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REVIEW ARTICLE

Copenhagen versus Bohmian Interpretations of Quantum Theory[1]

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Quantum theory consists of a mathematical formalism together with a vast amount of information concerning how to apply that formalism to electrons, atoms, radiation, fields, and so on. As an instrument for predicting the results of experiments it is enormously successful. But what it says, if it says anything at all, about the electrons, fields, etc. that produce those results, has proved very difficult to fathom. From its inception it has been a theory in search of an *interpretation*. The interpretation that has shaped the thinking of most physicists and philosophers of physics was thought up by Niels Bohr and others (most notably Heisenberg, Pauli, and Born) during the first third of the twentieth century and has come to be known as 'the Copenhagen Interpretation' of quantum mechanics (CQM).[2] Among the propositions one finds associated with CQM are these:

i) The quantum state (represented by a vector in an appropriate Hilbert space) of a physical system (e.g. a hydrogen atom) is its *complete* physical state. For a given state some physical properties possess *determinate* values while others fail to possess *determinate* values. For example, a state in which an electron possesses a determinate position is one in which it fails to possess a determinate momentum and visa versa. It follows that electrons (and similarly other quantum systems) do not possess determinate trajectories.
ii) The quantum state of a system determines the probabilities of outcomes of *measurements* of the system's physical properties (in accord with Born's rule). These probabilities are fundamental objective chances and not due to ignorance of the values of 'hidden variables'.

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[1] Review of James T. Cushing [1994]: *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*, Chicago, University of Chicago Press.

[2] There is no 'official' Copenhagen interpretation but rather a collection of views. For a discussion of it and further references, see the book under review.

iii) An isolated quantum system evolves in conformity with a linear deterministic law (Schrödinger's equation) unless it is measured. Measurements are governed by an indeterministic law—the collapse postulate. Measuring a physical property Q (e.g. position) on a system in a state for which Q doesn't possess a determinate value collapses that state into one in which Q has a determinate value.

iv) There are results—so-called 'no-go theorems' (associated with von Neumann, Kochen-Specker, and Bell)—that demonstrate the impossibility of supplementing the quantum mechanical formalism with 'hidden variables' so as to render all physical quantities determinate and the dynamical laws governing physical quantities deterministic.

v) What properties a quantum system possesses, or even what property-attributions are meaningful, is not an entirely objective matter of fact but is dependent in some way on an experimenter's choices of what to measure. For example, in the well-known two-slit experiment an electron (same for other quantum systems) sometimes behaves like a wave and sometimes like a particle, depending on what properties the experimenter *chooses* to measure. Bohr called this feature of quantum mechanics 'complementarity'.

vi) The right way to understand quantum mechanics is not as a true description of physical reality but rather as an *instrument* for predicting the outcomes of laboratory experiments. There is no coherent interpretation of the quantum-mechanical formalism as describing an unobservable reality that is responsible for those experimental results. That reality is forever beyond our ken.

The preceding propositions *interpret* quantum mechanics in that they attempt to say something about what the world must be like for the theory to be true or applicable. What CQM says is itself vague, obscure, and maybe even inconsistent. Nevertheless it became the *orthodox interpretation* and has been taught to generations of physicists as the *only* correct way to think about quantum theory.

CQM is vague since it doesn't tell us exactly which interactions are measurements. This is a significant omission since its laws specify that non-measurements evolve one way (by Schrödinger's law) and measurements a different way (by the collapse).[3] It is also vague in its scope. Bohr held that quantum descriptions presupposed classical descriptions, so that CQM isn't universally applicable. But exactly where the *cut* between the quantum-mechanical and the classical occurs is left unspecified.

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[3] There have been various proposals concerning exactly which interactions are measurements, ranging from interactions with macroscopic systems to interactions with conscious systems. See Albert [1992] and Wheeler and Zurek [1983].

CQM is obscure. How are we to understand the assertion that a *particle* can possess a determinate velocity but no determinate position, or a determinate position but no determinate velocity, or lack determinate values for both properties? The relationship between measurement and determinate reality is especially obscure. What is special about measurements that *they* initiate a collapse, making a quantity determinate? Especially puzzling is that the collapse seems to involve a 'spooky' non-locality, since a measurement of a part of a system at one location can instantaneously change the physical situation of far distant parts of that system. Although some Copenhagenists try to do without the collapse, it appears that without it macroscopic systems (assuming the theory applies to them) would evolve into states in which familiar properties fail to possess determinate values.[4] This is the point that Schrödinger made with his famous thought-experiment in which, without the collapse, a cat ends up in a state—a superposition of being alive and being dead—which, by the usual quantum-mechanical rules, is a state in which the cat is neither determinately alive nor determinately dead.

