QUANTUM PHYSICS WITHOUT QUANTUM PHILOSOPHY

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Abstract. Quantum philosophy, a peculiar twentieth century malady, is responsible for most of the conceptual muddle plaguing the foundations of quantum physics. When this philosophy is eschewed, one naturally arrives at Bohmian mechanics, which is what emerges from Schrödinger's equation for a nonrelativistic system of particles when we merely insist that "particles" means particles. While distinctly non-Newtonian, Bohmian mechanics is a fully deterministic theory of particles in motion, a motion choreographed by the wave function. The quantum formalism emerges when measurement situations are analyzed according to this theory. When the quantum formalism is regarded as arising in this way, the paradoxes and perplexities so often associated with quantum theory simply evaporate.

> Bohr's ... approach to atomic problems ... is really remarkable. He is completely convinced that any understanding in the usual sense of the word is impossible. Therefore the conversation is almost immediately driven into philosophical questions, and soon you no longer know whether you really take the position he is attacking, or whether you really must attack the position he is defending. (Schrödinger, letter to Wien.[?])

We begin by briefly explaining the title. Concerning "quantum physics" little need be said. We have in mind all of nonrelativistic quantum mechanics. For the sake of concreteness, however, we should, perhaps, be thinking about quantum interference, as displayed in the two-slit experiment—or electron diffraction. There is no need to describe these in detail. Let us just remind you of some salient features:

At low intensity, "electrons" are detected arriving at a screen ("photographic plate") one spot at a time, at random positions along the plate; these spots accumulate to form

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an interference pattern when both slits are open. If only one slit is open, there will be no interference pattern.

As far as quantum philosophy is concerned, we have in mind a wide assortment of peculiar assertions. Some examples:

> Quantum theory shows us where classical logic goes awry.... It requires radically new ways of thinking. (W. Thirring[?])

Referring to electron diffraction Landau and Lifshitz[?] say:

> It is clear that this result can in no way be reconciled with the idea that electrons move in paths.... In quantum mechanics there is no such concept as the path of a particle.

We have in mind also, perhaps primarily, the constant appeal to the observer, to "observables" rather than to objective real-world events, to "measurements." For example, according to Heisenberg[?]

> the idea of an objective real world whose smallest parts exist objectively in the same sense as stones or trees exist, independently of whether or not we observe them... is impossible....

Many prominent physicists have been disturbed by quantum philosophy. Here are some of their responses:

Referring to the prominence of measurement in orthodox quantum theory, as well as to the peculiar abrogation of the Schrödinger evolution when a measurement occurs, and the resulting (random) collapse (jump) of the wave function, John Stewart Bell[?] has said:

> It would seem that the theory is exclusively concerned about "results of measurement", and has nothing to say about anything else. What exactly qualifies some physical systems to play the role of "measurer"? Was the wavefunction of the world waiting to jump for thousands of millions of years until a single-celled living creature appeared? Or did it have to wait a little longer, for some better qualified system... with a Ph.D.? If the theory is to apply to anything but highly idealized laboratory operations, are we not obliged to admit that more or less "measurement-like" processes are going on more or less all the time, more or less everywhere. Do we not have jumping then all the time?

According to Schrödinger[?]

> [Complementarity is a] thoughtless slogan. ... If I were not thoroughly convinced that the man [Bohr] is honest and really believes in the relevance of his—I do not say theory but—sounding word, I should call it intellectually wicked.

A somewhat more constructive response (Einstein[?]):

> I am, in fact, rather firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this (theory) operates with an incomplete description of physical systems....
>
> [In] a complete physical description, the statistical quantum theory would ... take an approximately analogous position to the statistical mechanics within the framework of classical mechanics....

Part of what Einstein is saying here is that (much of) the apparent peculiarity of quantum theory arises from mistaking an incomplete description for a complete one.

So much by way of introduction (to the introduction). We'd like now to rather abruptly switch gears, and turn to a brief consideration of quantum theory qua formalism.

Quantum mechanics is often presented as an (axiomatic) formalism involving *observables* and *states*, represented by abstract algebraic objects of various sorts: Observables are represented by self-adjoint operators on some Hilbert space, states by vectors in that Hilbert space, the dynamics is given by a unitary evolution generated—via Schrödinger's equation—by a special observable, the Hamiltonian H, and the statistics for the results of measurements can be compactly summarized by the formula $E_\psi(A) = (\psi, A\psi)$ for the expected value of observable A in state ψ (normalized).

