Bohmian Mechanics as the Foundation of Quantum Mechanics

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In order to arrive at Bohmian mechanics from standard nonrelativistic quantum mechanics one need do almost nothing! One need only complete the usual quantum description in what is really the most obvious way: by simply including the positions of the particles of a quantum system as part of the state description of that system, allowing these positions to evolve in the most natural way. The entire quantum formalism, including the uncertainty principle and quantum randomness, emerges from an analysis of this evolution. This can be expressed succinctly—though in fact not succinctly enough—by declaring that the essential innovation of Bohmian mechanics is the insight that *particles move*!

1 Bohmian Mechanics is Minimal

> 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 1987, 191)

According to orthodox quantum theory, the *complete* description of a system of particles is provided by its wave function. This statement is somewhat problematical: If "particles" is intended with its usual meaning—point-like entities whose most important feature is their position in space—the statement is clearly false, since the complete description would then have to include these positions; otherwise, the statement is, to be charitable, vague. Bohmian mechanics is the theory that emerges when we indeed insist that "particles" means particles.

According to Bohmian mechanics, the complete description or state of an N-particle system is provided by its wave function $\psi(q,t)$, where $q = (\mathbf{q}_1, \dots, \mathbf{q}_N) \in \mathbb{R}^{3N}$, *and* its configuration $Q = (\mathbf{Q}_1, \dots, \mathbf{Q}_N) \in \mathbb{R}^{3N}$, where the $\mathbf{Q}_k$ are the positions of the particles. The wave function, which evolves according to Schrödinger's equation,

$$i\hbar\frac{\partial\psi}{\partial t} = H\psi\,. \tag{1}$$

choreographs the motion of the particles: these evolve according to the equation

$$\frac{d\mathbf{Q}_k}{dt} = \frac{\hbar}{m_k}\frac{\mathrm{Im}(\psi^*\boldsymbol{\nabla}_k\psi)}{\psi^*\psi}(\mathbf{Q}_1, \dots, \mathbf{Q}_N) \tag{2}$$

where $\boldsymbol{\nabla}_k = \partial/\partial\boldsymbol{q}_k$. In eq. (1), H is the usual nonrelativistic Schrödinger Hamiltonian; for spinless particles it is of the form

$$H = -\sum\nolimits_{k=1}^{N}\frac{\hbar^2}{2m_k}\boldsymbol{\nabla}_k^{\,2} + V, \tag{3}$$

containing as parameters the masses $m_1 \ldots, m_N$ of the particles as well as the potential energy function V of the system. For an N-particle system of nonrelativistic particles, equations (1) and (2) form a complete specification of the theory.[1] There is no need, and indeed no room, for any further *axioms*, describing either the behavior of other observables or the effects of measurement.

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.

It is important to bear in mind that regardless of which observable we choose to measure, the result of the measurement can be assumed to be given configurationally, say by some pointer orientation or by a pattern of ink marks on a piece of paper. Then the fact that Bohmian mechanics makes the same predictions as does orthodox quantum theory for the results of any experiment—for example, a measurement of momentum or of a spin component—at least assuming a random distribution for the configuration of the system and apparatus at the beginning of the experiment given by $|\psi(q)|^2$, is a more or less immediate consequence of (2). This is because the quantum continuity equation

$$\frac{\partial|\psi(q,t)|^2}{\partial t} + \operatorname{div} J^{\psi}(q,t) = 0, \tag{4}$$

where

$$J^{\psi}(q,t) = (\mathbf{J}_1^{\psi}(q,t), \ldots, \mathbf{J}_N^{\psi}(q,t)) \tag{5}$$

with

$$\mathbf{J}_k^\psi = \frac{\hbar}{m_k}\mathrm{Im}\,(\psi^* \boldsymbol{\nabla}_k \psi) \tag{6}$$

is the *quantum probability current*, an equation that is a simple consequence of Schrödinger's equation, becomes the classical continuity equation

$$\frac{\partial \rho}{\partial t} + \mathrm{div}\, \rho v = 0 \tag{7}$$

for the system $dQ/dt = v$ defined by (2)—the equation governing the evolution of a probability density ρ under the motion defined by the guiding equation (2)—when $\rho = |\psi|^2 = \psi^*\psi$, the *quantum equilibrium* distribution. In other words, if the probability density for the configuration satisfies $\rho(q, t_0) = |\psi(q, t_0)|^2$ at some time t_0, then the density to which this is carried by the motion (2) at any time t is also given by $\rho(q, t) = |\psi(q, t)|^2$. This is an extremely important property of Bohmian mechanics, one that expresses a certain compatibility between the two equations of motion defining the dynamics, a property which we call the *equivariance* of the probability distribution $|\psi|^2$. (It of course holds for any Bohmian system and not just the system-apparatus composite upon which we have been focusing.)

While the meaning and justification of the *quantum equilibrium hypothesis* that $\rho = |\psi|^2$ is a delicate matter, to which we shall later return, it is important to recognize at this point that, merely as a consequence of (2), Bohmian mechanics is a counterexample to all of the claims to the effect that a deterministic theory cannot account for quantum randomness in the familiar statistical mechanical way, as arising from averaging over ignorance: Bohmian mechanics is clearly a deterministic theory, and, as we have just explained, it does account for quantum randomness as arising from averaging over ignorance given by $|\psi(q)|^2$.

Note 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 (2), a first-order evolution equation for the configuration. In so doing it provides a precise role for the wave function in sharp contrast with its rather obscure status in orthodox quantum theory. Moreover, if we take Schrödinger's equation directly into account—as of course we should if we seek its rational completion—this additional equation emerges in an almost inevitable manner, indeed via several routes. Bell's preference is to observe that the probability current J^ψ and the probability density $\rho = \psi^*\psi$ would classically be related (as they would for any dynamics given by a first-order ordinary differential equation) by $J = \rho v$, obviously suggesting that

$$dQ/dt = v = J/\rho, \tag{8}$$

which is the guiding equation (2).

Bell's route to (2) makes it clear that it does not require great imagination to arrive at the guiding equation. However, it does not show that this equation is in any sense mathematically inevitable. Our own preference is to proceed in a somewhat different manner, avoiding any use, even in the motivation for the theory, of probabilistic notions, which are after all somewhat subtle, and see what symmetry considerations alone might suggest. Assume for simplicity that we are dealing with spinless particles. Then one finds (Dürr, Goldstein and Zanghì 1992, first reference) that, given Schrödinger's equation, the simplest choice, compatible with overall Galilean and time-reversal invariance, for an evolution equation for the configuration, the simplest way a suitable velocity vector can be extracted from the scalar field ψ, is given by

$$\frac{d\mathbf{Q}_k}{dt} = \frac{\hbar}{m_k}\mathrm{Im}\frac{\boldsymbol{\nabla}_k\psi}{\psi}, \tag{9}$$

which is of course equivalent to (2): The $\boldsymbol{\nabla}$ on the right-hand side is suggested by rotation invariance, the ψ in the denominator by homogeneity—i.e., by the fact that the wave function should be understood projectively, an understanding required for the Galilean invariance of Schrödinger's equation alone—and the "Im" by time-reversal invariance, since time-reversal is implemented on ψ by complex conjugation, again as demanded by Schrödinger's equation. The constant in front is precisely what is required for covariance under Galilean boosts.