CQM may also be inconsistent in that, on the one hand, it makes assertions about the nature of quantum-mechanical reality while, on the other hand, it denies that anything can be known about that reality. All this confusion and mystery didn't and doesn't deter the widespread acceptance of CQM.

Not all the creators of quantum theory were satisfied with the Copenhagen orthodoxy. Schrödinger and Einstein both disliked it. The latter described it as 'a tranquilizing philosophy ... a gentle pillow for the true believer from which he is not easily aroused.'[5] What most bothered Einstein was not (as his often cited remark that he doesn't believe in a 'God that plays dice' suggests) CQM's indeterministic laws, but rather its subjectivism, non-locality, and instrumentalism. He thought that physics should aspire to produce a true description of an objective reality. At one time Einstein suggested that the quantum-mechanical description is incomplete and that by adding 'hidden variables' to the characterization of a system's state it would be possible to do away with indeterminateness, collapses, and subjectivity. But the orthodox claimed to have arguments and proofs that demonstrated the impossibility of adding extra variables to the quantum-mechanical description while preserving its predictive successes. This claim receives almost universal acceptance even among those physicists and philosophers who don't buy all of CQM. That this was the prevailing opinion may explain, at least partly, why when in 1952

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[4] There is a tradition of arguing that even without the collapse, systems involving many degrees of freedom evolve into states which are practically indistinguishable from states, called 'mixtures', in which the relevant properties possess determinate values. This proposal has been made many times and criticized many times. The 'many-worlds interpretation' is another way of doing without the collapse. But making it work involves making additions (a preferred basis, worlds, minds, etc.) to quantum theory. See Albert [1992] for discussions of both these ideas.

[5] A. Einstein, Letter to E. Schrödinger, May 1928.

David Bohm produced just such a hidden-variable theory it was—and has been until quite recently—ignored by physicists and philosophers. The result most often cited to justify the impossibility of hidden-variable interpretations like Bohm's is a theorem due to John Bell that establishes that no *local* hidden variable theory can reproduce all the quantum mechanical statistics. For this reason the following quote may, even at this date, surprise some:

> But in 1952 I saw the impossible done. It was in papers by David Bohm. Bohm showed explicitly how parameters could indeed be introduced into nonrelativistic wave mechanics with the help of which the indeterministic description could be transformed into a deterministic one. More importantly, in my opinion, the subjectivity of the orthodox version, the necessity of reference to the 'observer,' could be eliminated. Moreover, the essential idea was one that had been advanced already by de Broglie in 1927 in his 'pilot wave' picture. But why then had Born not told me of this 'pilot wave'? If only to point out what was wrong with it? ... Should it not be taught ... [t]o show that vagueness, subjectivity, and indeterminism, are not forced on us by experimental facts, but by deliberate theoretical choice? (Bell [1987], p. 160)

The author of the preceding passage is John Bell, the very same Bell who discovered the theorem that bears his name. Someone familiar only with Copenhagen views could be pardoned for wondering whether Bell was aware that his theorem demonstrated the impossibility of hidden-variable interpretations.[6] But Bell understood the situation quite well, and quite differently from those who cite his theorem to justify rejecting Bohm's theory. He made it clear that the theorem showed that *local* hidden-variable theories cannot reproduce quantum-mechanical predictions, and that related arguments demonstrated that *no* local theory—whether or not it posits hidden variables and whether or not it is indeterministic—can do any better. The proper conclusion, he argued, is that if the quantum-mechanical statistics are lawful then non-locality is unavoidable.[7] In other papers Bell also explained why the other 'no-go' theorems go nowhere towards undermining Bohm's theory.[8]

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[6] My colleague Shelly Goldstein half-seriously suggests that those physicists and philosophers who believe that Bell's theorem demonstrates the impossibility of hidden-variable theories like Bohm's mistakenly think that there are two John Bells, one responsible for the theorem and the other advocating Bohm's theory.