What is there to complain about in this? Nothing, per se, but we *should* be clear as to what precisely this quantum formalism is about. What we are usually told, and what we believe is correct, is that the quantum formalism is a "measurement" formalism. Thus it is a phenomenological formalism describing certain macroscopic regularities. For example, in the two-slit experiment the macroscopic regularities involve the pattern of spots on a plate. As such the quantum formalism should be compared with the thermodynamic formalism.

As far as the thermodynamic formalism is concerned, physicists now all agree that a major step was taken around the turn of the century when, owing to the efforts of Maxwell, Boltzmann, Gibbs, and Einstein, the thermodynamic formalism was derived from microscopic physics, from the behavior of the constituents of the macroscopic systems. (Recall that this derivation was controversial back then, even if it isn't now.) Should we not demand a similar account of the quantum formalism?[?]

In fact, what makes quantum mechanics controversial is not, as we have already indicated, the quantum formalism itself, but rather a further assertion to the effect that we cannot get beneath this formalism, to account for it in microscopic terms. This is, indeed, a radical claim. However, it can easily be refuted by explicit counterexample, which we shall give.

Yet more radical is the claim that even if a microscopic account of the quantum formalism were possible, we should ignore it, since we would still have access only to our observations. Like most arguments that ultimately lead to solipsism, this argument cannot easily be refuted. But why should we, as physicists or philosophers of science, be concerned with such arguments? Are they not best left to the sceptics—ourselves included when we are in a sceptical frame of mind?

How are we to go about finding a microscopic theory yielding the quantum formalism? The best way to proceed (an important lesson) is to forget about the problem and go back to basics. We find that such a theory then emerges in such an inevitable manner that we are almost forced to conclude that philosophical prejudice must have played a crucial role in its nondiscovery.

We should recall, before proceeding, that what we are about to describe has been declared impossible, physically and philosophically, on the authority of Bohr, and even mathematically and logically, on the authority of von Neumann[?] and many others.[?] It is thus all the more remarkable that a counterexample to such declarations can easily be obtained even while ignoring the very formalism with which it is allegedly incompatible.

The quantum formalism does, however, give us a clue. The element of the quantum formalism which most *seems* to function as a theoretical entity on the microscopic level, as the objective state, is the wave function.

Suppose now that when we talk about the wave function of a system of N particles, we seriously mean what our language conveys, i.e., suppose we insist that *"particles" means particles*. If so, then the wave function cannot provide a complete description of the state of the system; we must also specify its most important feature, the *positions* $\mathbf{Q}_i$ of the particles themselves.

Suppose, in fact, that the complete description of the quantum system—its total state—

is given by (Q, ψ), where

$$Q = (\mathbf{Q}_1, \ldots, \mathbf{Q}_N) \in \mathbb{R}^{3N}$$

is the configuration of the system and

$$\psi = \psi(q) = \psi(\mathbf{q}_1, \ldots, \mathbf{q}_N),$$

a (normalized) function on the configuration space, is its wave function. Then we shall have a theory once we specify the law of motion for the state (Q, ψ). The simplest possibility is that this motion is given by first-order equations—so that (Q, ψ) is indeed the state in the sense that its present specification determines the future. We already have an evolution equation for ψ, i.e., Schrödinger's equation:

$$i\hbar \frac{\partial \psi_t}{\partial t} = H\psi_t = -\sum_{k=1}^{N} \frac{\hbar^2}{2m_k} \boldsymbol{\nabla}^2_{\mathbf{q}_k} \psi_t + V\psi_t.$$

According to what we have just said we are looking for an evolution equation for Q of the form

$$\frac{dQ_t}{dt} = v^{\psi_t}(Q_t)$$

with

$$v^{\psi} = (\mathbf{v}_1^{\psi}, \ldots, \mathbf{v}_N^{\psi})$$

and where v^{ψ} is a (velocity) vector field on configuration space $\mathbb{R}^{3N}$. Thus the role of the wave function ψ here is to generate the motion of the particles, through the vector field on configuration space to which it is associated:

$$\psi \longmapsto v^{\psi}$$

But how should v^{ψ} be chosen? A specific form for v^{ψ} arises by requiring space-time symmetry—Galilean and time-reversal invariance (or covariance), and simplicity:[?] For a one-particle system, we find that

$$\mathbf{v}^{\psi} = \frac{\hbar}{m} \operatorname{Im} \frac{\boldsymbol{\nabla}\psi}{\psi},$$

and for the general N-particle system,

$$\mathbf{v}_k^{\psi} = \frac{\hbar}{m_k} \operatorname{Im} \frac{\boldsymbol{\nabla}_k \psi}{\psi}.$$

Notice that the ∇ 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—the Im by time-reversal invariance, since time-reversal is implemented on ψ by complex conjugation, again as demanded by Schrödinger's equation, and the constant in front is precisely what is required for covariance under Galilean boosts.