2 Bohmian Mechanics and Classical Physics

You will no doubt have noticed that the quantum potential, introduced and emphasized by Bohm (Bohm 1952 and Bohm and Hiley 1993)—but repeatedly dismissed, by omission, by Bell (Bell 1987)—did not appear in our formulation of Bohmian mechanics. Bohm, in his seminal (and almost universally ignored!) 1952 hidden-variables paper (Bohm 1952), wrote the wave function ψ in the polar form $\psi = Re^{iS/\hbar}$ where S is real and $R \geq 0$, and then rewrote Schrödinger's equation in terms of these new variables, obtaining a pair of coupled evolution equations: the continuity equation (7) for $\rho = R^2$, which suggests that ρ be interpreted as a probability density, and a modified Hamilton-Jacobi equation for S,

$$\frac{\partial S}{\partial t} + H(\nabla S, q) + U = 0, \tag{10}$$

where $H = H(p, q)$ is the classical Hamiltonian function corresponding to (3), and

$$U = -\sum_k \frac{\hbar^2}{2m_k} \frac{{\boldsymbol{\nabla}_k}^2 R}{R}. \tag{11}$$

Noting that this equation differs from the usual classical Hamilton-Jacobi equation only by the appearance of an extra term, the *quantum potential* U, Bohm then used the equation to define particle trajectories just as is done for the classical Hamilton-Jacobi equation, that is, by identifying ∇S with mv, i.e., by

$$\frac{d\mathbf{Q}_k}{dt} = \frac{\boldsymbol{\nabla}_k S}{m_k}, \tag{12}$$

which is equivalent to (9). 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's rewriting of Schrödinger's equation via variables that seem interpretable in classical terms does not come without a cost. 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" (Bohm 1980, 80)] 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 is suggested by the quantum potential is rather misleading. Bohmian mechanics is not simply 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

$$\mathbf{v}_k = \boldsymbol{\nabla}_k S/m_k. \tag{13}$$

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.

We wish to stress that since the dynamics for Bohmian mechanics is completely defined by Schrödinger's equation together with the guiding equation, 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.

We believe that 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.[2] 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.

We have already indicated why such sentiments are inadequate, but we would like to go further. Bohmian mechanics should be regarded as a first-order theory, in which it is the velocity, the rate of change of position, that is fundamental in that it is this quantity that is specified by the theory, directly and simply, with the second-order (Newtonian) concepts of acceleration and force, work and energy playing no fundamental role. From our perspective the artificiality suggested by the quantum potential is the price one pays if one insists on casting a highly nonclassical theory into a classical mold.

This is not to say that these second-order concepts play *no* role in Bohmian mechanics; they are emergent notions, fundamental to the theory to which Bohmian mechanics converges in the "classical limit," namely, Newtonian mechanics. Moreover, in order most simply to see that Newtonian mechanics should be expected to emerge in this limit, it is convenient to transform the defining equations (1) and (2) of Bohmian mechanics into Bohm's Hamilton-Jacobi form. One then sees that the (size of the) quantum potential provides a rough measure of the deviation of Bohmian mechanics from its classical approximation.

It might be objected that mass is also a second-order concept, one that most definitely does play an important role in the very formulation of Bohmian mechanics. In this regard we would like to make several comments. First of all, the masses appear in the basic equations only in the combination $m_k/\hbar \equiv \mu_k$. Thus eq. (2) could more efficiently be written as

$$\frac{d\mathbf{Q}_k}{dt} = \frac{1}{\mu_k}\frac{\mathrm{Im}(\psi^*\boldsymbol{\nabla}_k\psi)}{\psi^*\psi}, \tag{14}$$

and if we divide Schrödinger's equation by $\hbar$ it assumes the form

$$i\frac{\partial\psi}{\partial t} = -\sum\nolimits_{k=1}^{N}\frac{1}{2\mu_k}\boldsymbol{\nabla}_k^2\psi + \hat{V}\psi, \tag{15}$$

with $\hat{V} = V/\hbar$. Thus it seems more appropriate to regard the naturalized masses μ_k, which in fact have the dimension of [time]/[length]^2, rather than the original masses m_k, as the fundamental parameters of the theory. Notice that if naturalized parameters (including also naturalized versions of the other coupling constants such as the naturalized electric charge $\hat{e} = e/\sqrt{\hbar}$) are used, Planck's constant $\hbar$ disappears from the formulation of this quantum theory. Where $\hbar$ remains is merely in the equations $m_k = \hbar\mu_k$ and $e^2 = \hbar\hat{e}^2$

relating the parameters—the masses and the charges—in the natural microscopic units with those in the natural units for the macroscopic scale, or, more precisely, for the theory, Newtonian mechanics, that emerges on this scale.

It might also be objected that notions such as *inertial* mass and the quantum potential are necessary if Bohmian mechanics is to provide us with any sort of *intuitive* explanation of quantum phenomena, i.e., explanation in familiar terms, presumably such as those involving only the concepts of classical mechanics. (See, for example, the contribution of Baublitz and Shimony to this volume.) 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. It is particularly important to bear these remarks on intuitive explanation in mind when we come to the discussion in the next section of the status of quantum observables, especially spin.

The problem with orthodox quantum theory is not that it is unintuitive. Rather the problem is that

> ...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 1987, 173)

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

3 What about Quantum Observables?

We have argued that quantities such as mass do not have the same meaning in Bohmian mechanics as they do classically. This is not terribly surprising if we bear in mind that the meaning of theoretical entities is ultimately determined by their role in a theory, and thus when there is a drastic change of theory, a change in meaning is almost inevitable. We would now like to argue that with most observables, for example energy and momentum, something much more dramatic occurs: In the transition from classical mechanics they cease to remain properties at all. Observables, such as spin, that have no classical counterpart also should not be regarded as properties of the system. The best way to understand the status of these observables—and to better appreciate the minimality of Bohmian mechanics—is Bohr's way: What are called quantum observables obtain meaning *only* through their association with specific *experiments*. We believe that Bohr's point has not been taken to heart by most physicists, even those who regard themselves as advocates of the Copenhagen interpretation, and that the failure to appreciate this point nourishes a kind of naive realism about operators, an uncritical identification of operators with properties, that is the source of most, if not all, of the continuing confusion concerning the foundations of quantum mechanics.

Information about a system does not spontaneously pop into our heads, or into our (other) "measuring" instruments; rather, it is generated by an *experiment:* some physical interaction between the system of interest and these instruments, which together (if there is more than one) comprise the *apparatus* for the experiment. Moreover, this interaction is defined by, and must be analyzed in terms of, the physical theory governing the behavior

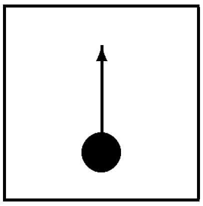

Figure 1: Initial setting of apparatus.

of the composite formed by system and apparatus. If the apparatus is well designed, the experiment should somehow convey significant information about the system. However, we cannot hope to understand the significance of this "information"—for example, the nature of what it is, if anything, that has been measured—without some such theoretical analysis.

