Trouble in Paradise? Problems for Bohm's Theory

As recently documented in J. Cushing's excellent book, throughout its almost fifty-year history, Bohm's interpretation of quantum mechanics has been systematically misunderstood and ignored.[1] On the rare occasions it was examined, it was usually dismissed for reasons having more to do with politics, religion, positivism, and sloppy thought, than for reasons central to physics. In recent times, however, Bohm's theory has received much more serious attention from physicists and philosophers, thanks mainly to J. S. Bell's positive influence on the theory. Most of this work reacts favorably toward Bohm's theory. And this is not surprising, for Bohm's theory provides us with a consistent, empirically adequate picture of the quantum world.

However, it would be naïve to think Bohm's theory is free of problems. Like any physical theory, Bohm's theory faces difficulties of varying degrees of severity. Some are problems encountered when applying the theory to new domains, e.g., quantum field theory, while others arise simply in understanding the basic ontology of the theory, e.g., the status of the wave-function. Since there is no real question—or at least there shouldn't be—as to whether Bohm's theory is internally coherent and empirically adequate, these problems should be regarded not as challenges to its coherency, but rather to its plausibility. In this essay we examine a handful of difficulties that can be distilled from the recent commentary on Bohm's theory. These challenges are by no means exhaustive. In deciding which to consider, we chose to focus on those problems we deem genuine ones, problems more physical than philosophical in origin. Bohm's theory has already suffered enough at the hands of philosophical critiques.

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1. Bohm's Theory: How It Works

Our first task is to make it clear that Bohm's theory does indeed solve the measurement problem in a coherent fashion. That it is incumbent upon us to do so is a bit of a scandal, as its coherency was well established as early as 1952. However, even to this day it has been claimed that Bohm's theory does not solve the measurement problem.[2] To remove any doubt inspired by such claims, we will briefly review how Bohm's theory works.

In the version of nonrelativistic Bohmian mechanics developed by Durr *et al.*, much more attention is paid to Bohm's theory being a universal theory than was originally given to it by Bohm.[3] Bohmian mechanics is characterized by an ontology of particles in spacetime governed by a universal wave function in configuration space. The particles are governed by $md\vec{x}/dt = \nabla S$, where S is the phase of the universal wave function Ψ, and Ψ is governed by the familiar Schrödinger equation. In addition, it is supposed that the particles were initially distributed with a probability density (on configuration space) such that $P = |\Psi(Q)|^2$. Call this the "distribution postulate"—we'll talk more about it later. These two laws, one for the wave function and one for the particle positions, or "beables" in Bell's terminology, constitute the theory. With the distribution postulate, it can be shown that the theory is empirically adequate (see Durr *et al.* 1992).

To briefly repeat a familiar story, the measurement problem arises when one assumes (1) the wave function of a system completely describes the physical properties of a system, (2) the wave function evolves according to Schrödinger's equation, and (3) measurements have definite outcomes. As Schrödinger showed, these three assumptions will allow for superpositions of macroscopic objects between macroscopically distinct states. Since we apparently don't observe such states, something more needs to be said.

Bohm's theory denies premise (1). The wave function is only half the story. There are also particles with continuous, determinate trajectories. Assuming all measurements are ultimately measurements of particle position, on Bohm's theory, measurements are observations of particle positions. Since these are always definite, so too are our observations. Bohmian measurement theory then guarantees that when the wave function of a system evolves into a superposition of macroscopically distinct states, the particles will always be forced into one component of the superposition or the other. Moreover, assuming the distribution postulate, it can be shown that the particles will be found with a probability given by Born's rule. For more on how Bohm's theory works, the reader is invited to study any one of the many splendid recent examinations of the theory.[4]

By postulating a world of particles, Bohm's theory nicely resolves the measurement problem. The particles are always definite, even when the wave function (as it typically will be) is not. Further, having a coherent ontology helps cure a host of other maladies in the foundations of the theory, e.g., it provides an attractive understanding of the classical limit.

2. *Spoiled Symmetries*

As we mentioned at the outset, Bohm's theory has suffered more at the hands of philosophy than at the hands of physics. But it is not easy to carve up all of its criticisms into two classes, the philosophical and the physical. Many of the physical problems are considered problems only due to opinions that might best be described as philosophical in origin. We begin by briefly considering three such problems. What is common to all three is that they challenge neither the internal coherency nor the empirical adequacy of the theory. Rather, they are complaints that Bohm's theory lacks various features that are—for some reason or other—desirable of physical theories.

First, since its inception, some have objected to (nonrelativistic) Bohm's theory's reliance on particles because it spoils the symmetry of Hilbert space. The theory is formulated in coordinate space rather than the infinite number of other spaces available, e.g., momentum space. Isn't it arbitrary that the coordinate basis is preferred over the momentum basis? To rid Bohm's theory of this "problem," some have sought to give a momentum-space treatment of Bohm and others have sought to give a Bohm theory for all possible spaces.[5] To our minds these attempts seem misguided. The "problem" is really no problem, and the "solutions" cannot succeed.

