declared flatly in referring to the double-slit experiment that [4] “It is clear that this result can in no way be reconciled with the idea that electrons move in paths. … In quantum mechanics there is no such concept as the path of a particle.”

I was, however, given pause by the fact that strong disapproval of the accepted understanding of the quantum revolution was expressed by a few physicists, including at least two of the founders of quantum theory: Schrödinger, the creator of wave mechanics, who maintained that “Bohr’s … approach to atomic problems … is really remarkable. He is completely convinced that any understanding in the usual sense of the word is impossible” (letter to Wien, quoted in [3]); and Einstein [6], who
believed that “the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this (theory) operates with an incomplete description of physical systems.”

But I was instructed that Einstein’s belief was out of the question, since John von Neumann had proven, with the utmost mathematical rigor, that a return by physics to any sort of fundamental determinism was impossible. Von Neumann himself [7] concluded that “It is therefore not, as is often assumed, a question of a re-interpretation of quantum mechanics—the present system of quantum mechanics would have to be objectively false, in order that another description of the elementary processes than the statistical one be possible.” According to Max Born, the physicist who formulated the now standard statistical interpretation of the wave function, von Neumann had shown that [5] “no concealed parameters can be introduced with the help of which the indeterministic description could be transformed into a deterministic one. Hence if a future theory should be deterministic, it cannot be a modification of the present one but must be essentially different. How this could be possible without sacrificing a whole treasure of well established results I leave to the determinists to worry about.”

Nonetheless, I did hear rumors now and then about a certain hidden variables theory constructed by David Bohm in 1952 [9], a theory that was reputed somehow to recast quantum theory into a completely deterministic form. I was not at all clear about what exactly Bohm had proposed, but I was assured by my teachers that whatever it was, it could not possibly accomplish what Bohm had claimed.

Occasionally I would hear that there might be some flaw in the proof of von Neumann, but I was advised not to worry about this because there were other, even more convincing, proofs of von Neumann’s result! What an embarrassment of riches! For example, according to Eugene Wigner [10, page 291] “the proof he [von Neumann] published …, though it was made much more convincing later on by Kochen and Specker, still uses assumptions which, in my opinion, can quite reasonably be questioned. … In my opinion, the most convincing argument against the theory of hidden variables was presented by J. S. Bell (1964).” Elsewhere [11] Wigner reports, concerning Bohm’s theory, that “This is an interesting idea and even though few of us were ready to accept it, it must be admitted that the truly telling argument against it was produced as late as 1965, by J. S. Bell. … This appears to give a convincing argument against the hidden variables theory.”

Were I young today—and wished to be reassured about the health of the quantum revolution—I could turn for support to Murray Gell-Mann, who reports [12] that “for more than forty years, David [Bohm] tried to reformulate and reinterpret quantum mechanics so as to overcome his doubts,” adding that “some theoretical work of John Bell revealed that the EPRB experimental setup could be used to distinguish quantum mechanics from hypothetical hidden variable theories … After the publication of Bell’s work, various teams of experimental physicists carried out the EPRB experiment. The result was eagerly awaited, although virtually all physicists were betting on the correctness of quantum mechanics, which was, in fact,
vindicated by the outcome.”

In view of what I’ve so far said, it would be natural to suppose that the two books with which I shall be concerned in this essay, *The Undivided Universe* by Bohm and Basil J. Hiley, which is a presentation of Bohm’s 1952 theory incorporating some later developments, many of which represent joint work with Bohm’s long-time collaborator Hiley, and Bell’s *Speakable and Unspeakable in Quantum Mechanics*, which collects all of Bell’s work, up to 1987, on the foundations of quantum mechanics, are in diametric opposition, with the former defending hidden variables and the latter defending quantum orthodoxy. This, however, is not so; in fact, Bell provides a far stronger argument against the orthodox Copenhagen interpretation of quantum mechanics and, indeed, in favor of what have been called (absurdly, according to Bell) hidden variables theories than do Bohm and Hiley!

It might thus seem, and it has in fact been suggested [13], that there must have been two Bells—that Bell must have been schizophrenic. However, there was, unfortunately for us, but one Bell, and he was one of the sanest and most rational of men. Bell did not—as did in fact Bohm—at some point convert from a defender to a critic of quantum orthodoxy; he was always unhappy with traditional quantum mechanics, and reports [4] that even as a student learning quantum theory “I hesitated to think it might be wrong, but I knew that it was rotten.”

But what, you might well ask, of the statements of Wigner and Gell-Mann, in effect praising Bell for destroying Bohm? The best that can be said for these statements, by two of the leading physicists of their respective generations, is that they are grossly misleading, since Bell’s analysis is entirely compatible with Bohm’s theory and in fact affords this theory dramatically compelling support!