Whatever its significance, the information conveyed by the experiment is registered in the apparatus as an *output*, represented, say, by the orientation of a pointer. Moreover, when we speak of an experiment, we have in mind a fairly definite initial state of the apparatus, the ready state, one for which the apparatus should function as intended, and in particular one in which the pointer has some "null" orientation, say as in Figure 1.

For Bohmian mechanics we should expect in general that, as a consequence of the quantum equilibrium hypothesis, the justification of which we shall address in Section 4 and which we shall now simply take as an assumption, the outcome of the experiment—the final pointer orientation—will be random: Even if the system and apparatus initially have definite, known wave functions, so that the outcome is determined by the initial configuration of system and apparatus, this configuration is random, since the composite system is in quantum equilibrium, i.e., the distribution of this configuration is given by $|\Psi(x,y)|^2$, where Ψ is the wave function of the system-apparatus composite and x respectively y is the generic system respectively apparatus configuration. There are, however, special experiments whose outcomes are somewhat less random than we might have thought possible.

In fact, consider a *measurement-like* experiment, one which is *reproducible* in the sense that it will yield the same outcome as originally obtained if it is immediately repeated. (Note that this means that the apparatus must be immediately reset to its ready state,

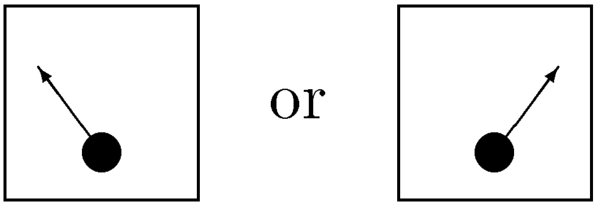

Figure 2: Final apparatus readings.

or a fresh apparatus must be employed, while the system is not tampered with so that its initial state for the repeated experiment is its final state produced by the first experiment.) Suppose that this experiment admits, i.e., that the apparatus is so designed that there are, only a finite (or countable) number of possible outcomes α,[3] for example, $\alpha =$"left" and $\alpha =$"right" as in Figure 2. The experiment also usually comes equipped with a *calibration* λ_α, an assignment of numerical values (or a vector of such values) to the various outcomes α.

It can be shown (Daumer, Dürr, Goldstein and Zanghì 1996), under further simplifying assumptions, that for such reproducible experiments there are special subspaces $\mathcal{H}_\alpha$ of the system Hilbert space $\mathcal{H}$ of (initial) wave functions, which are mutually orthogonal and span the entire system Hilbert space

$$\mathcal{H} = \bigoplus_\alpha \mathcal{H}_\alpha, \tag{16}$$

such that if the system's wave function is initially in $\mathcal{H}_\alpha$, outcome α definitely occurs and the value λ_α is thus definitely obtained. It then follows that for a general initial system wave function

$$\psi = \sum_\alpha \psi_\alpha \equiv \sum_\alpha P_{\mathcal{H}_\alpha}\psi \tag{17}$$

where $P_{\mathcal{H}_\alpha}$ is the projection onto the subspace $\mathcal{H}_\alpha$, the outcome α is obtained with (the usual) probability[4]

$$p_\alpha = \|P_{\mathcal{H}_\alpha}\psi\|^2. \tag{18}$$

In particular, the expected value obtained is

$$\sum_\alpha p_\alpha \lambda_\alpha = \sum_\alpha \lambda_\alpha \|P_{\mathcal{H}_\alpha}\psi\|^2 = \langle \psi, A\psi \rangle \tag{19}$$

where

$$A = \sum_{\alpha} \lambda_\alpha P_{\mathcal{H}_\alpha} \tag{20}$$

and $\langle \cdot , \cdot \rangle$ is the usual inner product:

$$\langle \psi, \phi \rangle = \int \psi^*(x)\, \phi(x)\, dx. \tag{21}$$

What we wish to emphasize here is that, insofar as the statistics for the values which result from the experiment are concerned, the relevant data for the experiment are the collection $(\mathcal{H}_\alpha)$ of special subspaces, together with the corresponding calibration (λ_α), and *this data is compactly expressed and represented by the self-adjoint operator A, on the system Hilbert space $\mathcal{H}$, given by (20).* Thus with a reproducible experiment $\mathcal{E}$ we naturally associate an operator $A = A_{\mathcal{E}}$,

$$\mathcal{E} \mapsto A, \tag{22}$$

a single mathematical object, defined on the system alone, in terms of which an efficient description of the possible results is achieved. If we wish we may speak of operators as observables, but if we do so it is important that we appreciate that in so speaking we merely refer to what we have just sketched: the role of operators in the description of certain experiments.[5]

In particular, so understood the notion of operator-as-observable in no way implies that anything is measured in the experiment, and certainly not the operator itself! In a general experiment no system property is being measured, even if the experiment happens to be measurement-like. Position measurements are of course an important exception. What in general is going on in obtaining outcome α is completely straightforward and in no way suggests, or assigns any substantive meaning to, statements to the effect that, prior to the experiment, observable A somehow had a value λ_α—whether this be in some determinate sense or in the sense of Heisenberg's "potentiality" or some other ill-defined fuzzy sense—which is revealed, or crystallized, by the experiment.[6]

Much of the preceding sketch of the emergence and role of operators as observables in Bohmian mechanics, including of course the von Neumann-type picture of "measurement" at which we arrive, applies as well to orthodox quantum theory.[7] In fact, it would appear that the argument against naive realism about operators provided by such an analysis has even greater force from an orthodox perspective: Given the initial wave function, at least in Bohmian mechanics the outcome of the particular experiment is determined by the initial configuration of system and apparatus, while for orthodox quantum theory there is nothing in the initial state which completely determines the outcome. Indeed, we find it rather surprising that most proponents of the von Neumann analysis of measurement, beginning with von Neumann, nonetheless seem to retain their naive realism about operators. Of course, this is presumably because more urgent matters—the measurement problem and the suggestion of inconsistency and incoherence that it entails—soon force themselves upon one's attention. Moreover such difficulties perhaps make it difficult to maintain much confidence about just what *should* be concluded from the "measurement" analysis, while in Bohmian mechanics, for which no such difficulties arise, what should be concluded is rather obvious.

It might be objected that we are claiming to arrive at the quantum formalism under somewhat unrealistic assumptions, such as, for example, reproducibility. (We note in this regard that many more experiments than those satisfying our assumptions can be associated with operators in exactly the manner we have described.) We agree. But this objection misses the point of the exercise. The quantum formalism itself is an idealization; when applicable at all, it is only as an approximation. Beyond illuminating the role of operators as ingredients in this formalism, our point was to indicate how naturally it emerges. In this regard we must emphasize that the following question arises for quantum orthodoxy, but does not arise for Bohmian mechanics: For precisely which theory is the quantum formalism an idealization?

That the quantum formalism is merely an idealization, rarely directly relevant in practice, is quite clear. For example, in the real world the projection postulate—that when the measurement of an observable yields a specific value, the wave function of the system is replaced by its projection onto the corresponding eigenspace—is rarely satisfied.