The simple reason why position is special in Bohm's theory is that the measurement problem requires it to be. Arguably, all measurements are the measurements of the positions of things. That is partly why Bohm's theory works. But the other reason (pointed out very nicely by Dickson [1995] in the framework of a different interpretation[6]) concerns an asymmetry between position measurements and momentum measurements: "effective collapses" of position superpositions also collapse momentum superpositions, but not *vice versa*. An effective collapse of a momentum superposition will sometimes leave a system in a *superposition of macroscopically distinct positions*. Since solving the measurement problem *just is* ensuring that this doesn't happen, a momentum-space version of Bohm's theory—while mathematically possible—cannot solve the measurement problem in the real world. For this reason, attempts at formulating the theory in spaces other than position may have academic interest, but it is doubtful that they will lead to physically viable theories. Inasmuch as one wishes to provide a coherent ontology for nonrelativistic quantum mechanics, then, one seems forced to choose position-like quantities as the beables of the theory.

Second, Einstein considered it a mark of a "physically reasonable" theory that all of the entities posited by the theory both act on and are acted upon by the other entities of the theory. This remark, the reader will recall, was part of his rejection of Newtonian absolute space. Anandan and Brown elevate this criterion, which they call the "action-reaction principle," to a kind of meta-scientific law.[7] Theories positing entities that are not acted on and do not act upon all other entities of the theory are not physically reasonable. They see this as a problem for Bohm's theory. In Bohm's theory, the wave function acts on the particles, but the particles do not react back on the wave function. Bohm's theory, therefore, fails to satisfy the action-reaction principle.

Why this should be a problem for the theory, however, is unclear. What is the physical justification for this principle? Bohm's theory is logically consistent and empirically adequate. Why should its failure to meet some *a priori*, apparently aesthetically-grounded, principle be a problem for it? Such meta-scientific principles have not enjoyed much success in the history of science. The action-reaction principle looks "natural" from the perspective of classical conservation laws. But it should be emphasized that Bohmian mechanics is a highly non-classical theory. Unlike classical mechanics, it has a dual ontology and the particles are governed by a *first-order* (Aristotelian) equation of motion. The classical picture of forces acting and reacting to each other is completely abandoned. In our opinion, there is simply no reason to expect the classical action-reaction principle to hold in a quantum world, and there is even less reason in a Bohmian world.

Third, as is well-known, Bohm's theory is highly non-local. The wave function evolves in multi-dimensional configuration space, so as long as the wave function for an n-particle system is non-separable, the motion of a particle will be connected to the motion of other, sometimes distant, particles. It is this feature of Bohm's theory that reproduces the Bell phenomena. Since at least Einstein linked together locality with "realistic" theories, philosophers and physicists have balked at the explicit non-locality in Bohm's theory. But why shouldn't the world be non-local? There is no logical problem with this. And indeed, it seems we have evidence of non-locality. As Bell observed, it's implicit in the mathematical structure of quantum mechanics, inasmuch as the theory is defined *via* a wave function on configuration space. Moreover, Bell's theorem and the Aspect experiments suggest we may have to make do with nonlocality, whatever interpretation of quantum mechanics we adopt (with the notable exception of the so-called "many-minds" view).[8] With this in mind, we feel it wise to heed the advice of Bohm and Hiley: "one can understand this feeling [that non-locality is spooky], but if one reflects deeply and seriously on this subject one can see nothing basically irrational about such an idea. Rather it seems most reasonable to keep an open mind on the subject and therefore to allow oneself to explore this possibility. If the price of locality is to make an intuitive explanation impossible, one has to ask whether the cost is not too great."[9]

3. *Field Theory*

According to contemporary physics, the quantum mechanics of non-relativistic particles is not fundamental. Instead, what we call particles are manifestations of relativistic quantum fields. Relativistic quantum field theory, then, is a more fundamental theory than elementary quantum mechanics, and it follows that believers in Bohm's theory should apply it to relativistic quantum fields.

Let's see how this is done, therefore, using the scalar field as an example. The scalar field is represented, at a given time t, by a function $\varphi(\vec{x})$ that assigns a scalar (the value of the field) to each point $\vec{x}$ of (3-dimensional) space. Just as in a many-particle system, where positions of the particles, $\vec{x}_i$, are indexed by the particle index, the field values are indexed by the spatial position $\vec{x}$. Thus, in the transition from a many-particle system to the scalar field, it is natural to take the new elements of reality, or "beables" (in Bell's terminology) to be the field values. Once this is decided, it is standard to give it a Bohmian treatment, in close analogy to the Bohm particle theory.

From the classical equation of motion for the field, we can find the Hamiltonian of the field. Working in the Schrödinger picture, the wave function $\Psi((\varphi(\vec{x}),t)$ will have the time evolution given by $id\Psi/dt = \hat{H}\Psi$. The Bohm equation of motion for the time evolution of the field will be

$$\delta\varphi(\vec{x})/\delta t = \delta S/\delta\varphi(\vec{x}) \qquad (1)$$

where S is the phase of Ψ. As in the case of Bohmian particle mechanics, we have two deterministic equations of motion: one for the field and one for the wave function. Again, as in that theory, $|\Psi(\phi(\vec{x}))|^2\Delta\varphi(\vec{x})$ is required to be the probability of finding the field between $\varphi(\vec{x})$ and $\varphi(\vec{x})$ $\vec{x}$ $\Delta\varphi(\vec{x})$. It is important to be clear that $\varphi(\vec{x})$ is not the operator-valued field that appears in books about quantum field theory. $\varphi(\vec{x})$ is instead an eigenvalue of that operator. If $|\phi(\vec{x})>$ is the quantum state of the field in which the field is $\varphi(\vec{x})$, and $\hat{\phi}(\vec{x})$ the operator-valued field, then $\hat{\phi}(\vec{x})\,|\,\varphi(\vec{x})> = \varphi(\vec{x})\,|\,\varphi(\vec{x})>$.