### Bohmian Mechanics

What Bohm did in 1952—the “proof” of von Neumann and the claims of Born, Bohr, Landau and Lifshitz, and Heisenberg notwithstanding—was to provide an objective and completely deterministic account of nonrelativistic quantum phenomena through a refinement and completion of de Broglie’s 1926 pilot-wave model. In Bohm’s theory, which he sometimes called the causal interpretation of quantum mechanics and sometimes the theory of the quantum potential but which is nowadays more frequently called Bohmian mechanics, a system of particles is described in part by its wave function, evolving, as usual, according to Schrödinger’s equation; however, the wave function provides only a partial description of the system, which is completed by the specification of the actual positions of the particles. The latter evolve according to Bohm’s “guiding equation,” which expresses the velocities of the particles in terms of the wave function. Thus, in Bohmian mechanics the configuration of a system of particles evolves via a deterministic motion choreographed by the wave function. In particular, when a particle is sent into a double-slit apparatus, the slit through which it passes and where it arrives on the photographic plate are completely determined by its initial position and wave function.
Bohm’s achievement was greeted by most physicists with indifference, if not with outright hostility. Bell’s reaction, however, was a refreshing exception:

In 1952 I saw the impossible done. It was in papers by David Bohm. Bohm showed explicitly how parameters could indeed be introduced, into nonrelativistic wave mechanics, with the help of which the indeterministic description could be transformed into a deterministic one. More importantly, in my opinion, the subjectivity of the orthodox version, the necessary reference to the ‘observer,’ could be eliminated. . . . But why then had Born not told me of this ‘pilot wave’? If only to point out what was wrong with it? Why did von Neumann not consider it? . . . Why is the pilot wave picture ignored in text books? Should it not be taught, not as the only way, but as an antidote to the prevailing complacency? To show us that vagueness, subjectivity, and indeterminism, are not forced on us by experimental facts, but by deliberate theoretical choice?” (Bell, page 160)

and

Bohm’s 1952 papers on quantum mechanics were for me a revelation. The elimination of indeterminism was very striking. But more important, it seemed to me, was the elimination of any need for a vague division of the world into “system” on the one hand, and “apparatus” or “observer” on the other. I have always felt since that people who have not grasped the ideas of those papers . . . and unfortunately they remain the majority . . . are handicapped in any discussion of the meaning of quantum mechanics. (Bell, page 173)

In view of what has so often been said—by most of the leading physicists of this century and in the strongest possible terms—about the radical implications of quantum theory, it is not easy to accept that Bohmian mechanics really works. However, in fact, it does: Bohmian mechanics accounts for all of the phenomena governed by nonrelativistic quantum mechanics, from spectral lines and quantum interference experiments to scattering theory and superconductivity. In particular, the usual measurement postulates of quantum theory, including collapse of the wave function and probabilities given by the absolute square of probability amplitudes, emerge as a consequence merely of the two equations of motion for Bohmian mechanics—Schrödinger’s equation and the guiding equation—without the traditional invocation of a special and somewhat obscure status for observation.

Both of these books provide explanations of how this comes about. You will not find in these books the evasions—by now almost standard fare for the foundations of quantum mechanics—in which the real problems are skirted rather than solved. The account of Bohm and Hiley is quite detailed; Bell’s is rather brief. The presentation of Bohm and Hiley is well written and insightfully describes a broad
spectrum of important physics. Bell’s is almost always precisely on target and almost never less than thoroughly masterful. Whereas Bohm and Hiley provide an explanation, it is Bell that supplies the explanation. Bell’s presentation affords by far the deeper appreciation of the essence of Bohmian mechanics, of its virtues and of its limitations.

Nonlocality

One of the most remarkable implications of quantum theory—a genuine implication, unlike those which intrigued me when I was younger and which reflect the prejudices of their proponents far more than they do anything about the demands of nature—is that of quantum nonlocality. It is to this that the main title of the book of Bohm and Hiley refers. No physicists have contributed as much to our understanding of quantum nonlocality as have Bell and Bohm. This subject plays a central role in every article in Bell’s book, and Bohm and Hiley also give it considerable attention. Both treatments of this subject are excellent, with Bell’s nearly perfect.

A great many physicists and philosophers have regarded nonlocality as scientifically and philosophically unacceptable. In this regard Bohm and Hiley make an observation with which it would be difficult to disagree:

For several centuries, there has been a strong feeling that nonlocal theories are not acceptable in physics. It is well known, for example, that Newton felt very uneasy about action-at-a-distance and that Einstein regarded it as ‘spooky’. One can understand this feeling, but if one reflects deeply and seriously on this subject one can see nothing basically irrational about such an idea. Rather it seems to be most reasonable to keep an open mind on the subject and therefore to allow oneself to explore this possibility. If the price of avoiding nonlocality is to make an intuitive explanation impossible, one has to ask whether the cost is not too great. (BH, page 57)

However, in linking nonlocality with the desire for an “intuitive explanation,” this observation of Bohm and Hiley is somewhat inadequate. The extent of this inadequacy can best be appreciated by way of comparison with Bell’s treatment of nonlocality.

First of all, Bell observes that quantum nonlocality is implicit in the fundamental mathematical structure of quantum mechanics, as provided by a wave function on the configuration space of a many-particle system. Thus, since the very same wave function is also central to Bohmian mechanics, we should not be surprised to find, as indeed we do, that it, too, is nonlocal. Bohmian mechanics, however, is explicitly nonlocal while the nonlocality of orthodox quantum theory is ambiguous and merely implicit—or so it was until 1964, when Bell showed that the very predictions of standard quantum theory provide unmistakable, though indirect, evidence for this nonlocality. As Bell has emphasized:
That the guiding wave, in the general case, propagates not in ordinary three-space but in a multidimensional-configuration space is the origin of the notorious ‘nonlocality’ of quantum mechanics. It is a merit of the de Broglie-Bohm version to bring this out so explicitly that it cannot be ignored. (Bell, page 115)

In the last quoted sentence, Bell is referring to his own analysis of Bohmian mechanics, some fifteen years earlier, in the course of which he observed that

... in this theory an explicit causal mechanism exists whereby the disposition of one piece of apparatus affects the results obtained with a distant piece.