More important, a great many significant real-world experiments are simply not at all associated with operators in the usual way. Consider for example an electron with fairly general initial wave function, and surround the electron with a "photographic" plate, away from (the support of the wave function of) the electron, but not too far away. This set-up measures the position of "escape" of the electron from the region surrounded by the plate. Notice that since in general the time of escape is random, it is not at all clear which operator should correspond to the escape position—it should not be the Heisenberg position operator at a specific time, and a Heisenberg position operator at a random time has no meaning. In fact, there is presumably no such operator, so that for the experiment just described the probabilities for the possible results cannot be expressed in the form (18), and in fact are not given by the spectral measure for any operator.

Time measurements, for example escape times or decay times, are particularly embarrassing for the quantum formalism. This subject remains mired in controversy, with various research groups proposing their own favorite candidates for the "time operator" while paying little attention to the proposals of the other groups. For an analysis of time measurements within the framework of Bohmian mechanics, see Daumer, Dürr, Goldstein and Zanghì 1994 and the contribution of Leavens to this volume.

Because of such difficulties, it has been proposed (Davies 1976) that we should go beyond operators-as-observables, to "generalized observables," described by mathematical objects even more abstract than operators. The basis of this generalization lies in the observation that, by the spectral theorem, the concept of self-adjoint operator is completely equivalent to that of (a normalized) projection-valued measure (PV), an orthogonal-projection-valued additive set function, on the value space $\mathbb{R}$. Since orthogonal projections are among the simplest examples of positive operators, a natural generalization of a "quantum observable" is provided by a (normalized) positive-operator-valued measure (POV). (When a POV is sandwiched by a wave function, as on the right-hand side of (19), it generates a probability distribution.)

It may seem that we would regard this development as a step in the wrong direction, since it supplies us with a new, much larger class of abstract mathematical entities about which to be naive realists. But for Bohmian mechanics POV's form an extremely natural class of objects to associate with experiments. In fact, consider a general experiment—beginning, say, at time 0 and ending at time t—with no assumptions about reproducibility or anything else. The experiment will define the following sequence of maps:

$$\psi \mapsto \Psi = \psi \otimes \Phi_0 \mapsto \Psi_t \mapsto d\mu = \Psi_t^* \Psi_t dq \mapsto \tilde{\mu} := \mu \circ F^{-1}$$

Here ψ is the initial wave function of the system, and Φ_0 is the initial wave function of the apparatus; the latter is of course fixed, defined by the experiment. The second map corresponds to the time evolution arising from the interaction of the system and apparatus, which yields the wave function of the composite system after the experiment, with which we associate its quantum equilibrium distribution μ, the distribution of the configuration Q_t of the system and apparatus after the experiment. At the right we arrive at the probability distribution induced by a function F from the configuration space of the composite system to some value space, e.g., $\mathbb{R}$, or $\mathbb{R}^m$, or what have you: $\tilde{\mu}$ is the distribution of $F(Q_t)$. Here F could be completely general, but for application to the results of real-world experiments F might represent the "orientation of the apparatus pointer" or some coarse-graining thereof.

Notice that the composite map defined by this sequence, from wave functions to probability distributions on the value space, is "bilinear" or "quadratic," since the middle map, to the quantum equilibrium distribution, is obviously bilinear, while all the other maps are linear, all but the second trivially so. Now by elementary functional analysis, the notion of such a bilinear map is completely equivalent to that of a POV! Thus the emergence and role of POV's as "generalized observables" in Bohmian mechanics is merely an expression of the bilinearity of quantum equilibrium together with the linearity of Schrödinger's evolution. Thus the fact that with every experiment is associated a POV, which forms a compact expression of the statistics for the possible results, is a near mathematical triviality. It is therefore rather dubious that the occurrence of POV's as observables—the simplest case of which is that of PV's—can be regarded as suggesting any deep truths about reality or about epistemology.

The canonical example of a "quantum measurement" is provided by the Stern-Gerlach experiment. We wish to focus on this example here in order to make our previous considerations more concrete, as well as to present some further considerations about the "reality" of operators-as-observables. We wish in particular to comment on the status of spin. We shall therefore consider a Stern-Gerlach "measurement" of the spin of an electron, even though such an experiment is unphysical (Mott 1929), rather than of the internal angular momentum of a neutral atom.

We must first explain how to incorporate spin into Bohmian mechanics. This is very easy; we need do, in fact, almost nothing: Our derivation of Bohmian mechanics was based in part on rotation invariance, which requires in particular that rotations act on the value space of the wave function. The latter is rather inconspicuous for spinless particles—with complex-valued wave functions, what we have been considering up till now—since rotations then act in a trivial manner on the value space $\mathbb{C}$. The simplest nontrivial (projective) representation of the rotation group is the 2-dimensional, "spin $\frac{1}{2}$" representation; this representation leads to a Bohmian mechanics involving spinor-valued wave functions for a single particle and spinor-tensor-product-valued wave function for many particles. Thus the wave function of a single spin $\frac{1}{2}$ particle has two components

$$\psi(\boldsymbol{q}) = \begin{pmatrix} \psi_1(\boldsymbol{q}) \\ \psi_2(\boldsymbol{q}) \end{pmatrix}, \tag{23}$$

which get mixed under rotations according to the action generated by the Pauli spin matrices $\boldsymbol{\sigma} = (\sigma_x, \sigma_y, \sigma_z)$, which may be taken to be

$$\sigma_x = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix} \quad \sigma_y = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix} \quad \sigma_z = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix} \tag{24}$$

Beyond the fact that the wave function now has a more abstract value space, nothing changes from our previous description: The wave function evolves via (1), where now the Hamiltonian H contains the Pauli term, for a single particle proportional to $\boldsymbol{B} \cdot \boldsymbol{\sigma}$, which represents the coupling between the "spin" and an external magnetic field $\boldsymbol{B}$. The configuration evolves according to (2), with the products of spinors now appearing there understood as spinor-inner-products.

Let's focus now on a Stern-Gerlach "measurement of $A = \sigma_z$." An inhomogeneous magnetic field is established in a neighborhood of the origin, by means of a suitable arrangement of magnets. This magnetic field is oriented more or less in the positive z-direction, and is increasing in this direction. We also assume that the arrangement is invariant under translations in the x-direction, i.e., that the geometry does not depend upon x-coordinate. An electron, with a fairly definite momentum, is directed towards the origin along the negative y-axis. Its passage through the inhomogeneous field generates a vertical deflection of its wave function away from the y-axis, which for Bohmian mechanics leads to a similar deflection of the electron's trajectory. If its wave function ψ were initially an eigenstate of σ_z of eigenvalue 1 (-1), i.e., if it were of the form[8]

$$\psi = |\uparrow\rangle \otimes \phi_0 \quad (\psi = |\downarrow\rangle \otimes \phi_0) \tag{25}$$

where

$$|\uparrow\rangle = \begin{pmatrix} 1 \\ 0 \end{pmatrix} \quad \text{and} \quad |\downarrow\rangle = \begin{pmatrix} 0 \\ 1 \end{pmatrix}, \tag{26}$$

then the deflection would be in the positive (negative) z-direction (by a rather definite angle). For a more general initial wave function, passage through the magnetic field will, by linearity, split the wave function into an upward-deflected piece (proportional to $|\uparrow\rangle$) and a downward-deflected piece (proportional to $|\downarrow\rangle$), with corresponding deflections of the possible trajectories.