Three questions immediately arise about this Bohm field theory. (1) Working within the context of special relativity for the time being, is the theory Lorentz invariant? (2) The scalar field is only one among many (possible) physical fields. The electromagnetic field is a 4-vector and the Dirac (electron) field is a spinor field, for example. Can the Bohm treatment just sketched be extended to them as well? (3) A field is an entity that is extended throughout space. But the world appears to be full of localized entities—some large, to be sure, like stars, but also planets, houses, cars, people, bananas, amoebas, hydrogen atoms, electrons, etc. Can we give an account of these localized entities in terms of a fundamental entity (the field or fields) that is non-localized?

Beginning with the question of Lorentz invariance, at first sight the news looks bad. The Bohm equation of motion for the scalar field (1) is not Lorentz invariant. It holds only with respect to a preferred frame. The theory's non-locality bears primary responsibility for this. There are non-local effects between different points of the field, and the frame in which these connections are instantaneous is the preferred one.

The Bohm equation of motion for a nonrelativistic particle mechanics is also highly non-local. However, it is Galilean invariant since simultaneity is Galilean invariant. All inertial frames share the same series of moments of time, so non-local causal connections do not conflict with Galilean invariance. For a single relativistic particle there *can* be a Lorentz-invariant Bohm theory. But once we have the simplest generalization of a single particle, a relativistic two-particle system with no interaction through a potential (much less a field), we lose Lorentz invariance. This is due to the non-local "quantum" causal connections between the two particles.

But is this news as bad as it first seems? Not at all, and for two reasons. First, while the dynamics of the field are not Lorentz invariant, the dynamics of the quantum theory is Lorentz invariant. In particular, as long as the field version of the distribution postulate holds, observed distributions will agree with those predicted by quantum theory. That is, we will not detect a violation of Lorentz invariance, even though it is violated by the field dynamics.

Second, although there is a violation of Lorentz invariance, this doesn't mean that we have to give up the view that there is an underlying flat spacetime structure. What we have to accept is that there is additional, physically significant spacetime structure, namely, that there is a preferred frame. We do not, as Bohm and Hiley do, postulate that the Lorentz metric breaks down at very small length scales.[10]

Question two asked whether the Bohmian program for the scalar field could be extended to other physically significant fields as well. To answer this, we must note a distinction between two kinds of fields, Bose fields and spinor fields. Bose fields are fields of integer spin, and classically, they are tensor representations of the Lorentz group. The scalar field, the electromagnetic (4-vector) field, and the nonabelian gauge fields (which are also vectors with respect to the Lorentz group, but carry so-called internal space indices as well) are all examples. These fields can be handled in close analogy with the scalar field, with a little modification due to gauge invariance. For spinor fields, like the Dirac electron field, the extension is not as straightforward. For a long time it was believed such an extension was not possible. But a few years ago Valentini realized that one could extend the Bohm equation of motion to spin 1/2 Fermi fields.[11] The trick—classically—is to take such fields to be two-component anticommuting or Grassman fields, functions which assign to each point $\vec{x}$ of three-space a two-component Grassman spinor $\Phi^\alpha(\vec{x})$.

This raises new questions. In particular, the whole motivation of the Bohm program is to have a well-defined ontology, a beable that has a definite value at each time. But could such an element of reality be a Grassman quantity, whose components are Grassman numbers? Besides being anti-commuting, real Grassman "numbers" do not, in any natural sense, have an ordering like real numbers. Further, whether Grassman or not, spinors have the property that they change sign under a 2π rotation.

Whether or not these are really sufficient reasons for denying that the anticommuting spinor field $\Phi^\alpha(\vec{x})$ is (or directly represents) an element of reality is, in our opinion, not settled. However, whatever the status of $\Phi^\alpha(\vec{x})$, $\Phi^\alpha(\vec{x})\Phi^\alpha$ will be a field whose values are ordinary commuting numbers and is thus observable.

Before the work of Valentini, Bell proposed a different solution to the problem of Fermi fields.[12] What he did, in effect, was to write down a dynamics for $\Phi^{\alpha\dagger}(\vec{x})\Phi^\alpha(\vec{x})$ directly, and not through a dynamics for $\Phi^\alpha(\vec{x})$. While Bohmian in spirit, since the beable has definite values at all times, it has the drawbacks that the beable evolves stochastically in discrete jumps and space is taken to be a lattice with characteristic dimensions of (say) the Planck length. The interested reader can find more about this approach from Vink, who does investigate the important question of the continuum limit.

Question three is the problem of accounting for localized particles in terms of fields that are spread out in space. Again, let's consider the scalar field for simplicity. By using one-particle states, we can construct states that in the non-relativistic (low energy) domain are effective one-particle wave functions $\psi(\vec{x})$. For such a state, we can show that the most probable ("mp") field distribution $\varphi_{mp}(\vec{x})$ satisfies, up to a constant,

$$\varphi_{mp}(\vec{x}) = \mathrm{Re}\ \psi(\vec{x}).$$

Thus, for localized one-particle wave functions, we see that the field is concentrated in a region the size of the wave function. Therefore, it is highly probable that (from the point of view of the effective one-particle state) a particle will be found in that localized region, for the field is concentrated in that region. In *such* circumstances, localized concentrations of the field can play the role of particles.