We've arrived at **Bohmian mechanics**—for a nonrelativistic system (universe) of N particles, without spin. (Spin, as well as Fermi and Bose-Einstein statistics, can easily be dealt with[?,?] and in fact arise in a natural manner,[?] but we shall not consider these matters here.) This theory, a refinement of de Broglie's pilot wave model,[?] was constructed and compellingly analyzed by David Bohm in 1952.[?]

Bohmian mechanics is the most naively obvious embedding imaginable of Schrödinger's equation into a completely coherent physical theory. If one didn't already know better, one would naturally conclude that it can't "work," i.e., that it can't account for quantum phenomena. After all, if something so obvious and, indeed, so trivial, works, great physicists would never have insisted, as they have and as they continue to do, that quantum theory demands radical epistemological and metaphysical innovations.

Be that as it may, Bohmian mechanics is a fully deterministic theory of particles in motion, but a motion of a profoundly nonclassical, non-Newtonian sort. We should remark, however, that in the limit $\frac{\hbar}{m} \to 0$, the Bohm motion Q_t approaches the classical motion.[?]

But what in fact does this theory, Bohmian mechanics, have to do with orthodox quantum theory, i.e., with the quantum formalism? Well, of course, they share Schrödinger's equation. On the other hand, in orthodox quantum theory noncommuting observables, represented by self-adjoint operators, play a fundamental role, while they do not appear at all in the *formulation* of Bohmian mechanics. Nonetheless, it can be shown that Bohmian mechanics not only accounts for quantum phenomena—this was essentially done by Bohm[?] in 1952 and 1953—but also embodies the quantum formalism itself, self-adjoint operators, randomness given by $\rho = |\psi|^2$, and all the rest, as the very expression of its empirical import.[?]

Before proceeding to a sketch of how some of these things emerge from Bohmian mechanics, let's reconsider briefly the two-slit experiment. How *does* the electron know, when it passes through one of the slits, whether or not the other slit is open so that it can adjust its motion accordingly? The answer is rather trivial: The motion of the electron is governed by the wave function. When both slits are open, the wave function develops an interference profile, and it is not terribly astonishing that this pattern should be reflected in the motion that this wave function generates for the electron.

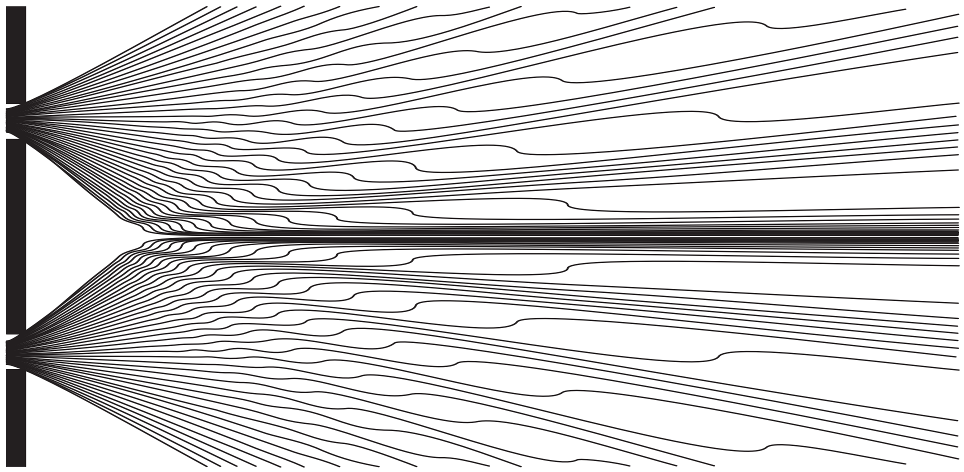

Figure 1.

In Fig. 1, we see an ensemble of Bohm trajectories when both slits are open. Notice the *development* of the interference pattern in the ensemble of trajectories. (At the left you see a homogeneous ensemble, at the right a typical interference profile.) Concerning Fig. 1, a few comments and a warning:

(1) No forces act to the right of the slits, yet the paths are rather crooked: The motion is highly non-Newtonian.