Bohm of course was well aware of these features of his scheme, and has given them much attention. However, it must be stressed that, to the present writer’s knowledge, there is no proof that any hidden variable account of quantum mechanics must have this extraordinary character. It would therefore be interesting, perhaps, to pursue some further “impossibility proofs,” replacing the arbitrary axioms objected to above by some condition of locality, or of separability of distant systems. (Bell, page 11)

In a footnote, Bell added that “Since the completion of this paper such a proof has been found.” He is referring, of course, to his celebrated paper \[15\] “On the Einstein-Podolsky-Rosen Paradox,” in which he derives Bell’s inequality—on the basis of which he concludes that the predictions of quantum theory are irreducibly nonlocal.

Thus Bell’s refutation, referred to by Wigner and Gell-Mann, of the theory of hidden variables is directed only against local hidden variables and does not touch Bohmian mechanics, which is nonlocal. It is true that if the nonlocality of Bohmian mechanics had not already been noticed, Bell’s theorem could have been used to draw the conclusion that Bohmian mechanics must be nonlocal, and thus, for some, unacceptable. But Bohmian mechanics is explicitly nonlocal and is not rendered any more nonlocal—or any less acceptable—because of Bell’s theorem. On the contrary, as the passage from which I have just quoted demonstrates, Bell’s recognition of this feature of Bohmian mechanics is the origin of Bell’s inequality, which shows that there exists no hidden variables theory that improves upon Bohmian mechanics by avoiding its nonlocality.

Moreover, as I have already indicated, Bell’s analysis shows much more. It shows not only that any hidden variables account of quantum phenomena must be nonlocal, but that nonlocality is implied merely by the observational consequences of standard quantum theory itself, so that if nature is governed by these predictions, then nature is nonlocal! That nature is so governed, even in the crucial EPR-correlation experiments, has by now been established by a great many experiments, the most conclusive of which is perhaps that of Aspect \[16\]. Thus the statement in
which Bohm and Hiley describe what is to be concluded from the results of Aspect’s experiment, namely that “we have an experimental proof that if there are hidden variables they must be nonlocal” (BH, page 144), does not go far enough.

It must be admitted that on this rather crucial point even Bell’s writing was not always quite as clear as it needed to be. It would therefore be worthwhile to spend some time tracing the evolution of, not so much his ideas as, his mode of expression on this matter. In the Bell’s inequality paper itself (1964) we find Bell saying that

It is the requirement of locality, or more precisely that the result of a measurement on one system be unaffected by operations on a distant system with which it has interacted in the past, that creates the essential difficulty. There have been attempts to show that even without such a separability or locality requirement no ‘hidden variable’ interpretation of quantum mechanics is possible. These attempts have been examined elsewhere and found wanting. Moreover, a hidden variable interpretation of elementary quantum theory has been explicitly constructed. That particular interpretation has indeed a grossly non-local structure. This is characteristic, according to the result to be proved here, of any such theory which produces exactly the quantum mechanical predictions. (Bell, page 14)

However, Bell’s argument in this paper demonstrates the stronger conclusion. In fact, this is clear from his very next paragraph, in which Bell summarizes the argument of Einstein, Podolsky, and Rosen to the effect that the assumption of locality implies the existence of certain elements of reality or, what amounts to more or less the same thing, local hidden variables or what Bell calls here “predetermined” results:

Consider a pair of spin one-half particles formed somehow in the singlet state and moving freely in opposite directions. Measurements can be made, say by Stern-Gerlach magnets, on selected components of the spins $\sigma_1$ and $\sigma_2$. If measurement of the component $\sigma_1 \cdot a$, where $a$ is some unit vector, yields the value +1 then, according to quantum mechanics, measurement of $\sigma_2 \cdot a$ must yield the value $-1$ and vice versa. Now we make the hypothesis, and it seems one at least worth considering, that if the two measurements are made at places remote from one another the orientation of one magnet does not influence the result obtained with the other. Since we can predict in advance the result of measuring any chosen component of $\sigma_2$, by previously measuring the same component of $\sigma_1$, it follows that the result of any such measurement must actually be predetermined. (Bell, page 15)

Bell then proceeds to show that these EPR elements of reality must satisfy a certain (Bell’s) inequality incompatible with the predictions of quantum theory.

In 1981 Bell is more explicit
Could it be that this strange non-locality is a peculiarity of the very particular de Broglie-Bohm construction . . . and could be removed by a more clever construction? I think not. It now seems that the non-locality is deeply rooted in quantum mechanics itself and will persist in any completion. (Bell, page 132)

and, indeed, quite emphatic

It is important to note that to the limited degree to which determinism plays a role in the EPR argument, it is not assumed but inferred. What is held sacred is the principle of ‘local causality’—or ‘no action at a distance’ . . .