However, more needs to be said for two reasons. First, highly localized one-particle wave functions for free particles spread rapidly. Second, states of the free field that are eigenstates of the number operator, in which momentum is sharply defined, are highly non-local. Yet when we interact with the field, say with a phosphorescent screen, we will find localized particles, i.e., tiny blips, on our screen. How does Bohmian field theory account for these two facts?

The answer is simply that the little blips do not mean that localized entities are striking the screen. Rather, a detailed analysis (see Bohm and Hiley) shows that the extended propagating field is being sucked in or concentrated at points on the screen. As long as the "particles" of the instrument are localized, the appearance of the blips can be given a Bohmian explanation. Further, the wave functions of the particles in the instruments (and in people, thank goodness) are *bound states* and therefore do not spread! Consequently, they can be identified with localized regions in which the field is concentrated (or for Fermi fields, regions in which $\Phi^{\alpha}(\hat{x})\Phi^{\alpha}$ is concentrated).[13]

4. Quantum Gravity?

We have been considering quantum-field theory in flat spacetime. But what about quantized general relativity? Can there be a Bohm theory of the gravitational (or spacetime metric) field? The answer, with certain qualifications (one of them important) is "yes." The metric field is a Bose field and its Bohm theory can be developed in close analogy with other Bose fields we have considered. Since the Bohm equation of motion requires that we put general relativity into Hamiltonian form (so that we can write down the Schrödinger equation), we must assume that spacetime can be sliced up globally into space and time. That is one of the qualifications. Given this, the configuration space of our wave function will be the set of all pairs $\{h,\varphi^i\}$ of three-metrics h and matter fields φ^i. We can then write down a Bohm equation of motion for h in close analogy with the other Bose fields and the evolution of h will be driven by the phase of Ψ.

This Bohm treatment of quantized general relativity has some distinct advantages over other interpretations. Let us mention one. It turns out that the Hamiltonian H of classical relativity is zero. In quantum mechanics this leads to the so-called "Wheeler-DeWitt equation"

$$i d\Psi/dt = \hat{H}\Psi = 0.$$

There is no time evolution of the (Schrödinger) wave function; moreover, $d/dt\langle\Psi \mid \hat{R} \mid \Psi\rangle = 0$, where $\hat{R}$ is the operator representing the radius of the universe. The world is static. This is known as the "problem of time" in quantum gravity. According to Bohm's theory, however, the wave function Ψ and the radius of the universe (the metric) R are governed by different equations. In particular, in simple Robertson-Walker universes the Bohm equation of motion is (roughly) $dR/dt \propto \partial S/\partial R$, where again, S is the phase of the universal wave function. As long as S is not constant, R will evolve. Bohm's theory thus solves the problem of time.[14]

In effect, we have defined a privileged or distinguished time, the Bohm time, via $\partial S/\partial R$. The question then arises, what is the relation between this Bohm time, which has been introduced theoretically, and the "observed" time of physics, the time variable in the Schrödinger equation for quantum-field theory. The answer, given plausible assumptions, is that they are the same.[15]

Let us briefly elaborate on this. Remember that the universal wave function Ψ is a functional not only of the spatial metric h but also of *all* the matter fields φ^i too. Thus the Wheeler-DeWitt equation is the fundamental quantum mechanical equation. On the other hand, we know that we can usually (in the Schrödinger picture) write "effective" wave functions Ψ_{matter} for the matter fields without considering the metric. And these wave functions are governed by a time-dependent Schrödinger equation that does not vanish, $\hat{H}\Psi = i d\Psi dt \neq 0$. What can be shown is that in certain models, the 't' in $i d\Psi/dt \neq 0$ is the fundamental Bohm time! In our opinion, this is an important concrete example of how the "new physics" associated with solving the measurement problem can aid physical theorizing in areas outside measurement theory. The "new physics" need not be idle, but instead may be crucial to our theory-building!

Although Bohmian-quantized general relativity can thus be carried out, problems remain with quantized general relativity. These are not problems of interpretation, but technical problems in the formulation of the quantum dynamics, not the Bohm dynamics. At least in perturbation theory, quantized general relativity is a non-renormalizable quantum field theory. But that is a subject for another paper.

5. The Status of the Wave Function

To begin, let's consider the nonrelativistic Bohm particle theory. The universal wave function is the many-particle wave function $\Psi(\vec{q}_1,\vec{q}_2, \ldots \vec{q}_n)$. As we know, its evolution is given by the Schrödinger equation, while its phase, *via* the Bohm equation, determines the particle velocities. Now what is the status of Ψ? There seem to be just two possibilities. Either Ψ is itself a beable or element of reality, or it is part of the law of motion of the particles and not an "independent" variable itself.

The first choice has the problem—if it is one—that Ψ is not a function of physical space, but of the 3n-dimensional configuration space of the n-particle system. This has troubled some people because if Ψ is an element of reality, doesn't that mean that configuration space is too? We will return to this question momentarily.