[7] Of course, the question then arises of the compatibility of quantum mechanics and special relativity. This is a complex issue. While QM and SR are empirically compatible none of the realist interpretations of quantum mechanics except the many-minds account (Albert and Loewer [1988]) are completely compatible with special relativity, since the quantum-mechanical wave function exists in very high-dimensional configuration space, not in 4-dimensional Minkowski space-time. See Maudlin [1994] and Albert [forthcoming] for a thorough discussion.

[8] Bell (1987). The no-go theorems assume that all Hermitian operators represent physical quantities and then prove that given some reasonable assumptions about physical quantities, not all can simultaneously possess determinate values. But Bohm's theory rejects this kind of naïve realism about operators. See Durr, Goldstein and Zanghi [1992].

And while he didn't endorse Bohm's theory as the last word on interpreting quantum mechanics, he clearly thought it superior to the Copenhagen alternative.[9] In view of this, it is a bit shocking to find Bell's name so often invoked to justify rejecting or ignoring Bohm's theory.

Bohmian Quantum Mechanics (BQM) is an alternative to CQM in that it makes the same predictions for usual quantum-mechanical experiments. But its account of what underlies its empirical predictions is strikingly different. According to BQM (in its non-relativistic version), the complete physical state of an n-particle system at time t is characterized by (1) a quantum-mechanical state represented by a vector in the appropriate Hilbert space (on this it agrees with CQM), and (2) positions of the particles at t (in this it departs from CQM). In BQM, electrons (and other elementary particles) always possess definite positions and have determinate trajectories. The evolution of the quantum state is governed by the Schrödinger equation and the evolution of particle positions is governed by a 'guidance equation'.[10] Both these laws are deterministic. Quantum-mechanical probabilities enter the theory by way of a postulate to the effect that particle positions are distributed in conformity with the usual quantum-mechanical probabilities.[11] These probabilities represent *ignorance* of the precise values of the quantum state and the particle positions. It follows from the theory that this ignorance is irremediable. BQM has no need for the collapse law since the particles possess determinate positions even when their quantum state fails to assign them determinate positions. So when the state of a cat is a superposition of alive and dead, the positions of the cat's particles 'decide' whether the cat is alive or is dead. BQM is realist and objective in that it describes an unobservable and mind-independent reality that underlies its empirical predictions.[12] It thus rejects both (v) and (vi) of CQM.

BQM isn't quite what Einstein had hoped for since it is non-local. But CQM is also non-local and, as previously mentioned, Bell demonstrated that any theory that reproduces the lawfulness of the quantum-mechanical statistics will

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[9] Bell (1987) says: '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' (p. 58).

[10] The guidance equation says that if a particle system **p** is located at x (in configuration space) then the velocity of **p** depends on the value of Ψ in the region around x.

[11] It can be shown that if particle positions are initially distributed in conformity with $|\Psi|^2$ then they will continue to be so distributed. See Bohm and Hiley [1993]. The status of the distribution and the nature of the probabilities are not completely clear. See Durr, Goldstein, and Zanghi [1992], Loewer [1994], and Callender and Weingard [1997] for discussion of probabilities in Bohm's theory.

[12] In BQM measurements are treated just like any other interactions. Making a measurement on a system involves an interaction that typically affects the system. It turns out that given the probability postulate any measurement of one quantity, e.g. position, affects the quantum state in a manner so that other quantities, e.g. momentum, become uncertain in just the way required by Heisenberg's uncertainty principle.

also be non-local.[13] In view of all of this, the question naturally arises of why BQM was not seriously considered as an alternative to CQM. Perhaps it is not difficult to understand why physicists, who are primarily interested in developing and applying the theory to new situations, would put foundational questions out of mind, leaving them to philosophers. But BQM's neglect by philosophers is less excusable. It is easy to find books and papers by reputable philosophers of physics in which claims are advanced that are flatly incompatible with the mere existence of BQM. Prior to the early 1990s it is difficult to find philosophers of physics even mentioning the theory.[14]

Recently the situation has begun to change. In the 'foundations-of-QM community' there is now a lively discussion of alternatives to CQM, and Bohm's theory has come to be seen as one of the most interesting of these alternatives. James Cushing's book *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony* is a valuable contribution to this literature. In it Cushing tells the story of how the Copenhagen interpretation achieved hegemony over the thinking of physicists, and offers explanations of why de Broglie's pilot-wave theory, and later Bohm's theory, were rejected or ignored by the physics community. The book provides a good introduction to BQM for those unfamiliar with it, and interesting comparisons between it and CQM.