It is remarkably difficult to get this point across, that determinism is not a presupposition of the analysis. (Bell, page 143)

Let me summarize once again the logic that leads to the impasse. The EPRB correlations are such that the result of the experiment on one side immediately foretells that on the other, whenever the analyzers happen to be parallel. If we do not accept the intervention on one side as a causal influence on the other, we seem obliged to admit that the results on both sides are determined in advance anyway, independently of the intervention on the other side, by signals from the source and by the local magnet setting. But this has implications for non-parallel settings which conflict with those of quantum mechanics. So we cannot dismiss intervention on one side as a causal influence on the other. (Bell, page 149)

. . . Despite my insistence that the determinism was inferred rather than assumed, you might still suspect somehow that it is a preoccupation with determinism that creates the problem. Note well then that the following argument makes no mention whatever of determinism. . . . Finally you might suspect that the very notion of particle, and particle orbit . . . has somehow led us astray. . . . So the following argument will not mention particles . . . nor any other picture of what goes on at the microscopic level. Nor will it involve any use of the words ‘quantum mechanical system’, which can have an unfortunate effect on the discussion. The difficulty is not created by any such picture or any such terminology. It is created by the predictions about the correlations in the visible outputs of certain conceivable experimental set-ups. (Bell, page 150)

Later still, Bell identifies the characteristic feature of any formulation of quantum mechanics with manifest nonlocality as merely clarity and precision. Bell 1984:

The de Broglie-Bohm picture disposes of the necessity to divide the world somehow into system and apparatus. But another problem is
brought into focus. This picture, and indeed, I think, any sharp formulation of quantum mechanics, has a very surprising feature: the consequences of events at one place propagate to other places faster than light. (Bell, page 171)

And 1986:

The very clarity of this picture puts in evidence the extraordinary ‘non-locality’ of quantum theory. (Bell, page 194)

Quantum Observables and the Impossibility of Hidden Variables

So much for what Wigner regarded as “the truly telling argument against” the possibility of hidden variables. What about the other “proofs” of the impossibility of hidden variables—for example, those of von Neumann [7], of Gleason [17], of Kochen and Specker [18], and of Jauch and Piron [19]—having little if anything to do with nonlocality? It should not be necessary to mention that the existence of Bohmian mechanics conclusively demonstrates that none of these could possibly prove any such thing. It is, however, mildly astonishing that Bohmian mechanics preceded all of these “proofs,” except for von Neumann’s, by at least six years!

Be that as it may, the question arises as to just where these proofs go wrong. It is with precisely this question that Bell begins his analysis of the problem of hidden variables in quantum mechanics, in a paper [20] in the Reviews of Modern Physics written before his EPR paper (1964) but published only in 1966:

The realization that von Neumann’s proof is of limited relevance has been gaining ground since the 1952 work of Bohm. However, it is far from universal. Moreover, the writer has not found in the literature any adequate analysis of what went wrong. Like all authors of noncommissioned reviews, he thinks that he can restate the position with such clarity and simplicity that all previous discussions will be eclipsed. (Bell, page 2)

In fact, Bohm himself had addressed this very question in his 1952 hidden variables paper, a fact of which Bell was well aware when he complained of the inadequacy of the literature on this subject. In a footnote on this sentence Bell informs us that

In particular the analysis of Bohm seems to lack clarity, or else accuracy. He fully emphasizes the role of the experimental arrangement. However, it seems to be implied . . . that the circumvention of the theorem requires the association of hidden variables with the apparatus as well as with
the system observed. The scheme of Section 2 is a counter example to this. Moreover, it will be seen in Section 3 that if the essential additivity assumption of von Neumann were granted, hidden variables wherever located would not avail. (Bell, page 12)

Bell’s criticism of Bohm’s analysis is completely on target. It is therefore somewhat unfortunate that some twenty seven years later we find Bohm and Hiley still insisting that

\[ \ldots \text{the essential point is that von Neumann had in mind hidden parameters that belonged only to the observed system itself and were not affected by the apparatus.} \] (BH, page 118)

As Bell said, what was essential for von Neumann’s argument per se was his linearity assumption, not the location of the hidden parameters. However, we need not go into this linearity assumption here because Gleason, Bell, and Kochen and Specker have shown that it is, in fact, completely unnecessary, that (for a Hilbert space of dimension at least 3) whatever can be proven concerning the impossibility of hidden variables by invoking this assumption can, by a different argument, also be proven without it. Rather, the critical assumption behind just about all of the proofs of the impossibility of hidden variables (except, it might be argued, for those revolving around locality) is that of noncontextuality, “that measurement of an observable must yield the same value independently of what other measurements may be made simultaneously” rather than depending upon “the complete disposition of the apparatus” (Bell, page 9).