Despite this problem, the first choice seems the natural one. The wave function has its own time evolution governed by a law (the Schrödinger equation) and it is *not* uniquely determined by that law. An initial wave function must be specified. In effect, the second choice elevates the initial wave function to the status of a law of nature, thereby conflating the two. But one wonders why the initial wave function should have a nomologically different status than the initial particle positions? If one is law, why not the other?

The second choice also *appears* to dramatically curtail the number and kinds of counterfactual claims one can make consistent with Bohm's theory. If the initial wave function is nomological in nature, then models satisfying the two Bohmian evolution laws but with different initial wave functions are not models of Bohm's theory. The statement "The wave function for this system might have been different" is no longer a counterfactual, but is now a "counterlegal." Assuming that the laws support counterfactual claims, this consequence is a bit awkward. Perhaps it can be skirted, however, by interpreting counterfactual claims in terms of claims about particles. Since particle location is not law-like, and the universal wave function is by now extremely entangled, we can make sense of the idea that the particles might have been distributed some other way than they are, that (consistent with the laws) the world might have looked quite different. Although the particular form of the wave function may be nomologically necessary on this view, *which* components of the wave function the particles occupy is a contingent matter, depending only on the initial conditions.

Now let's look at the two choices in the context of Bohmian field theory. According to choice one, we have to accept as an element of reality a function that is now defined on a configuration space whose points are collections of fields, $\{g, \varphi^i\}$. However, the equation of motion for Ψ is not the time dependent, but as we have now seen, the time independent Schrödinger equation $\hat{H}\Psi = 0$. It has recently been suggested by Durr *et al.* that, as a consequence, there is no distinction between law and initial conditions for Ψ, a view which is popular in contemporary quantum cosmology.[16] Rather, $\hat{H}\Psi = 0$ is just the condition the lawful wave function satisfies, rather like $\nabla^2\phi = \delta$ is the condition the inverse-square law satisfies. In other words, Durr *et al.* claim that choice one is no longer the natural one.

Two points should be made about this suggestion. First, $\hat{H}\Psi = 0$ does not uniquely determine the universal wave function Ψ. Boundary conditions must be imposed, as in the difference between the Vilenkin and Hawking wave functions. Second, because of the non-renormalizability of standard quantum general relativity, there is, as we indicated earlier, a question of how seriously we should take $\hat{H}\Psi = 0$. There is, of course, the question of whether $\hat{H}\Psi = 0$ or something like it will still be present in a more satisfactory formulation of quantum gravity. But there is a deeper question. Namely, will we be able to give a Bohm version of that new theory?[17]

If choice one is the right one, and making Ψ a beable means taking configuration space to also be an element of reality, it seems we have three radically different metaphysical pictures open to the Bohmian. There is the *dualistic* theory, according to which both spacetime and configuration space are real. The particles evolve in spacetime, the wave function in configuration space, with the latter "guiding" the former. The problem here, like the problem with Cartesian mind-body dualism, lay in explaining the mysterious interaction between the two quite different domains. The other option is a *monistic* theory. One might argue that either solely configuration space or solely spacetime is real. If only configuration space is real, the world would consist of configuration space and a Bohm "world particle." Space and localized entities would be merely logical constructs from this strange world. The problems with this view are that it is remarkably counter-intuitive and that it seems to leave all the (physical space) symmetries of the Hamiltonian mere coincidences. If only spacetime is real, one would have to figure out a way to write the wave function as a function of 3-space instead of 3n-space. This would implement de Broglie's original interpretation, in which the Ψ-field is conceived of as propagating in physical space. Although there have been some attempts at doing this, e.g., Friestadt, none have been completely successful.[18] In our opinion, if it can be achieved, this is the most desirable option, although a certain amount of pessimism concerning its chances is probably in order.

6. Born's Rule

Unlike in the standard interpretation, the ψ-field in Bohm's theory determines the velocity of the beables *via* the guidance equation, e.g., equation (1). Although defined on configuration space, the ψ-field is a real field, much like the electromagnetic field. Logically speaking, it has nothing to do with the probability of measuring observables with various values. However, we know that on the standard theory the probability density for particle position ρ equals $|\psi|^2$. Bohm's theory can only maintain observational equivalence with the standard theory, then, if it tells us that our measurements of particles will agree with Born's rule. That is, the distribution postulate must hold if Bohm's theory is to be empirically adequate. But why should it? On the usual interpretation this relation is an essential part of the theory, but in Bohm's case, there seems to be no particular reason why it should hold.

Now, of course, one reply to this problem is to simply assert that our world began with initial conditions such that the Bohmian dynamics guarantees that Born's rule will hold approximately for measurement and measurement-like situations. Call these conditions the "Good Set" of initial conditions. That there exist such initial conditions is not controversial. And for the Bohmian, there is no question that the world began in a condition from the Good Set, for only distributions from this set will evolve into those observed in experiment. Supplemented with the Bohmian dynamics, the mere fact that Born's rule seems to work straightforwardly implies that the initial conditions are in the Good Set. Thus, for the Bohmian, there is good reason to believe the world began with the right distribution.