The outcome is registered by detectors placed in the way of these two "beams." Thus of the four kinematically possible outcomes ("pointer positions") the occurrence of no detection defines the null output, simultaneous detection is irrelevant (since it does not occur if the experiment is performed one particle at a time), and the two relevant outcomes correspond to registration by either the upper or the lower detector. Thus the calibration for a measurement of σ_z is $\lambda_{\text{up}} = 1$ and $\lambda_{\text{down}} = -1$ (while for a measurement of the z-component of the spin angular momentum itself the calibration is the product of what we have just described by $\frac{1}{2}\hbar$).

Note that one can completely understand what's going on in this Stern-Gerlach experiment without invoking any additional property of the electron, e.g., its *actual* z-component of spin that is revealed in the experiment. For a general initial wave function there is no such property; what is more, the transparency of the analysis of this experiment makes it clear that there is nothing the least bit remarkable (or for that matter "nonclassical") about the *nonexistence* of this property. As we emphasized earlier, it is naive realism about operators, and the consequent failure to pay attention to the role of operators as observables, i.e., to precisely what we should mean when we speak of measuring operator-observables, that creates an impression of quantum peculiarity.

Bell has said that (for Bohmian mechanics) spin is not real. Perhaps he should better have said: *"Even* spin is not real," not merely because of all observables, it is spin which is generally regarded as quantum mechanically most paradigmatic, but also because spin is treated in orthodox quantum theory very much like position, as a "degree of freedom"—a discrete index which supplements the continuous degrees of freedom corresponding to position—in the wave function. Be that as it may, his basic meaning is, we believe, this: Unlike position, spin is not *primitive*,[9] i.e., no *actual* discrete degrees of freedom, analogous to the *actual* positions of the particles, are added to the state description in order to deal with "particles with spin." Roughly speaking, spin is *merely* in the wave function. At the same time, as just said, "spin measurements" are completely clear, and merely reflect the way spinor wave functions are incorporated into a description of the motion of configurations.

It might be objected that while spin may not be primitive, so that the result of our "spin measurement" will not reflect any initial primitive property of the system, nonetheless this result *is* determined by the initial configuration of the system, i.e., by the position of our electron, together with its initial wave function, and as such—as a function $X_{\sigma_z}(\boldsymbol{q},\psi)$ of the state of the system—it is some property of the system and in particular it is surely real. Concerning this, several comments.

The function $X_{\sigma_z}(\boldsymbol{q},\psi)$, or better the property it represents, is (except for rather special choices of ψ) an extremely complicated function of its arguments; it is not "natural," not a "natural kind": It is not something in which, in its own right, we should be at all interested, apart from its relationship to the *result* of this particular experiment.

Be that as it may, it is not even possible to identify this function $X_{\sigma_z}(\boldsymbol{q}, \psi)$ with the measured spin component, since different experimental setups for "measuring the spin component" may lead to entirely different functions. In other words $X_{\sigma_z}(\boldsymbol{q}, \psi)$ is an abuse of notation, since the function X should be labeled, not by σ_z, but by the particular experiment for "measuring σ_z".

For example (Albert 1992, 153), if ψ and the magnetic field have sufficient reflection symmetry with respect to a plane between the poles of our SG magnet, and if the magnetic field is reversed, then the sign of what we have called $X_{\sigma_z}(\boldsymbol{q}, \psi)$ will be reversed: for both orientations of the magnetic field the electron cannot cross the plane of symmetry and hence if initially above respectively below the symmetry plane it remains above respectively below it. But when the field is reversed so must be the calibration, and what we have denoted by $X_{\sigma_z}(\boldsymbol{q}, \psi)$ changes sign with this change in experiment. (The change in experiment proposed by Albert is that "the *hardness box* is *flipped over*." However, with regard to spin this change will produce essentially no change in X at all. To obtain the reversal of sign, either the polarity or the geometry of the SG magnet must be reversed, but not both.)

In general X_A does not exist, i.e., $X_{\mathcal{E}}$, the result of the experiment $\mathcal{E}$, in general depends upon $\mathcal{E}$ and not just upon $A = A_{\mathcal{E}}$, the operator associated with $\mathcal{E}$. In foundations of quantum mechanics circles this situation is referred to as *contextuality,* but we believe that this terminology, while quite appropriate, somehow fails to convey with sufficient force the rather definitive character of what it entails: Properties which are merely contextual are not properties at all; they do not exist, and their failure to do so is in the strongest sense possible! We thus believe that contextuality reflects little more than the rather obvious observation that the result of an experiment should depend upon how it is performed!

4 The Quantum Equilibrium Hypothesis

The predictions of Bohmian mechanics for the results of a quantum experiment involving a system-apparatus composite having wave function ψ are precisely those of the quantum formalism, and moreover the quantum formalism of operators as observables emerges naturally and simply from Bohmian mechanics as the very expression of its empirical import, *provided* it is assumed that prior to the experiment the configuration of the system-apparatus composite is random, with distribution given by $\rho = |\psi|^2$. But how, in this deterministic theory, does randomness enter? What is special about $\rho = |\psi|^2$? What exactly does $\rho = |\psi|^2$ mean—to precisely which ensemble does this probability distribution refer? And why should $\rho = |\psi|^2$ be true?

We have already said that what is special about the quantum equilibrium distribution $\rho = |\psi|^2$ is that it is equivariant [see below eq. (7)], a notion extending that of stationarity to the Bohmian dynamics (2), which is in general explicitly time-dependent. It is tempting when trying to justify the use of a particular "stationary" probability distribution μ for a dynamical system, such as the quantum equilibrium distribution for Bohmian mechanics, to argue that this distribution has a dynamical origin in the sense that even if the initial distribution μ_0 were different from μ, the dynamics generates a distribution μ_t which changes with time in such a way that μ_t approaches μ as t approaches ∞ (and that μ_t is approximately equal to μ for t of the order of a "relaxation time"). Such 'convergence to equilibrium' results—associated with the notions of 'mixing' and 'chaos'—are mathematically quite interesting. However, they are also usually very difficult to establish, even for rather simple and, indeed, artificially simplified dynamical systems. One of the nicest and earliest results along these lines, though for a rather special Bohmian model, is due to Bohm (Bohm 1953).[10]

However, the justification of the quantum equilibrium hypothesis is a problem that by its very nature can be adequately addressed only on the universal level. To better appreciate this point, one should perhaps reflect upon the fact that the same thing is true for the related problem of understanding the origin of thermodynamic nonequilibrium (!)

and irreversibility. As Feynman has said (Feynman, Leighton and Sands 1963, 46–8),

> Another delight of our subject of physics is that even simple and idealized things, like the ratchet and pawl, work only because they are part of the universe. The ratchet and pawl works in only one direction because it has some ultimate contact with the rest of the universe. ...its one-way behavior is tied to the one-way behavior of the entire universe.