Here Bell puts his finger on the essential point. However, just as happened with his discussion of nonlocality, Bell did not express himself at first quite as sharply as he did later. The crucial thing to observe is that, like any good physicist, Bell here speaks without embarrassment of the measurement of a quantum observable; that is, he does not feel compelled, as he later did, to surround the word “measurement” with quotation marks in such situations. For example, in 1982 Bell explains that

\[ \text{the extra assumption is this: the result of ‘measuring’ } P_1 \text{ is independent of which complementary set, } P_2 \ldots \text{ or } P'_2 \ldots, \text{ is ‘measured’ at the same time.} \ldots \text{ We are doing a different experiment when we arrange to ‘measure’ } P'_2 \ldots \text{ rather than } P_2 \ldots \text{ along with } P_1. \] (Bell, page 165)

Moreover, the reason that we find, some seventeen years later, Bell writing in this way is not that he had become more sensitive to grammatical niceties in the intervening years. Rather it reflected his frustration with the continuing abuse of the word “measurement” in discussions on the foundations of quantum mechanics and the confusion that this abuse makes all too common, as well as his desire not himself to contribute to this confusion. As Bell goes on to say
A final moral concerns terminology. Why did such serious people take so seriously axioms which now seem so arbitrary? I suspect that they were misled by the pernicious misuse of the word ‘measurement’ in contemporary theory. This word very strongly suggests the ascertaining of some preexisting property of some thing, any instrument involved playing a purely passive role. Quantum experiments are just not like that, as we learned especially from Bohr. The results have to be regarded as the joint product of ‘system’ and ‘apparatus,’ the complete experimental set-up. But the misuse of the word ‘measurement’ makes it easy to forget this and then to expect that the ‘results of measurements’ should obey some simple logic in which the apparatus is not mentioned. The resulting difficulties soon show that any such logic is not ordinary logic. It is my impression that the whole vast subject of ‘Quantum Logic’ has arisen in this way from the misuse of a word. I am convinced that the word ‘measurement’ has now been so abused that the field would be significantly advanced by banning its use altogether, in favour for example of the word ‘experiment.’ (Bell, page 166)

Now Bohm and Hiley also emphasize the importance of contextuality, but they do not get the emphasis quite right. They say, for example, that “The context dependence of results of measurements is a further indication of how our interpretation does not imply a simple return to the basic principles of classical physics.” Maybe so. But the context dependence of the results of measurements is quite a different matter from the context dependence of the results of “measurements.” By insisting upon speaking of measurements of quantum observables in the usual careless way deplored by Bell, by not taking Bell’s admonition to heart, they make contextuality seem a much more striking innovation than would be expected to be supported by the rather unremarkable observation that the result of an experiment should depend upon how it is performed.

It is, however, somewhat curious that what are otherwise rather different experiments should have come to be regarded by quantum physicists as “measurements” of the same “observable.” For an analysis of how, from the perspective of Bohmian mechanics, experiments are naturally associated with operators—the quantum observables—and in a many-to-one manner, see [21]. I note here merely that any such analysis must take into account Bell’s admonition that

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\ldots \text{in physics the only observations we must consider are position observations, if only the positions of instrument pointers. It is a great merit of the de Broglie-Bohm picture to force us to consider this fact. If you make axioms, rather than definitions and theorems, about the ‘measurement’ of anything else, then you commit redundancy and risk inconsistency. (Bell, page 166)}
\]
A Matter of Ontology

Bohm and Hiley call Bohmian mechanics an ontological interpretation of quantum theory and in so doing they underline a crucial aspect of the theory. What they have in mind is that Bohmian mechanics is grounded in ontology—given by particles described by their positions in space for the nonrelativistic theory—rather than, like the Copenhagen interpretation, in epistemology. Bohmian mechanics directly governs the behavior of the basic elements of this ontology and it is out of this behavior that the observational principles expressed by the quantum formalism emerge. However, the essential point is not some ontology versus no ontology but a clear ontology versus a vague ontology. After all, one would imagine that any physical theory must at some level invoke some ontology, and, in fact, the Copenhagen interpretation presupposes the classical ontology (and it would seem from what is so often written even classical physics) on the macroscopic level—without which, so the argument goes, there could be no coherent discussion of the results of measurement. For the Copenhagen interpretation there is, of course, no ontology for the microscopic level, but would this be so very bad were the notion of the macroscopic not so vague? In surveying the major possibilities for the interpretation of quantum mechanics, Bell noted concerning one of these that

... it may be that Bohr’s intuition was right—in that there is no reality below some ‘classical’ ‘macroscopic’ level. Then fundamental physical theory would remain fundamentally vague, until concepts like ‘macroscopic’ could be made sharper than they are today. (Bell, page 155)

The Quantum Potential and the Structure of Bohmian Mechanics

A reference to the quantum potential, a concept that Bohm and Hiley present as marking the departure of Bohmian mechanics from classical physics, is nowhere to be found in Bell’s book. This not because Bell didn’t like the expression. It was the concept itself, however expressed, that Bell did not like, and it was the concept itself, and not just the expression, that he completely avoided. This might make it difficult to appreciate that the two books under consideration here are concerned with the same physical theory, since one of them eschews what appears to be the central innovation of the theory described by the other.

Bohm and Hiley, following Bohm’s 1952 paper [9], arrive at Bohmian mechanics by first writing the wave function $\psi$ in the polar form $\psi = Re^{iS/\hbar}$ where $S$ is real and $R \geq 0$. They then rewrite Schrödinger’s equation in terms of these new variables, obtaining a pair of coupled evolution equations. In particular, they obtain a “continuity equation” for $\rho = R^2$, which, as usual, suggests that $\rho$ be interpreted as a probability density, and a modified Hamilton-Jacobi equation for $S$, differing
from the usual classical Hamilton-Jacobi equation only by the appearance of an extra term, the quantum potential, alongside the classical potential energy term.