What is lacking, however, is an *explanation* of why it holds. The feeling is that it is just too good to be true that the initial universal wave function and initial particle distribution happened to be such that measurements would agree with Born's rule. The prior probability for this to occur is extremely low. Thus Bohm's theory suffers from a problem not unlike the so-called Horizon Problem in standard big-bang cosmology. The Horizon Problem stems from the (nearly) isotropic cosmic background radiation. In a world with particle horizons, many regions of the universe won't share a common causal past. Roughly speaking, this means a dynamic explanation of the sameness of the radiation can not be found, and consequently, that the isotropy must be explained by appeal to special initial conditions. In cosmological circles this is deemed a problem that the inflationary scenario cures, for allegedly it drives "most" initial conditions to isotropy.[19] The supposed problem that the two theories share is that they both must resort to explaining an observed uniformity by postulating extremely "special" initial conditions. In a word, this just seems cheap, for (miracles aside) one could "explain" anything by appealing to special initial conditions.

When we ask for an explanation of why $\rho = |\psi|^2$ in Bohm's theory, we are essentially asking for reasons to think the distribution postulate is inevitable. This could be achieved by making the initial distribution of particles and initial wave function nomologically necessary. This would be tantamount to stipulating that the initial conditions are explanatory, which would not be dissimilar to the status Born's rule has in the standard theory. Whether this approach is anything more than an *ad hoc* fix depends on deep questions about the nature of lawhood. The inevitability of Born's rule could also follow from probability arguments. God may have picked our world from an urn filled with worlds with the same wave function but particle distributions given by Born's rule. If so, then probably Born's rule holds in our world (given some measure on these worlds). The problem with this approach, of course, is that it is mere fantasy.[20]

A third approach is found in the work of Durr. Goldstein and Zanghi.[21] They claim to show that Born's rule holds for *typical* Bohmian universes. "Typical," the authors emphatically stress, does not mean "probable," so their position should not be conflated with the second approach. They write, "The term 'typical' is used here in its mathematically precise sense: The conclusion [that Born's rule emerges for *typical* Bohmian universes] holds for 'almost every' universe, i.e., with the exception of a set of universes, or initial configurations, that is very small with respect to a certain natural measure . . . on the set of all universes."[22]

What they hope to show is that for *any* given universal wave function, if it is the measure, then typical universal particle configurations Q will be such that Born's rule holds. In what follows, we shall ask whether they successfully demonstrate what they claim to and whether such a demonstration is really of explanatory value or not.

Durr *et al.* begin by assuming the "quantum equilibrium condition" $P = |\Psi(Q)|^2$.[23] Q is the particle configuration of the universe, and Ψ is the universal wave function. It is perhaps best to assume quantum equilibrium is a law of nature on their approach. As a law, it need not itself be explained. Rather it is what will explain $\rho = |\psi|^2$. Here it is relevant to point out their opinion (discussed above) that the distinction between law and initial condition collapses for the initial wave function of the universe. At any rate, understanding what this expression means is somewhat difficult. One could imagine a configuration space of universes, where each point of this space Q represents an initial universal particle configuration. Then one could understand the expression in terms of the probability that a universe has a particle configuration Q. Fortunately, this is not what Durr *et al.* mean. If it were, they could be charged with circularity and a commitment to an ensemble of universes. Rather, $|\Psi(Q)|^2$ is a measure, but it is *not* a probability measure.

Durr *et al.* then assume that Q is distributed randomly. From this they show that if at some time quantum equilibrium holds, then at all times this condition holds. This property, akin to stationarity, is called "equivariance." Now consider some Q at some time t that can be divided into a subsystem X and environment Y, $Q_t = (X_t, Y_t)$. Given some Y_t, the conditional probability of X_t having the generic configuration space values $\tilde{x}$ is $P(\tilde{x}/Y_t) = |\Psi(Q)|^2 = |\psi_t(\tilde{x})|^2$. Here ψ is the Bohmian "effective wave function" (see note 23), which is just the standard wave function of quantum mechanics. Suppose the subsystem of interest itself consists of N identical subsystems, each with the same wave function. Then this conditional probability formula implies that the configurations of each of these subsystems is randomly distributed according to $|\psi(\tilde{x})|^2$. Using the law of large numbers, Durr *et al.* show that as N gets large, the observed distribution of particles becomes overwhelmingly likely to be $\rho = |\psi|^2$, which is just what we wanted. Thus, they claim to have shown that with respect to a natural measure, most of the particle configurations agree with standard quantum mechanics. Therefore, we should not be surprised, even on Bohm's theory, to find that Born's rule holds. We refer readers interested in further details to Durr *et al.*'s paper.

Do Durr *et al.*'s demonstration succeed? Mathematically, we believe it does. However, they seem to equivocate in their argument. $P = |\Psi(Q)|^2$, recall, is not a probability measure. If this initial assumption were a probability measure, it would be a measure over an ensemble of universes, and further, it would open their argument to the charge of begging the question. $P = |\Psi(Q)|^2$ is best understood as a primitive "natural" measure. (Durr *et al.* seem to think its naturalness stems from its equivariance.) Let's call the property corresponding to this measure "ability." If they are to remain consistent, therefore, their argument can only appeal to the assumed measure as an ability measure and not as a probability measure. However, what is confusing is that Durr *et al.* freely help themselves to a notion of probability, not ability. One can see this most clearly when they introduce their conditional probability formula $P(\vec{x}/Yt) = |\Psi(Q)|^2$. (Or maybe even earlier: What does "random" mean when they claim Q is distributed randomly?) What justifies calling this a conditional *probability*, if as we have just seen, $|\Psi(Q)|^2$ is *not* a probability measure? And if $P(\vec{x}/Yt)$ is not a conditional probability, then neither is $|\psi_t(\vec{x})|^2$, and consequently, neither is $|\psi(\vec{x})|^2$. One might also make this sort of objection regarding their use of the law of large numbers. As the number of systems gets large, the law of large numbers implies that it gets ever more *likely* that the observed distributions will agree with quantum mechanics. Where does this probability come from? It is evaluated with respect to the measure $|\Psi(Q)|^2$. If this is interpreted as ability, then the law of large numbers merely implies that the ability function will assign high values to the observed distributions agreeing with quantum mechanics. But what does this mean?