An argument establishing the convergence to quantum equilibrium for local systems, if it is not part of an argument explaining universal quantum equilibrium, would leave open the possibility that conditions of local equilibrium would tend to be overwhelmed, on the occasions when they do briefly obtain, by interactions with an ambient universal nonequilibrium. In fact, this is precisely what does happen with thermodynamic equilibrium. In this regard, it is important to bear in mind that while we of course live in a universe that is not in universal thermodynamic equilibrium, a fact that is crucial to everything we experience, all available evidence supports universal quantum equilibrium. Were this not so, we should expect to be able to achieve violations of the quantum formalism—even for small systems. Indeed, we might expect the violations of universal quantum equilibrium to be as conspicuous as those of thermodynamic equilibrium.

Moreover, there are some crucial subtleties here, which we can begin to appreciate by first asking the question: Which systems should be governed by Bohmian mechanics? The systems which we normally consider are subsystems of a larger system—for example, the universe—whose behavior (the behavior of the whole) determines the behavior of its subsystems (the behavior of the parts). Thus for a Bohmian universe, it is only the universe itself which a priori—i.e., without further analysis—can be said to be governed by Bohmian mechanics.

So let's consider such a universe. Our first difficulty immediately emerges: In practice $\rho = |\psi|^2$ is applied to (small) subsystems. But only the universe has been assigned a wave function, which we shall denote by Ψ. What is meant then by the right hand side of $\rho = |\psi|^2$, i.e., by the wave function of a subsystem?

Fix an initial wave function Ψ_0 for this universe. Then since the Bohmian evolution is completely deterministic, once the initial configuration Q of this universe is also specified, all future events, including of course the results of measurements, are determined. Now let X be some subsystem variable—say the configuration of the subsystem at some time t—which we would like to be governed by $\rho = |\psi|^2$. How can this possibly be, when there is nothing at all random about X?

Of course, if we allow the initial universal configuration Q to be random, distributed according to the quantum equilibrium distribution $|\Psi_0(Q)|^2$, it follows from equivariance that the universal configuration Q_t at later times will also be random, with distribution given by $|\Psi_t|^2$, from which you might well imagine that it follows that any variable of interest, e.g., X, has the "right" distribution. But even if this were so (and it is), it would be devoid of physical significance! As Einstein has emphasized (Einstein 1953) "Nature as a whole can only be viewed as an individual system, existing only once, and not as a collection of systems."[11]

While Einstein's point is almost universally accepted among physicists, it is also very often ignored, even by the same physicists. We therefore elaborate: What possible physical significance can be assigned to an ensemble of universes, when we have but one universe at our disposal, the one in which we happen to reside? We cannot perform the *very same* experiment more than once. But we can perform many similar experiments, differing, however, at the very least, by location or time. In other words, insofar as the use of probability in physics is concerned, what is relevant is not sampling across an ensemble of universes, but sampling across space and time within a single universe. What is relevant is empirical distributions—actual relative frequencies for an ensemble of actual events.

At the expense belaboring the obvious, we stress that in order to understand why our universe should be expected to be in quantum equilibrium, it would not be *sufficient* to establish convergence to the universal quantum equilibrium *distribution*, even were it possible to do so. One simple consequence of our discussion is that proofs of convergence to equilibrium for the configuration of the universe would be of rather dubious physical significance: What good does it do to show that an initial distribution converges to some 'equilibrium distribution' if we can attach no relevant physical significance to the notion of a universe whose configuration is randomly distributed according to this distribution? In view of the implausibility of ever obtaining such a result, we are fortunate that it is also *unnecessary* (Dürr, Goldstein and Zanghì 1992), as we shall now explain.

Two problems must be addressed, that of the meaning of the wave function ψ of a subsystem and that of randomness. It turns out that once we come to grips with the first problem, the question of randomness almost answers itself. We obtain just what we want—that $\rho = |\psi|^2$ in the sense of empirical distributions; we find (Dürr, Goldstein and Zanghì 1992) that in a *typical* Bohmian universe an appearance of randomness emerges, precisely as described by the quantum formalism.

The term "typical" is used here in its mathematically precise sense: The conclusion 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, namely the universal quantum equilibrium distribution—the equivariant distribution for the universal Bohmian mechanics—on the set of all universes. It is important to realize that this guarantees that it holds for many particular universes—the overwhelming majority with respect to the only natural measure at hand—one of which might be ours.[12]

Before proceeding to a sketch of our analysis, we would like to give a simple example. Roughly speaking, what we wish to establish is analogous to the assertion, following from the law of large numbers, that the *relative frequency* of appearance of any particular digit in the decimal expansion of a *typical* number in the interval $[0, 1]$ is $\frac{1}{10}$. In this statement two related notions appear: typicality, referring to an a priori measure, here the Lebesgue measure, and relative frequency, referring to structural patterns in an individual object.

It might be objected that unlike the Lebesgue measure on $[0, 1]$, the universal quantum equilibrium measure will not in general be uniform. Concerning this, a comment: The uniform distribution—the Lebesgue measure on $\mathbb{R}^{3N}$—has no special significance for the dynamical system defined by Bohmian mechanics. In particular, since the uniform distribution is not equivariant, typicality defined in terms of this distribution would depend critically on a somewhat arbitrary choice of initial time, which is clearly unacceptable. The sense of typicality defined by the universal quantum equilibrium measure is independent of any choice of initial time.

Given a subsystem, the x-system, with generic configuration x, we may write, for the generic configuration of the universe, $q = (x, y)$ where y is the generic configuration of the environment of the x-system. Similarly, we have $Q_t = (X_t, Y_t)$ for the actual configurations at time t. Clearly the simplest possibility for the wave function of the x-system, the simplest function of x which can sensibly be constructed from the actual state of the universe at time t (given by Q_t and Ψ_t), is

$$\psi_t(x) = \Psi_t(x, Y_t), \tag{27}$$

the *conditional wave function* of the x-system at time t. This is almost all we need, almost but not quite.[13]

The conditional wave function is not quite the right notion for the *effective wave function* of a subsystem (see below; see also Dürr, Goldstein and Zanghì 1992), since it will not in general evolve according to Schrödinger's equation even when the system is isolated from its environment. However, whenever the effective wave function exists it agrees with the conditional wave function. Note, incidentally, that in an after-measurement situation, with a system-apparatus wave function as in note 3, we are confronted with the measurement problem if this wave function is the complete description of the composite system after the measurement, whereas for Bohmian mechanics, with the outcome of the measurement embodied in the configuration of the environment of the measured system, say in the orientation of a pointer on the apparatus, it is this configuration which, when inserted in (27), selects the term in the after-measurement macroscopic superposition that we speak of as defining the "collapsed" system wave function produced by the measurement. Moreover, if we reflect upon the structure of this superposition, we are directly led to the notion of effective wave function (Dürr, Goldstein and Zanghì 1992).

Suppose that

$$\Psi_t(x, y) = \psi_t(x)\Phi_t(y) + \Psi_t^{\perp}(x, y), \tag{28}$$

where Φ_t and $\Psi_t^{\perp}$ have macroscopically disjoint y-supports. If

$$Y_t \in \operatorname{supp} \Phi_t \tag{29}$$

we say that ψ_t is the *effective wave function* of the x-system at time t. Note that it follows from (29) that $\Psi_t(x, Y_t) = \psi_t(x)\Phi_t(Y_t)$, so that the effective wave function is unambiguous, and indeed agrees with the conditional wave function up to an irrelevant constant factor.