Bohm and Hiley then use the modified Hamilton-Jacobi equation to define particle trajectories just as is done for the classical Hamilton-Jacobi equation. The resulting motion is precisely what would have been obtained classically if the particles were acted upon by the force generated by the quantum potential in addition to the usual forces. Bohm and Hiley point out that the quantum potential has many unusual properties, which, they say, account for all the quantum miracles, from the two-slit experiment to nonlocality.

The quantum potential is a function on configuration space which is determined by the wave function—it is a functional of the wave function—whose detailed form (which happens to be $-\frac{\hbar^2}{2m} \nabla^2 R$) need not concern us here. It is, however, worth noting that the quantum potential does depend upon $R$ (and only on $R$). This seems to suggest that Bohmian mechanics requires a peculiar dependence of dynamics upon probability and naturally leads to Wigner’s question [10, page 290] as to “how the properties of the ensemble, described by $R^2$, can influence the motion of an individual system, the system which $S$ is supposed to describe.”

Now Bohm and Hiley were well aware of such objections, and carefully explain why they are not valid—as did Bohm already in 1952. The essential point is that the identification of $R^2$ with probability is somehow of a secondary character, and that the fundamental role of $R$, as well as of $S$, is the dynamical one. Nonetheless, formulating Bohmian mechanics directly in terms of variables that we naturally regard as having a probabilistic character is an invitation to such objections, an invitation that most physicists are glad to accept!

It must be admitted that the problem of the status of probability in Bohmian mechanics—of the identification of $|\psi|^2$ with the probability density for position—is a subtle one, and that the analysis of Bohm and Hiley of this issue is neither entirely adequate nor completely up to date. (For a recent detailed treatment, see [22].) However, whatever inadequacies exist in the treatment of Bohm and Hiley on this score are relevant only to a level of analysis far deeper than would be appropriate, alas, in almost any contemporary discussion of the foundations of quantum mechanics.

Bohm’s rewriting of Schrödinger’s equation via variables that seem interpretable in classical terms does not come without a cost—beyond the one mentioned above. The most obvious cost is increased complexity: Schrödinger’s equation is rather simple, not to mention linear, whereas the modified Hamilton-Jacobi equation is somewhat complicated, and highly nonlinear—and still requires the continuity equation for its closure. The quantum potential itself is neither simple nor natural (even to Bohm it has seemed “rather strange and arbitrary” [23]) and it is not very satisfying to think of the quantum revolution as amounting to the insight that nature is classical after all, except that there is in nature what appears to be a rather ad hoc additional force term, the one arising from the quantum potential.

Moreover, the connection between classical mechanics and Bohmian mechanics
that emerges from the quantum potential is rather misleading. The point is that Bohmian mechanics is not classical mechanics with an additional force term. In Bohmian mechanics the velocities are not independent of positions, as they are classically, but are constrained by the guiding equation

\[ v = \nabla S/m. \]

In classical Hamilton-Jacobi theory we also have this equation for the velocity, but there the Hamilton-Jacobi function \( S \) can be entirely eliminated and the description in terms of \( S \) simplified and reduced to a finite dimensional description, with basic variables the positions and momenta of all the particles, given by Hamilton’s or Newton’s equations.

It is important to recognize that the guiding equation, expressing the rate of change \( dq/dt \) of the configuration as a specific functional of the wave function, is not only necessary for Bohmian mechanics but is also sufficient. Given any solution \( \psi(q,t) \) of Schrödinger’s equation, the right hand side of the guiding equation—and hence a first-order evolution equation for the configuration—is determined for all time. This equation in turn determines the configuration at any time in terms of the configuration at any other time. In particular, since a solution of Schrödinger’s equation is determined by the wave function at any initial time, Bohmian mechanics is entirely deterministic, in the sense that the initial wave function and configuration completely determine the motion of the system under consideration.

What concerns us here is the fact that the dynamics for Bohmian mechanics is thus completely defined by Schrödinger’s equation together with the guiding equation, and there is neither need nor room for any further axioms involving the quantum potential! Thus the quantum potential should not be regarded as fundamental, and we should not allow it to obscure, as it all too easily tends to do, the most basic structure defining Bohmian mechanics.

This is not to say that the quantum potential is of no value. One would naturally expect the modified Hamilton-Jacobi equation to be convenient, for example, when considering the classical limit of Bohmian mechanics, in which the quantum potential is negligible, and Bohm and Hiley have an excellent presentation of this. However, in just about all of the other applications of the quantum potential found in the book of Bohm and Hiley, the quantum potential itself does not play a genuine role in the analysis; rather in all such cases it is the wave function and the guiding equation that are relevant to the analysis.