Cushing's account of the history of the foundations of quantum theory is full of drama. High points include the struggle between Bohr and Einstein at the Solvay Conferences, the famous EPR paper in which Einstein [1935] *et al.* tried to demonstrate the incompleteness of CQM and Bohr's [1935] obscure reply, Pauli's supposed devastating criticisms of de Broglie's pilot-wave theory causing the latter to abandon it, Bohm's rediscovery of the pilot wave and his disappointment that Einstein found it 'too cheap' and, finally, Bohm's death just when his theory was beginning to be taken seriously. Cushing's telling of the story is informative and engaging. Especially interesting is a *what if* chapter in which he speculates about the course that the history of physics would have taken had BQM got to the top of the hill first. Cushing suggests that had de Broglie been able to answer Pauli's objections, and had Bell's theorem been discovered in the 1920s, then perhaps the pilot wave would have won over the physics community. And once BQM established itself, physicists would have had little reason to exchange it for CQM, with its obscure metaphysics.

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[13] Bell says that it was his acquaintance with Bohm's theory that led him to ask whether any theory that reproduces QM statistics must also be non-local and that resulted in his famous theorem.

[14] One can find no mention of Bohm's theory in well-known books by Redhead [1987], Forrest [1988], and Hughes [1989]. Healy [1989] and van Fraassen [1991] scarcely mention it. I am not sure why philosophers of physics showed so little interest in Bohm's theory. During the 1970s and 1980s discussions of 'quantum logic' and 'non-locality' dominated the philosophy of quantum mechanics, with very little interest in the measurement problem. The logic of BQM is completely classical (see Loewer [1994]) and its non-locality was taken as a decisive objection. These considerations, along with Bohm's being seen as dabbling with eastern and mystical thought, probably contributed to lack of interest in BQM.

Cushing thinks that the actual history of quantum theory illustrates the ways that the development of science is driven by many kinds of forces, including cultural and sociological factors, considerations of scientific methodology, personalities, and historical contingencies (like de Broglie being unable to answer Pauli's objections). In this he steers a middle course between certain advocates of 'science studies' who seem to think of science as entirely responsive to sociological and cultural forces, and certain philosophers of science who think of science as entirely responsive to scientific methodology.[15]

The main philosophical issue discussed in the book involves the lessons that CQM and BQM may have for Realism. There are two notions of Realism that have figured in the philosophy of quantum theory. One is that the world investigated by physics is (for the most part) mind-independent in that *it is what it is* independently of our beliefs, thoughts, choices, and so on.[16] The second—'scientific realism'—is that we can have good reasons for beliefs about the unobservable aspects of reality that explain our observations. Cushing observes that CQM presents challenges to both kinds of realism. First, complementarity seems to involve the claim that physical reality is in some sense mind-dependent.[17] Second, CQM says that the unobservable reality that gives rise to laboratory observations is beyond our ken. If CQM were, as its proponents claimed it to be, the only way to understand quantum mechanics, then physics would have refuted the disjunction of both sorts of realism. However, the existence of BQM shows, as Bell says, that a subjective view is not forced on us by physics. And since BQM provides a consistent account of an unobservable reality that gives rise to the quantum-mechanical predictions, instrumentalism is not forced on us either.

Cushing argues that while BQM may save Realism from the Scylla of mind-dependence, it threatens shipwreck on the Charybdis of 'constructive empiricism'. This is van Fraassen's [1980] term for the view that even when a theory has a coherent interpretation, there can be no reason to believe what it says about the unobservable part of reality. The main argument for constructive empiricism depends on the existence of theories that are empirically equivalent but conflict with respect to what they say about the unobservables that account for observable phenomena. In brief, the argument is that if T and T′ are empirically equivalent then there can be no empirical reason to believe one to be true rather than the other. Further, it is claimed that for any empirically adequate theory T there is an empirically equivalent, incompatible theory T′.

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[15] Cushing's discussion is something of an antidote to Paul Forman's [1971] study of the early development of quantum theory, which emphasizes the influence of political and cultural factors on physics.

[16] Of course, Realists think that a small part of the world is dependent on our beliefs, choices, etc., i.e. the part that consists of these and their ordinary causal consequences. See Devitt [1984].

[17] John Wheeler claimed that it is only when an observer becomes consciously aware of a quantum state that a collapse is initiated, producing determinate facts even about the past.