(2) The pattern depends, of course, on the (initial) ensemble or distribution. While a reasonably "regular" distribution would behave much like what you see, very special choices could lead to a very different pattern, or to no pattern at all. As an extreme example, suppose all trajectories in the ensemble start at the same point. Then of course there is but one trajectory in the ensemble, and hence we would find but one very bright spot on the plate in this case.

(3) At the other extreme, if the initial ensemble is given by $\rho = |\psi|^2$, it can be shown that this will remain the case, and the accumulated pattern of arrivals at the plate will completely agree with the prediction of the quantum formalism.

(4) It thus follows that given $\rho = |\psi|^2$ (for the initial ensemble), the *detailed* integration of the Bohm evolution equation has no predictive value for the result of the two-slit experiment. This is in fact quite generally the case.

What is special about the familiar distribution $\rho = |\psi|^2$ for Bohmian mechanics? It is **equivariant**: Consider the ensemble evolution $\rho \to \rho_t$ arising from the Bohm motion. ρ_t is the ensemble to which the Bohm evolution carries the ensemble ρ in t units of time. If $\rho = \rho^\psi$ is a functional of ψ (e.g., $\rho^\psi = |\psi|^2$) we may also consider the transformation $\rho^\psi \to \rho^{\psi_t}$ arising from Schrödinger's equation. If these evolutions are compatible,

$$\left(\rho^\psi\right)_t = \rho^{\psi_t},$$

we say that ρ^ψ is **equivariant**. In other words, the equivariance of ρ^ψ means that under the time evolution it retains its form as a functional of ψ.

That $\rho^\psi = |\psi|^2$ is equivariant follows immediately from the observation that the quantum probability current $J^\psi = |\psi|^2 v^\psi$, so that the continuity equation

$$\frac{\partial \rho}{\partial t} + \operatorname{div}(\rho v^\psi) = 0$$

is satisfied by the density $\rho_t = |\psi_t|^2$. As a consequence,

If $\rho(q,t_0) = |\psi(q,t_0)|^2$ at some time t_0, then $\rho(q,t) = |\psi(q,t)|^2$ for all t.

What we have said so far is merely a prologue to a systematic analysis of the empirical implications of Bohmian mechanics. Such an analysis falls naturally into two parts:

(1) The emergence and significance of other observables.

(2) The clarification and justification of the formula $\rho = |\psi|^2$.

Concerning the former, we'll make here but a couple of comments:

The crucial observation has been made by Bell:[?]

> ...in physics the only observations we must consider are position observations, if only the positions of instrument pointers. It is a great merit of the de Broglie-Bohm picture to force us to consider this fact. If you make axioms, rather than definitions and theorems, about the 'measurement' of anything else, then you commit redundancy and risk inconsistency.[?]

But one can go further and consider a general measurement-like experiment $\mathcal{M}$. By the latter we mean merely that, unlike a coin flip, the outcome is "reproducible". It turns out[?] that, given $\rho = |\psi|^2$, it follows easily from linearity that with every such experiment we may associate a self-adjoint operator $A = A_{\mathcal{M}}$,

$$\mathcal{M} \to A_{\mathcal{M}},$$

which governs the statistics of the outcomes in the usual way. From a Bohmian perspective, a "measurement of the observable A" means nothing more than an experiment so associated with the operator A that the result of this experiment, given for example by a pointer reading, is distributed according to the spectral measure of A.[?]

What about the assertion that $\rho = |\psi|^2$? The statement may seem to some clear enough: When a system has wave function ψ, its configuration is random, with distribution $|\psi|^2$, an assertion which can be regarded as roughly analogous to the Gibbs postulate of statistical mechanics. On the one hand, there is what may be called **quantum equilibrium**

$$\rho(q) = |\psi(q)|^2.$$

On the other hand, there is the familiar (classical) **thermodynamic equilibrium**

$$\rho(q,p) \sim e^{-\beta H(q,p)}.$$

We note, however, that it turns out that while the complete justification of the latter is remarkably difficult (and as of now nonexistent), that of the former is remarkably easy.[?]

But 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?

Let's go further. 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 $\rho = |\psi|^2$. But 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 is so (and it is), it would be devoid of physical significance! As Einstein has emphasized,[?] "Nature as a whole can only be viewed as an individual system, existing only once, and not as a collection of systems."

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.