We remark that it is the relative stability of the macroscopic disjointness employed in the definition of the effective wave function, arising from what are nowadays often called mechanisms of decoherence—the destruction of the coherent spreading of the wave function due to dissipation, the effectively irreversible flow of "phase information" into the (macroscopic) environment—which accounts for the fact that the effective wave function of a system obeys Schrödinger's equation for the system alone whenever this system is isolated. One of the best descriptions of the mechanisms of decoherence, though not the word itself, can be found in Bohm's 1952 "hidden variables" paper (Bohm 1952). We wish to emphasize, however, that while decoherence plays a crucial role in the very formulation of the various interpretations of quantum theory loosely called decoherence theories, its role in Bohmian mechanics is of a quite different character: For Bohmian mechanics decoherence is purely phenomenological—it plays no role whatsoever in the formulation (or interpretation) of the theory itself.[14]

An immediate consequence (Dürr, Goldstein and Zanghì 1992) of (27) is the *fundamental conditional probability formula*:

$$\mathbb{P}(X_t \in dx \mid Y_t) = |\psi_t(x)|^2 \, dx, \tag{30}$$

where $\mathbb{P}(dQ) = |\Psi_0(Q)|^2 \, dQ$.

Now suppose that at time t the x-system consists itself of many identical subsystems $x_1, \ldots, x_M$, each one having effective wave function ψ (with respect to coordinates relative to suitable frames). Then (Dürr, Goldstein and Zanghì 1992) the effective wave function of the x-system is the product wave function

$$\psi_t(x) = \psi(x_1) \cdots \psi(x_M). \tag{31}$$

Note that it follows from (30) and (31) that the configurations of these subsystems are independent identically distributed random variables with respect to the initial universal quantum equilibrium distribution $\mathbb{P}$ conditioned on the environment of these subsystems. Thus the law of large numbers can be applied to conclude that the empirical distribution of the configurations $X_1, \ldots, X_M$ of the subsystems will typically be $|\psi(x)|^2$—as demanded by the quantum formalism. For example, if $|\psi|^2$ assigns equal probability to the events "left" and "right," typically about half of our subsystems will have configurations belonging to "left" and half to "right." Moreover (Dürr, Goldstein and Zanghì 1992), this conclusion applies as well to a collection of systems at possibly different times as to the equal-time situation described here.[15]

It also follows (Dürr, Goldstein and Zanghì 1992) from the formula (30) that a typical universe embodies *absolute uncertainty*: the impossibility of obtaining more information about the present configuration of a system than what is expressed by the quantum equilibrium hypothesis. In this way, ironically, Bohmian mechanics may be regarded as providing a sharp foundation for and elucidation of Heisenberg's uncertainty principle.

5 What is a Bohmian Theory?

Bohmian mechanics, the theory defined by eqs.(1) and (2), is not Lorentz invariant, since (1) is a nonrelativistic equation, and, more importantly, since the right hand side of (2) involves the positions of the particles at a common (absolute) time. It is also frequently asserted that Bohmian mechanics cannot be made Lorentz invariant, by which it is presumably meant that no Bohmian theory—no theory that could be regarded somehow as a natural extension of Bohmian mechanics—can be found that is Lorentz invariant. The main reason for this belief is the manifest nonlocality of Bohmian mechanics (Bell 1987). It must be stressed, however, that nonlocality has turned out to be a fact of nature: nonlocality must be a feature of any physical theory accounting for the observed violations of Bell's inequality. (See Bell 1987 and the contributions of Maudlin and Squires to this volume.)

A serious difficulty with the assertion that Bohmian mechanics cannot be made Lorentz invariant is that what it actually means is not at all clear, since this depends upon what is to be understood by a Bohmian theory. Concerning this there is surely no uniformity of opinion, but what we mean by a *Bohmian theory* is the following:

1) A Bohmian theory should be based upon a clear ontology, the primitive ontology, corresponding roughly to Bell's local beables. This primitive ontology is what the theory is fundamentally about. For the nonrelativistic theory that we have been discussing, the primitive ontology is given by particles described by their positions, but we see no compelling reason to insist upon this ontology for a relativistic extension of Bohmian mechanics.

Indeed, the most obvious ontology for a bosonic field theory is a field ontology, suggested by the fact that in standard quantum theory, it is the field configurations of a bosonic field theory that plays the role analogous to that of the particle configurations in the particle theory. However, we should not insist upon the field ontology either. Indeed, Bell (Bell 1987, 173–180) has proposed a Bohmian model for a quantum field theory involving both bosonic and fermionic quantum fields in which the primitive ontology is associated only with fermions—with no local beables, neither fields nor particles, associated with the bosonic quantum fields. Squires (contribution in this volume) has made a similar proposal.

While we insist that a Bohmian theory be based upon some clear ontology, we have no idea what the appropriate ontology for relativistic physics actually is.

2) There should be a quantum state, a wave function, that evolves according to the unitary quantum evolution and whose role is to somehow generate the motion for the variables describing the primitive ontology.

3) The predictions should agree (at least approximately) with those of orthodox quantum theory—at least to the extent that the latter are unambiguous.

This description of what a Bohmian theory should involve is admittedly vague, but greater precision would be inappropriate. But note that, vague as it is, this characterization clearly separates a Bohmian theory from an orthodox quantum theory as well as from the other leading alternatives to Copenhagen orthodoxy: The first condition is not satisfied by the decoherent or consistent histories (Griffiths 1984, Omnès 1988, Gell-Mann and Hartle 1993) formulations while with the spontaneous localization theories the second condition is deliberately abandoned (Ghirardi, Rimini and Weber 1986 and Ghirardi, Pearle and Rimini 1990). With regard to the third condition, we are aware that it is not at all clear what should be meant by even an orthodox theory of quantum cosmology or gravity, let alone a Bohmian one. Nonetheless, this condition places strong constraints on the form of the guiding equation.

Furthermore, we do not wish to suggest here that the ultimate theory is likely to be a Bohmian theory, though we do think it very likely that if the ultimate theory is a quantum theory it will in fact be a Bohmian theory.

Understood in this way, a Bohmian theory is merely a quantum theory with a coherent ontology. If we believe that ours is a quantum world, does this seem like too much to demand? We see no reason why there can be no Lorentz invariant Bohmian theory. But if this should turn out to be impossible, it seems to us that we would be wiser to abandon Lorentz invariance before abandoning our demand for a coherent ontology.

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Notes

[1] When a magnetic field is present, the gradients in the equations must be understood as the covariant derivatives involving the vector potential. If ψ is spinor-valued, the bilinear forms appearing in the numerator and denominator of (2) should be understood as spinor-inner-products. (See the discussion of spin in Section 3.) For indistinguishable

particles, it follows from a careful analysis (Dürr, Goldstein and Zanghì 1996) of the natural configuration space, which will no longer be $\mathbb{R}^{3N}$, that when the wave function is represented in the usual way, as a function on $\mathbb{R}^{3N}$, it must be either symmetric or antisymmetric under permutations of the labeled position variables. Note in this regard that according to orthodox quantum mechanics, the very notion of indistinguishable particles seems to be grounded on the nonexistence of particle trajectories. It is thus worth emphasizing that with Bohmian mechanics the classification of particles as bosons or fermions emerges naturally from the very existence of trajectories.