Suppose the authors remove this equivocation and remain faithful to the interpretation of $|\Psi(Q)|^2$ as a measure of ability, not probability. Formally, the demonstration would proceed in the same manner as in the original argument, only now the conditional probability is "conditional ability" and the law of large numbers is reinterpreted as above. We then question whether knowing that typical Bohmian universes agree with quantum mechanics is of explanatory value anyway. Knowing that typical Bohm worlds agree with Born's rule doesn't give one reason to think it's *likely* that the system one is measuring has its probability density given by

Born's rule. Without inspiring such confidence, we wonder how this measure can be explanatory. For example, if the measure were one of beauty, finding out that Born's rule holds for most worlds with respect to the beauty measure hardly explains why Born's rule holds! Yet for all we know about typicality, it has no more and no less explanatory power than does the beauty measure. The problem, in short, is that typicality and largeness with respect to the measure of beauty do not bear the same relationship to our beliefs as probability does.[24]

An approach similar to that of Durr *et al.* is found in recent work by Valentini.[25] Valentini gives an analog of Boltzmann's H-Theorem to show that a coarse-graining of P will approach a coarse-graining of $|\Psi(Q)|^2$, even if P didn't originally equal $|\Psi(Q)|^2$. Intuitively, the idea is that if one thinks of both quantities as fluids, because they obey the same continuity equation, it is as though they are being mixed with the same straw. After a certain amount of time (not specified by the proof) the two fluids should mix and their densities should approach each other's. However, what all this could mean is rather mysterious. $|\Psi(Q)|^2$, remember, is defined on the configuration space of universes. What mixing and coarse-graining mean at this level is not at all clear, at least to us. Nor is the meaning of $|\Psi(Q)|^2$ straightforward, for many of the reasons mentioned above, whether or not $|\Psi(Q)|^2$ initially equals P. Furthermore, the point of such a proof is somewhat obscure, too. It is still the case that Valentini must assume the universe began with initial conditions in the Good Set. He cannot show that one can expect Born's rule to hold for Bohmian worlds with arbitrary initial conditions. With the problem firmly in mind, one sees that mixing arguments do not explain why Born's rule holds, since they do not explain why the universe started in the Good Set.

Both the Valentini and the Durr *et al.* approaches are highly problematic. Is there a better response to the problem of Born's rule? Our opinion is that we should abandon the search for a justification for Born's rule. The world began with initial conditions in the Good Set. That is just the way it is. Nothing more can be said, for the Good Set is not probable (for what could this mean?), and if it is "natural," this is not explanatory. Explaining Born's rule in a deterministic theory like Bohm's means explaining the boundary conditions of the universe, and it is just here that we believe explanation must come to a halt.

In closing, we would like to point out that the Valentini and Durr *et al.* approaches respectively correspond to quite different visions of what a

Bohm world looks like. On Valentini's approach, and Bohm's before him, the fact that one can think of Born's rule as merely contingently holding in a Bohm world is emphasized. On this approach, one assumes the existence of special initial conditions. There are initial conditions in the Good Set that lead to Born's rule holding in our epoch of the universe, but which do not lead to Born's rule holding at some past and future times (or even distant places). Further, there are initial conditions that lead to distributions approximately close to Born's rule, but not exactly close to Born's rule. Thus, in these cases we would have violations of quantum mechanics. On the approach of Durr *et al.* by contrast, Born's rule holds as a matter of law. There are no times at which Born's rule does not hold, and consequently, no times at which Bohmian mechanics will not make the same predictions as standard mechanics. Hence, whether we should expect deviations from quantum mechanics depends on which approach one adopts.

In his discussion of the distribution postulate J. Barrett favors (in our language) the second vision of Bohm's theory over the first.[26] He is not sure the initial conditions needed by the first approach are so much less restrictive than that required by the second approach as to make it more attractive, and he is worried about the possibility of different predictions than standard quantum mechanics. For what it is worth, our opinion is the exact opposite of his. Although we agree with his first observation, we very much like the fact that Bohmian mechanics may diverge from quantum mechanics. In spirit, this seems a much more "Bohmian" result. Bohmian mechanics is not merely a theory cooked up to yield the same predictions of quantum mechanics, it is a new theory in its own right, inspired by very natural physical intuitions. Physics is risky. That Bohm's theory share part of this risk, insofar as it offers new physics, is something we should desire, not something we should fear.[27]

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NOTES

1. Cushing, J. (1994), *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*. Chicago: University of Chicago Press.

2. See A. Stone (1994), "Does the Bohm Theory Solve the Measurement Problem?," *Philosophy of Science* **61**: 250–66, but also T. Maudlin (1995), "Discussion: Why Bohm's Theory Solves the Measurement Problem," *Philosophy of Science* **62**: 479–83.