This last claim is often supported by logical constructions showing that for a given theory T there are conflicting, empirically equivalent theories. Such constructions have a feeling of unreality about them. Given a theory that is empirically adequate (or approximately empirically adequate) these constructions, if we can apply them, typically yield theories that would not be taken seriously by scientists. As is often pointed out, finding one theory that is empirically adequate, let alone more than one, is very difficult. The argument against scientific realism would be more persuasive if one could find examples of scientifically serious theories that are conflicting, empirically equivalent, and empirically adequate (or nearly so). Cushing believes that non-relativistic versions of CQM and BQM provide a real-life example of conflicting, empirically equivalent theories that, if we pretend a little (i.e. ignore relativistic complications), are empirically adequate. This, if correct, would be important since, if there is no non-empirical reason to believe one more likely to be true than the other, it would follow that we can have no evidence that would make one more credible than the other. We may have no empirical reasons about what to believe concerning fundamental features of physical reality: whether the basic laws are deterministic or chancy, whether particles possess determinate trajectories, whether cats are sometimes neither alive nor dead, and so on.

But are CQM and BQM empirically equivalent? This issue is somewhat more complicated and subtle than Cushing's treatment makes out. Bohm demonstrated that for any system S in quantum state Ψ, any physical quantity Q, and any interaction that measures Q for which the outcome of the measurement is recorded in the *position* of something (e.g. the position of a pointer), both CQM and BQM assign the same probabilities to the possible outcomes of the measurement. Is this sufficient to conclude that CQM and BQM are empirically equivalent? The first problem encountered in trying to answer this is that it is not clear what the scope of CQM is. If CQM applies only to laboratory measurements (situations in which a system is prepared in a particular quantum state) then it says nothing about many observational events that are within BQM's scope. BQM is a *total* theory in that it applies to all events, including measurements and observations. CQM can be also be understood as a total theory (though this seems contrary to Bohr's intentions). So understood, every system, including measuring devices, cats, human observers, etc., has a quantum-mechanical description. The state of a system evolves in conformity with Schrödinger's law, except when measurement interactions occur. Then the collapse takes over.

The fact that BQM and CQM are total theories has an important consequence for the anti-realist argument. Realists often counter the anti-realist argument by observing that what is observationally relevant to a theory is not a fixed matter. It can depend on what other theories and auxiliary assumptions are accepted and on what instrumentation is available. This line of thought has been used to derail the anti-scientific realist argument since two theories that are empirically equivalent at one time may no longer be so when conjoined with some further theory or auxiliary assumptions, or when new observations can be made. But since CQM and BQM are total theories they will make predictions about our observations, beliefs, and so on no matter what further scientific advances are made (in the special sciences), and if they are empirically equivalent they will make the same predictions. So if CQM and BQM are genuinely empirically equivalent and also empirically adequate, then one of the Realist replies to the anti-realist argument, perhaps the strongest one, would be unavailable.

Cushing raises one worry about whether CQM and BQM are in fact empirically equivalent. He considers an experiment in which a beam of particles are directed toward a potential barrier (p. 54). Some particles are reflected by the barrier and some pass through. Cushing observes that, within standard quantum theory, there is no fact of the matter concerning the length of time that a particle is within the barrier, and no way to calculate this time. However, he thinks that in BQM these quantities can be calculated, and so suggests that BQM may make a prediction that CQM fails to make. I find this very puzzling. If Cushing is claiming that there is a *measurement* for which BQM makes a prediction while CQM makes no prediction then he is mistaken. Bohm proved otherwise (as long as outcomes are recorded in position). But if he means that there are predictions concerning the values of certain quantities that BQM makes but CQM fails to make, then he is certainly correct. BQM predicts that particles possess determinate trajectories while CQM predicts that particles don't possess determinate trajectories. But there is no way to check to see which is correct, since when we actually make a measurement the two theories will make exactly the same predictions. So this worry about empirical equivalence is groundless.