To my mind, the most serious flaw in the quantum potential formulation of Bohmian mechanics is that it gives a completely wrong impression of the lengths to which we must go in order to convert orthodox quantum theory into something more rational. The quantum potential suggests, and indeed it has often been stated, that in order to transform Schrödinger’s equation into a theory that can account, in what are often called “realistic” terms, for quantum phenomena, many of which are dramatically nonlocal, we must incorporate into the theory a quantum potential of a grossly nonlocal character.
But note again that Bohmian mechanics incorporates Schrödinger’s equation into a rational theory describing the motion of particles merely by adding a single equation, the guiding equation, a first-order evolution equation for the configuration. Moreover, the form of the right hand side of this equation is already suggested by the (pre-Schrödinger equation) de Broglie relation $p = \hbar k$, as well as by the eikonal equation of classical optics.

Furthermore, if we take Schrödinger’s equation directly into account—as of course we should since we seek its rational completion—a form for the velocity emerges in an almost inevitable manner, indeed via several routes. Bell’s preference is to observe that since standard quantum mechanics provides us with a probability current $J$ as well as with a probability density $\rho$, which are classically related (as they are for any dynamics given by a first-order ordinary differential equation) by $J = \rho v$, it requires no great imagination to write

$$v = J/\rho,$$

which is completely equivalent to the guiding equation as written above. We should thus not be surprised to find Bell writing the following about the two-slit experiment:

Is it not clear from the smallness of the scintillation on the screen that we have to do with a particle? And is it not clear, from the diffraction and interference patterns, that the motion of the particle is directed by a wave? De Broglie showed in detail how the motion of a particle, passing through just one of two holes in screen, could be influenced by waves propagating through both holes. And so influenced that the particle does not go where the waves cancel out, but is attracted to where they cooperate. This idea seems to me so natural and simple, to resolve the wave-particle dilemma in such a clear and ordinary way, that it is a great mystery to me that it was so generally ignored. (Bell, page 191)

My own preference is to proceed in a somewhat different manner, avoiding any use of probabilistic notions even in the motivation for the theory, and see what symmetry considerations alone might suggest. Then one finds (see the first reference of [22]) that the simplest choice, compatible with overall Galilean and time-reversal invariance, for an evolution equation for the configuration is given by

$$v = \frac{\hbar}{m} \text{Im} \frac{\nabla \psi}{\psi},$$

which also is completely equivalent to the original guiding equation.

It thus seems fair to say that with regard to nonrelativistic quantum mechanics, the essential departure of Bohmian mechanics from quantum orthodoxy is merely to insist that particles have positions regardless of whether or not they are observed. We arrive at the specific evolution equations of Bohmian mechanics and a rational completion of Schrödinger’s equation simply by assigning, in the most obvious way, a role to the wave function in the evolution of these positions.
Intuitive Explanation

In connection with their attitude towards nonlocality, we have seen that Bohm and Hiley ascribe considerable importance to intuitive explanation, a rather vague concept that Bell avoids. It is not unreasonable to suppose that the difference of opinion described above with regard to the quantum potential originates in a difference with respect to intuitive explanation, a notion that presumably involves explanation in familiar terms, for example in terms based on the concepts of classical mechanics—hence the invocation of the quantum potential.

It hardly seems necessary to remark, however, that physical explanation, even in a realistic framework, need not be in terms of classical physics. Moreover, when classical physics was first propounded by Newton, this theory, invoking as it did action at a distance, did not provide an explanation in familiar terms. Even less intuitive was Maxwell’s electrodynamics, insofar as it depended upon the reality of the electromagnetic field. We should recall in this regard the lengths to which physicists, including Maxwell, were willing to go in trying to provide an intuitive explanation for this field as some sort of disturbance in a material substratum to be provided by the Ether. These attempts of course failed, but even had they not, the success would presumably have been accompanied by a rather drastic loss of mathematical simplicity.

In the present century fundamental physics has moved sharply away from the search for such intuitive explanations in favor of explanations having an air of mathematical simplicity and naturalness, if not inevitability, and this has led to an astonishing amount of progress. In this regard it must be emphasized that the problem with orthodox quantum theory is not that it is unintuitive. Rather the problem is that

\[ \ldots \text{conventional formulations of quantum theory, and of quantum field theory in particular, are unprofessionally vague and ambiguous. Professional theoretical physicists ought to be able to do better. Bohm has shown us a way. (Bell, page 173)} \]

The problem, in other words, is not that orthodox quantum theory fails to be intuitively formulated, but rather that, with its incoherent babble about measurement, it is not even well formulated!

Spin

This difference of opinion concerning the importance of “intuitive explanation” is perhaps put in sharpest relief by the respective approaches of Bell and of Bohm and Hiley to the treatment of spin in Bohmian mechanics. Spin is the canonical observable having no classical counterpart, reputed to be impossible to grasp in a nonquantum way. The source of the difficulty is not so much that spin is quantized in
the sense that its allowable values form a discrete set (for a spin $\frac{1}{2}$ particle, $\pm h/2$) — energy too may be quantized in this sense—nor even precisely that the components of spin in the different directions fail to commute and so cannot be simultaneously discussed, measured, imagined, or whatever it is that we are admonished not to do with noncommuting observables. Rather the difficulty is that there is no ordinary (nonquantum) quantity which, like the spin observable, is a 3-vector and which also is such that its components in all possible directions belong to the same discrete set. The problem, in other words, is that the usual relationships among the various components of spin are incompatible with the quantization conditions on the values of these components.