3. Durr, D., Goldstein, S., and Zanghi, N. (1992), "Quantum Equilibrium and the Origin of Absolute Uncertainty," *Journal of Statistical Physics* **67**: 843–907.

4. Cushing; Durr *et al*; Bohm, D. and Hiley, B. J. (1993), *The Undivided Universe: An Ontological Interpretation of Quantum Theory*. London: Routledge, Chapman and Hall; Albert, D. (1992) *Quantum Mechanics and Experience*. Harvard University Press; Holland, P. (1993) *The Quantum Theory of Motion*. Cambridge University Press.

5. Respectively, Stone; Vink, J. (1993), "Quantum Mechanics in Terms of Discrete Observables," *Physical Review A* **48**: 1808–18.

6. Dickson, M. (1995), "What is Preferred About the Preferred Basis?," *Foundations of Physics* **25**, 3: 423–40.

7. Anandan, J. and Brown, H. R. (1995), "On the Reality of Spacetime Geometry and the Wavefunction" *Foundations of Physics* **25**, n.2, 349.

8. For more on this issue and whether non-locality conflicts with special relativity, see Maudlin's excellent treatment of these issues in T. Maudlin, (1994), *Quantum Non-Locality and Relativity*. Oxford: Blackwell.

9. Bohm, D. and Hiley, p. 57.

10. *Ibid*, ch. 12.

11. Valentini, A. (1992) *On the Pilot-Wave Theory of Classical, Quantum and Sub-quantum Physics*. Unpublished Ph.D. dissertation, ISAS, Trieste, Italy.

12. Bell, J. S. (1987) "Beables for Quantum Field Theory" in *Speakable and Unspeakable in Quantum Mechanics*. Cambridge: Cambridge University Press.

13. The same kind of effect occurs in the particle theory, too, as was pointed out by Englert, B., Scully, M., Sussman, G., and Walther, H. (1992) "Surrealistic Bohm Trajectories" *Z. Naturforsch.* **47a**, 1175. As to whether it is a problem for Bohm's theory, see Durr, D., Fusseder, W., Goldstein, S., and Zanghi, N. (1993) "Comment on 'Surrealistic Bohm Trajectories'" *Z. Naturforsch.* **48a**, 1261.

14. For more, see C. Callender and R. Weingard (1994), "The Bohmian Model of Quantum Cosmology," in Hull, D., Forbes, M. and Burian, R. (eds.) *PSA 1994*, 1, 218–27.

15. Callender, C. and Weingard, R. (1996) "Time, Bohm's Theory, and Quantum Cosmology," forthcoming in *Philosophy of Science*.

16. Durr, D., Goldstein, S., and Zanghi, N. (1995) "Bohmian Mechanics and the Meaning of the Wavefunction" LANL preprint quant-ph/9512031.

17. For more on this concerning string theory, see R. Weingard (1996), "Exotic (Quixotic?) Applications of Bohm Theory" in R. Clifton (ed.), *Perspectives on Quantum Reality*. The Netherlands: Kluwer, pp. 195–210.

18. Freistadt, H. (1957) *Suppl. Nuovo Cimento* **5**: 1–70.

19. For more, see J. Earman (1995) *Bangs, Crunches, Whimpers and Shrieks: Singularities and Acausalities in Relativistic Spacetimes* (Oxford: Oxford University Press).

20. There is another approach that derives this relation by *changing* Bohm's theory. Bohm and Vigier develop an indeterministic version of the theory in which an underlying stochastic field perturbs ρ into $|\psi|^2$ (see Bohm and Hiley for references). We will leave modifications of Bohm's theory to one side and examine what can be done to explain the distribution postulate within the original theory.

21. Durr *et al.* 1992, 1996.

22. Durr, D., Goldstein, S. and Zanghi, N. (1996) "Bohmian Mechanics as the Foundation of Quantum Mechanics," in Cushing, J., Fine, A. and Goldstein, S. *Bohmian Mechanics*

and Quantum Theory: An Appraisal. (Dordrecht, The Netherlands: Kluwer Academic Publishers.)

23. In Bohm's theory subsystems of the universe do not, strictly speaking, have wave functions. There is only the wave function for the universe Ψ. However, what Bohm and others have shown is that in typical measurement situations, *effective* wave functions for subsystems of the universe ψ can be defined. These are simply the wave functions of standard quantum mechanics. In Bohm's theory, we are thus ultimately concerned with showing why $P = |\psi|^2$. See Durr *et al.* 1992.

24. Since this objection also holds regarding the canonical probability distribution in statistical mechanics, we wish to acknowledge that the problem is a deep one. Nevertheless, that the problem appears elsewhere shouldn't attenuate the force of this objection. It may be that we *must* use certain probability measures even if they can not be justified. But we shouldn't be misled into thinking that that fact itself provides a justification for the measure.

25. Valentini, A. (1991), "Signal-locality, Uncertainty, and the Subquantum H-Theorem I," *Physics Letters A* **156**: 5–11.

26. Barrett, J. (1995) "The Distribution Postulate in Bohm's Theory," *Topoi* **14**: 45–54.

27. We would like to thank Barry Loewer and Tim Maudlin for many useful conversations about issues in Bohm's theory.