However, there are other reasons to qualify the claim that CQM and BQM are empirically equivalent. These reasons involve CQM's vagueness. Since the notion of *measurement* occurs in CQM's fundamental laws, exactly how systems evolve is not clearly specified until measurement is clearly specified. The exact location of the cut makes a difference to empirical predictions. Here is an example of how that works. In Schrödinger's cat thought-experiment the interaction, if governed by Schrödinger's equation, results in a state of the form $c|\text{Alive}\rangle + c^*|\text{Dead}\rangle$, but if governed by the collapse, results in either $|\text{Dead}\rangle$ or $|\text{Alive}\rangle$. Further, the theory makes different predictions for certain measurements on the states $|\text{Dead}\rangle + |\text{Alive}\rangle$ and $|\text{Alive}\rangle$ (and similarly for $|\text{Dead}\rangle$). It is incredibly difficult to know exactly what these measurements are and, even if we could figure them out, to make them. This is why CQM can get away with leaving 'measurement' vague. But let's suppose that the vagueness is replaced by a definite characterization of measurement. Now the question is whether this theory is empirically equivalent to BQM. BQM makes predictions concerning the positions, and in particular, the positions of the pointers (and other devices that register the results of measurements in position). Recall that Bohm showed that if a system is in state Ψ and a measurement recorded in the position of some device is made, then BQM and CQM make the same predictions (a probability distribution) concerning the values of that position. But this is not quite the same as proving that the two are empirically equivalent. The reason is that the two theories may make different predictions concerning how the state of a complex system evolves. There is no collapse on BQM. So at the conclusion of a measurement of a system S by a measuring device M, BQM and CQM assign different quantum states to the composite system $S + M$. And this means that there are measurements that are in principle possible on $S + M$, for which the two theories yield different predictions. Therefore CQM and BQM are not exactly empirically equivalent. Still, for all practical purposes they are empirically equivalent, since we are in no position either to know exactly what measurements will discriminate between the two or to make such measurements.

Suppose, then, we accept that BQM and CQM are for all practical purposes empirically equivalent. Does this mean that we now have no reason to believe one of CQM and BQM over the other? I find Cushing's view of this difficult to pin down. At the conclusion of the book he seems to see the situation as a standoff between the two theories. But throughout the book he emphasizes CQM's defects, its vagueness and obscurity. If the choice is between CQM and BQM, then (ignoring the problems of making both theories fully compatible with relativity) it seems to me that BQM wins hands down. I don't mean that we have reason to believe the unobservable parts of Bohm's theory, but only that the existence of CQM as a competitor gives us no reason to suspend judgement between the two.

So if the choice were only between CQM and BQM, Realism would be safe (at least as far as this argument from QM is concerned). But there are a number of other interpretations that conflict with BQM but are, in a sense to explained shortly, for all practical purposes empirically equivalent to it. I have in mind many-worlds (and many-minds), modal, and GRW collapse theories.[18] The first two, like BQM, are no-collapse theories and the latter, like CQM, is a collapse theory but, unlike CQM, is realist and gives no special place in its laws to measurement.[19]

The question of empirical equivalence with respect to these theories is rather interesting since each has a different account (or in the case of GRW more than one account) of exactly which experimental outcomes are determinate. For

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[18] See Albert [1992] for discussion of these alternatives.

[19] GRW may turn out to have empirical consequences that can be tested. See Albert [forthcoming].

example, on the many-minds variant of the many-worlds interpretation experimental outcomes are restricted to mental states of observers, while on Bohm's theory and GRW they involve positions (of pointers, or particles in the brain, or some such). Further, the positions that are determinate on GRW are not exactly those that are determinate on Bohm's theory. One might think this means that we can then experimentally distinguish the theories by looking and seeing what positions actually are determinate. But this turns out not to be the case. The reason is that each theory also says when it is determinate that an observer sees a particular outcome. What can be said about all these theories (at least if we stick to non-relativistic quantum mechanics) is that if any one is true then there is (plausibly) no feasible experiment whose outcomes could count differentially among them. And if this is right, then as Cushing suggests, we may never be in a position to have empirical reasons which decide among fundamental physical features of our world. Of course, that doesn't mean that we might not have *other* reasons—non-empirical reasons involving simplicity, unification, compatibility with familiar metaphysics, etc.—to favour one theory over another. There is one other hope for the scientific realist. The existence of theories with conflicting ontologies that are empirically equivalent and adequate is a contingent matter. Perhaps we now have reason to believe that the true fundamental physical theory of our world is empirically equivalent to other, conflicting (and so false) theories. But all this might change given future research. Perhaps, as Einstein apparently thought near the end of his life, a fully empirically adequate theory, one that can combine general relativity and quantum mechanics, will involve radical reworking of physics that results in a theory without empirically equivalent competitors. In the meantime we may have to learn to live with the idea that nature keeps some of its most important features secret from us.

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