Having said this, we would like explicitly to address a common misconception. It is tempting when trying to justify the use of a particular 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. They are also usually very difficult to establish, even for rather simple and, indeed, artificially simplified dynamical systems. A recent attempt along these lines, due to Valentini,[?] concerns the convergence to quantum equilibrium for the distribution of the configuration of the universe as a whole under the Bohmian dynamics. One simple consequence of our discussion is that, regardless of their mathematical validity, such proofs of 'convergence to equilibrium,' for the configuration of the universe, are 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?[?]

Two problems must thus 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[?] that in a *typical* Bohmian universe an appearance of randomness emerges, precisely as described by the quantum formalism.[?]

What about the wave function of a subsystem? Given a subsystem we may write $q = (x, y)$ where x and y are generic variables for the configurations of the subsystem and its environment. Similarly, we have $Q_t = (X, Y)$ for the actual configurations (at time t). What is the simplest possibility for the wave function of the subsystem, the x-system; what is the simplest function of x which can sensibly be constructed from the actual state of the universe at time t (which we remind you is given by Q_t and $\Psi_t = \Psi$)? Clearly the answer is

$$\psi(x) = \Psi(x, Y).$$

This is all we need.[?]

The key ingredients in the analysis of randomness[?] are the following:

(1) The *effective wave function*: Suppose that

$$\Psi(x, y) = \psi(x)\Phi(y) + \Psi^{\perp}(x, y),$$

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

$$Y \in \operatorname{supp} \Phi$$

we say that ψ is the **effective wave function** of the x-system.

Note that it follows that $\Psi(x, Y) = \psi(x)\Phi(Y)$, so that the effective wave function is unambiguous, and indeed agrees with the formula in the preceding paragraph, 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, 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 the Bohm's 1952 "hidden variables" paper.[?] 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.

(2) 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 the effective wave function of the x-system is the product wave function

$$\psi_t(x) = \psi(x_1) \cdots \psi(x_M).$$

(3) The *fundamental conditional probability formula*:

$$\mathbf{P}^{\Psi_0}\big(X_t \in dx \mid Y_t\big) = |\psi_t(x)|^2 \, dx,$$

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

Note that it follows from (2) and (3) that the configurations of the subsystems referred to in (2) are independent, identically distributed random variables with respect to the quantum equilibrium distribution conditioned on the environment of these subsystems. Thus the law of large numbers can be applied to conclude that under the supposition in (2), 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, as is shown in the first reference in note ?, this conclusion applies as well to a collection of systems at possibly different times as to the equal-time situation described here.

We would like to make a few comments now about Bohmian mechanics and "the real world". There is at best an uneasy truce between orthodox quantum theory and the view that there is an objective reality, of a more or less familiar sort on the macroscopic level.

Recall, for example, Schrödinger's cat. What does Bohmian mechanics contribute here? In a word, everything! A world of objects, of large collections of particles which combine and move more or less as a whole, presents no conceptual difficulty for Bohmian mechanics, since Bohmian mechanics is after all a theory of particles in motion and allows for the *possibility* of such large collections.

So what, when all is said and done, does the incorporation of the particle positions, of the configurations, buy us? A great deal:

(1) randomness
(2) familiar (macroscopic) reality
(3) the wave function of a (sub)system
(4) collapse of the wave packet
(5) absolute uncertainty

We have not yet explicitly addressed here item 5, which is a consequence of the analysis[?] of $\rho = |\psi|^2$. It expresses the impossibility of obtaining information about positions more detailed than what is given by the quantum equilibrium distribution. It provides a precise, sharp foundation for the uncertainty principle, and is itself an expression of global quantum equilibrium.

We close with two comments and a quotation:

(1) From the perspective afforded by Bohmian mechanics, it is not terribly astonishing that all sorts of conceptual difficulties arise from disregarding the actual configuration, in effect ripping out the heart, if not cutting off the head, of the quantum dynamical system.
(2) In fact, when all is said and done, it seems fair to say that Bohmian mechanics is nothing but quantum physics without quantum philosophy.
(3) Finally, in response to the outrage sometimes expressed towards the suggestion that particles might have positions when they are not, or cannot be, observed, Bell,[?] referring to theories such as Bohm's, declared:

Absurdly, such theories are known as "hidden variable" theories. Absurdly, for there it is not in the wavefunction that one finds an image of the visible world, and the results of experiments, but in the complementary "hidden"(!) variables. Of course the extra variables are not confined to the visible "macroscopic" scale. For no sharp definition of such a scale could be made. The "microscopic" aspect of the complementary variables is indeed hidden from us. But to admit things not visible to the gross creatures that we are is, in my opinion, to show a decent humility, and not just a lamentable addiction to metaphysics.