[2] The contortions required to deal with spin in the spirit of the quantum potential are particularly striking (Bohm and Hiley 1993, Holland 1993).

[3] This is really no assumption at all, since the outcome should ultimately be converted to digital form, whatever its initial representation may be.

[4] In the simplest such situation the unitary evolution for the wave function of the composite system carries the initial wave function $\Psi_i = \psi \otimes \Phi_0$ to the final wave function $\Psi_f = \sum_\alpha \psi_\alpha \otimes \Phi_\alpha$, where Φ_0 is the ready apparatus wave function, and Φ_α is the apparatus wave function corresponding to outcome α. Then integrating $|\Psi_f|^2$ over $\operatorname{supp}\Phi_\alpha$, we immediately arrive at (18).

[5] Operators as observables also naturally convey information about the system's wave function after the experiment. For example, for an ideal measurement, when the outcome is α the wave function of the system after the experiment is (proportional to) $P_{\mathcal{H}_\alpha}\psi$. We shall touch briefly upon this collapse of the wave function, i.e., the projection postulate, in Section 4, in connection with the notion of the effective wave function of a system.

[6] Even speaking of the observable A as having value λ_α when the system's wave function is in $\mathcal{H}_\alpha$, i.e., when this wave function is an eigenstate of A of eigenvalue λ_α, insofar as it suggests that something peculiarly quantum is going on when the wave function is not an eigenstate whereas in fact there is nothing the least bit peculiar about the situation, perhaps does more harm than good.

[7] It also applies to the spontaneous collapse models (Ghirardi, Rimini and Weber 1986 and Ghirardi, Pearle and Rimini 1990), the interpretation of which (see, e.g., Albert

1992, 92–111) is often marred by naive realism about operators. See, however, Bell's presentation of GRW (Bell 1987, 205) for an illuminating exception, as well as Ghirardi, Grassi and Benatti 1995 and the contribution of Ghirardi and Grassi to this volume.

8 Here we use the usual notation $\begin{pmatrix} a \\ b \end{pmatrix} \otimes \phi_0$ for $\begin{pmatrix} a\phi_0 \\ b\phi_0 \end{pmatrix}$.

9 We should probably distinguish two senses of "primitive": i) the *strongly primitive* variables, which describe what the theory is fundamentally *about*, and ii) the *weakly primitive* variables, the basic variables of the theory, those which define the complete state description. The latter may either in fact be strongly primitive, or, like the electromagnetic field in classical electrodynamics, they may be required in order to express the laws which govern the behavior of the strongly primitive variables in a simple and natural way. While this probably does not go far enough—we should further distinguish those weakly primitive variables which, like the velocity, are functions of the trajectory of the strongly primitive variables, and those, again like the electromagnetic field, which are not—these details are not relevant to our present purposes, so we shall ignore these distinctions.

10 As an illustration of the pitfalls in trying to establish convergence to quantum equilibrium, a recent attempt of Valentini (Valentini 1991) is instructive. Valentini's argument is based on a "subquantum H-theorem," $d\bar{H}/dt \leq 0$, that is too weak to be of any relevance (since, for example, the inequality is not strict). The H-theorem is itself not correctly proven—it could not be since it is in general false. Even were the H-theorem true, correctly proven, and potentially relevant, the argument given would still be circular, since in proceeding from the H-theorem to the desired conclusion, Valentini finds it necessary to invoke "assumptions similar to those of classical statistical mechanics," namely that (Valentini 1992, 36) "the system is 'sufficiently chaotic'," which more or less amounts to assuming the very mixing which was to be derived.

11 For a rather explicit example of the failure to appreciate this point, see Albert 1992, 144: "And the statistical postulate ... can be construed as stipulating something about the *initial conditions* of the universe; it can be construed (in the fairy-tale language, say) as stipulating that what God did when the universe was created was first to choose a wave

function for it and sprinkle all of the particles into space in accordance with the quantum-mechanical probabilities, and then to leave everything alone, forever after, to evolve deterministically. And note that just as the one-particle postulate can be derived ... from the two-particle postulate, *all* of the more specialized statistical postulates will turn out to be similarly derivable from *this* one." (Note that an initial sprinkling in accordance with the quantum-mechanical probabilities need remain so only if "quantum-mechanical probabilities" is understood as referring to the quantum equilibrium distribution for the configuration of the entire universe rather than to empirical distributions for subsystems arising from this configuration. Note also that the analogy with the relationship between the one-particle and the two-particle postulates also requires that the universal "statistical postulate" be understood in this way.)

[12] It is important to realize that an appeal to typicality is unavoidable if we are to explain why the universe is at present in quantum equilibrium. This is because our analysis also demonstrates that there is a set B of initial configurations, a set of nonvanishing Lebesgue measure, that evolve to present configurations violating the quantum equilibrium hypothesis and hence the quantum formalism. This set cannot be wished away by any sort of mixing argument. Indeed, if, as is expected, mixing holds on the universal level, then this set B should be so convoluted as to be indescribable without a specific reference to the universal dynamics and hence cannot be dismissed as unphysical without circularity.

[13] For particles with spin, (27) should be replaced by $\Psi_t(x, Y_t) = \psi_t(x) \otimes \Phi_t(Y_t)$. In particular, for particles with spin, not every subsystem has a conditional wave function.

[14] However, decoherence is important for a serious discussion of the emergence of Newtonian mechanics as the description of the macroscopic regime for Bohmian mechanics, leading to the picture of a macroscopic Bohmian particle, in the classical regime, guided by a macroscopically well-localized wave packet with a macroscopically sharp momentum moving along a classical trajectory. It may, indeed, seem somewhat paradoxical that the gross features of our world should appear classical because of interaction with the environment and the resulting wave function entanglement (Joos and Zeh 1985, Gell-Mann

and Hartle 1993), the characteristic quantum innovation (Schrödinger 1935).

[15] It should not be necessary to say that we do not claim to have established the impossibility—but rather the atypicality—of quantum nonequilibrium. On the contrary, as we have suggested in the first reference of Dürr, Goldstein and Zanghì 1992, 904, "the reader may wish to explore quantum nonequilibrium. What sort of behavior would emerge in a universe which is in quantum nonequilibrium?" Concerning this, we wish to note that despite what is suggested by the misuse of ensembles for the universe as a whole and the identification of the physical universal convergence of configurations to those characteristic of quantum equilibrium with the expected convergence of universal measures, of $P \to |\Psi|^2$, quantum equilibrium is not an attractor, and no "force" pushes the universal configuration to one of quantum equilibrium. Rather, any transition from quantum nonequilibrium to quantum equilibrium would be entropic and time-symmetric—driven indeed primarily by measure-theoretic effects, by the fact that the set of quantum equilibrium configurations is vastly larger than the set of configurations corresponding to quantum nonequilibrium—just as is the convergence to thermodynamic equilibrium. (For some speculations on the possible value of quantum nonequilibrium, see the contribution of Valentini to this volume.)

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