For example, one such relationship is that if $s_x$ and $s_y$ are the components of spin along the positive $x$ and $y$ directions, then $(s_x + s_y)/\sqrt{2}$ is also the component of spin in a certain direction (namely, the direction bisecting the positive $x$ and the positive $y$ directions). This implies that the possible values of the component of spin in this direction must, for the case of spin $\frac{1}{2}$, be either 0 or $\pm \sqrt{2}(h/2)$, none of which are permissible values for such a spin component. (In fact, insofar as it is relevant to the issue of hidden variables, von Neumann’s theorem on their impossibility amounts to no more than the preceding rather trivial observation.)

We thus might naturally wonder how Bohmian mechanics manages to cope with spin. And the impression we will get from Bohm and Hiley is: only with great difficulty! Bohm and Hiley first propose a model in which the electron is a spinning body, a proposal that they ultimately abandon, having “reached a point where the attempt to account for the acceleration in terms of various forces no longer provides any useful insight,” in favor of a theory that “still works consistently as long as we have a suitable guidance condition.” But even at this point, at which “there is no extended particle that is spinning,” they still find it necessary to say that “spin represents only some average property of the circulating orbital motion” (BH, page 221) [my emphasis].

From Bell, however, we get an entirely different impression. The guiding equation $v = J/\rho$ serves as well to define Bohmian mechanics for a spinor-valued wave function, governed by the Pauli equation, as it does for a scalar-valued wave function, governed by Schrödinger’s equation. Moreover, this guiding equation is also what we find if we express the symmetry-based form of the guiding equation, $v = \frac{\hbar}{m} \text{Im} \frac{\nabla}{\psi} \psi$, in a manner appropriate for a spinor-valued wave function, namely, $v = \frac{\hbar}{m} \text{Im} \frac{\psi^* \nabla \psi}{\psi^* \psi}$. All of the “mysteries of spin” are a simple consequence of the dynamics so defined:

We have here a picture in which although the wave has two components [for a single particle], the particle has only position. . . . The particle does not ‘spin’, although the experimental phenomena associated with spin are reproduced. Thus the picture . . . need not very much resemble the traditional classical picture that the researcher may, secretly, have been keeping in mind. The electron need not turn out to be a small spinning yellow sphere.” (Bell, page 35)
The Quantum Revolution from the Perspective of Bohmian Mechanics

Bohmian mechanics is more than merely an alternative to the orthodox Copenhagen interpretation of quantum theory, more than a choice between equals. After all, orthodox quantum theory, with its invocation of “measurement” in a fundamental and irreducible manner, with its appeal to collapse and to the observer, does not exist as a precise, well-formulated physical theory. In fact, it could be argued that orthodox quantum theory is physically vacuous.

This of course raises the question as to how physicists have managed with such great success to employ orthodox quantum theory—how this theory could work so well for all practical purposes! The reason for this, I would argue, is that in using orthodox quantum theory physicists are thinking in Bohmian terms—despite the fact that they would claim they are doing precisely the opposite. Consider, for example, how a physicist thinks about a scattering experiment: a particle described by a wave packet with more or less definite momentum and position enters the scattering center and emerges in a random, though definite, direction, revealed by detectors. Or consider, more generally, the prominence of detectors in bringing quantum experiments to a close—detectors which every physicist, when not in deep quantum mode, would regard as detecting the particles where these particles in fact are at! Or consider, in particular, the double-slit experiment, in which an interference pattern is formed from the accumulated spots marking the arrivals of the particles at the detector.

Moreover, Bohmian mechanics is not merely a counterexample—to the assertion that quantum phenomena demand a fundamentally nondeterministic and subjective description—cooked up to do the job. It has a deep mathematical and physical integrity. In addition, it provides at least the beginnings of an answer to the question, “What exactly is the quantum revolution?” I believe that this involves a transition from Newtonian physics, second-order physics, in which acceleration and forces play a fundamental role, to first-order physics, in which it is the velocities, the rates of change of position, that are fundamental in that they are specified by the theory in a reasonably simple manner. Note that the very possibility of such a theory, a relativistic (Galilean relativity for the nonrelativistic case) Aristotelian dynamics as it were, is quite surprising.

Bohmian mechanics is the natural embedding of Schrödinger’s equation—which equation is the common part of almost all interpretations of quantum theory, however different they may otherwise be, from the Copenhagen interpretation to the many-worlds interpretation—into a physical theory. It emerges if one merely insists that the Schrödinger wave function be relevant to the motion of particles. (And notice that if we are to have a clear physical theory at all, the wave function had better be relevant to the behavior of something of clear physical significance!) In other words, Bohmian mechanics arises from Schrödinger’s equation when (perhaps naively) we insist upon the simplest ontology—particles described by their
positions—and seek a natural evolution for this ontology.

All the mysteries of quantum theory find a compelling explanation in Bohmian mechanics—in the obvious ontology evolving in the obvious way! To understand how this can be, as well as to develop some appreciation for the many very difficult remaining open problems in the foundations of quantum theory, such as that of the tension between nonlocality and Lorentz invariance, the books of Bell and of Bohm and Hiley are indispensable. Read them—first Bell, then Bohm and Hiley, and then Bell again! You will be richly rewarded for your efforts. If you don’t quite develop a clear conception of what the quantum revolution in fact is, you will at least be confident about what it is not!

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