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8 We have let the quantum formalism off a bit too easy. Indeed, in the absence of such an account, the quantum formalism itself suffers from serious vagueness and ambiguity, owing to the fact that this formalism *seems* to refer to the relationship between the microscopic and the macroscopic, for example insofar as it refers to "measurements of observables" for microscopic systems—and in practice this is what it always does! What is supposed to be meant by the "measurement" of observables such as $p+q$ for an electron, when we are told that an electron is not the sort of thing which it can ever make sense to regard as having

simultaneous position and momentum? Or, more fundamentally, what do we really mean by the "measurement" of any (in effect, nonexistent until measured) microscopic quantum observable? At the same time, is not something like measurement—a suitable interaction between a system of interest and its environment—going on, as Bell has emphasized, more or less everywhere, all the time? Remember, too, that Einstein[?] has emphasized that a fundamental theory cannot itself be founded upon *observables*, since "whether you can observe a thing or not depends on the theory which you use. It is the theory which decides what can be observed." Thus without a fully microscopic theory, the quantum measurement formalism must suffer from a lack of clarity and precision in the statement of its operational details. (Perhaps "must" is too strong. In any case it *does* so suffer.)

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19 M. Daumer, D. Dürr, S. Goldstein, and N. Zanghí, On the role of operators in quantum theory, in preparation, and reference 14.

20 Reference 10.

21 We should perhaps remark that it would be better for the advocates of Bohmian mechanics were it not the case that every measurement is ultimately a position measurement in the sense that its result is grounded configurationally, at least potentially. That would open the possibility of some empirical disagreement between Bohmian mechanics and orthodox quantum theory, so that some decisive experimental test might arise. Unfortunately this appears to be extremely unlikely—see footnote 30 of the first reference in note 11.

22 Reference 19.

23 It is perhaps worth remarking that from a strict Copenhnagen perspective "measurement of the observable A" also has precisely this meaning.

24 Reference 11.

25 A. Einstein, in Scientific Papers Presented to Max Born, Oliver Boyd, Edinburgh, 1953, pages 33–40; quoted in reference ?, page 570.

26 A. Valentini, Signal-locality, uncertainty, and the subquantum H-theorem, I, Phys. Lett. A, 156 (1992), 5–11.

27 As far as mathematical significance is concerned, Valentini's argument claiming to establish convergence to quantum equilibrium is unfortunately not valid: (i) It 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). (ii) The H-theorem is itself not correctly proven—it could not be since it is in general false. (iii) 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[?] "the system is 'sufficiently chaotic'," which more or less amounts to assuming the very mixing which was to be derived.

28 Reference 11.

29 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—which is very small with respect to a certain natural measure, namely the universal quantum equilibrium distribution, on the set of all universes. It is important to realize that this guarantees that it holds for many particular universes, one of which might be ours. For a recent article emphasizing the importance of typicality to our — i.e., Boltzmann's—understanding of the origin of macroscopic irreversiblity, and warning against the all too frequent misuse of ensembles in statistical physics, see J. L. Lebowitz, Boltzmann's entropy and time's arrow, Physics Today 46 (1993), 32.

30 This is not quite the right notion for the *effective wave function* of a subsystem (see below; see also reference 11), but whenever the latter exists it agrees with what we have just described. Incidentally, you should try to see what you can do without actual configurations. You'll, of course, quickly encounter the measurement problem. Note, in fact, that the result of a measurement performed upon a quantum system is embodied in the configuration of the environment of this system, for example in the orientation of a pointer on the apparatus used for the measurement. It is this configuration which, when inserted in the formula above, selects the term in the after-measurement macroscopic superposition—arising from the Schrödinger evolution of system and apparatus in interaction—that we speak of as defining the system wave function produced by the measurement.

31 Reference 11.

32 Reference 13.

33 Reference 11.

34 J. S. Bell, Are there quantum jumps?, in reference ?, pages 201–212.

35 Quoted by W. Heisenberg, Theory, criticism, and a philosophy, in Unification of Fundamental Forces, by A. Salam, Cambridge University Press, New York, 1990, page 99.

36 J. S. Bell, Speakable and unspeakable in quantum mechanics, Cambridge University Press, Cambridge, 1987.

37 P. R. Holland The Quantum Theory of Motion, Cambridge University Press, Cambridge, 1993.

38 A. Valentini, On the Pilot-Wave Theory of Classical, Quantum and Subquantum Physics, Ph.D. thesis, International School for Advanced Studies, 1992, page 36.