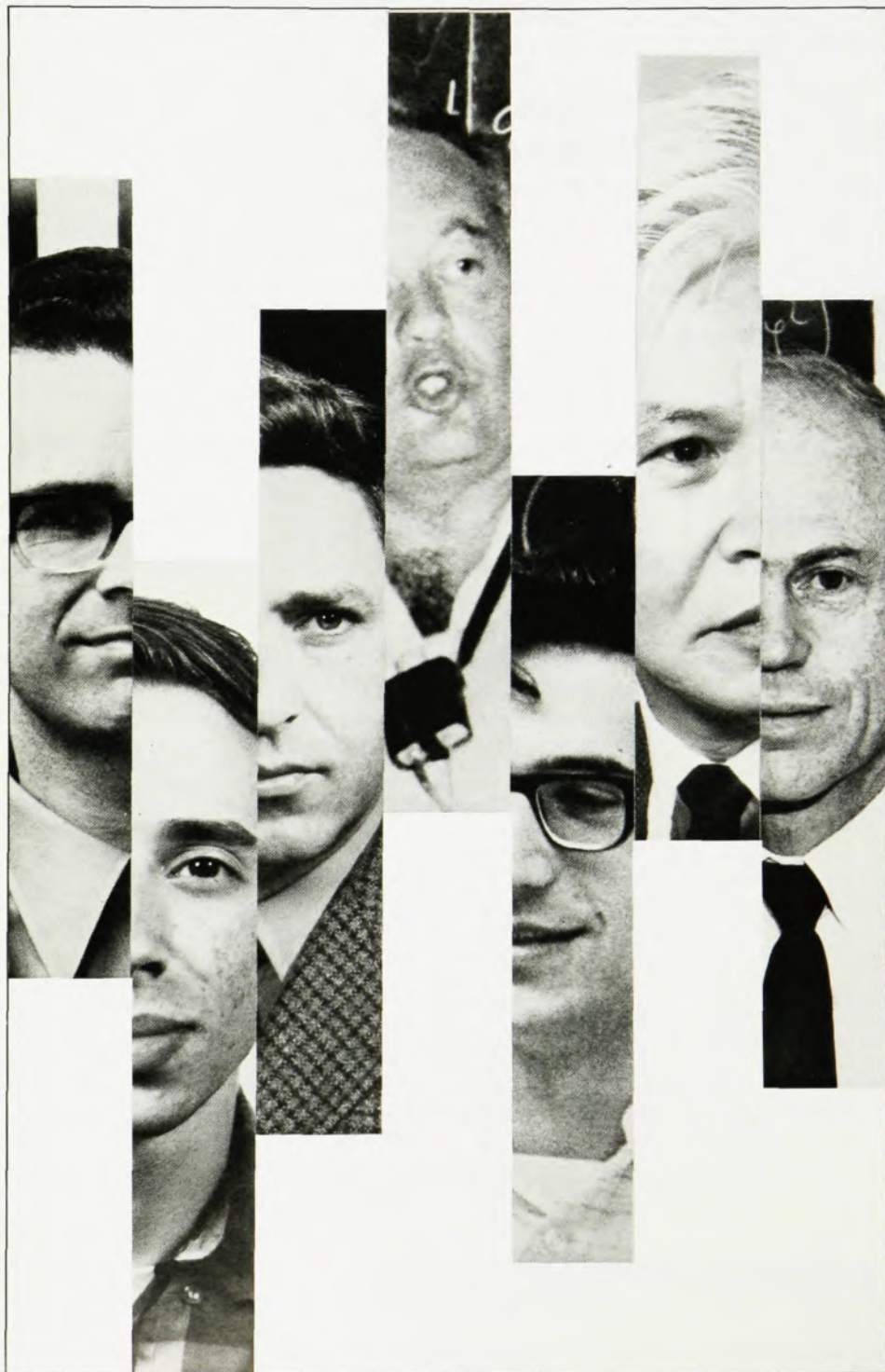


Quantum-mechanics debate

Not everyone agrees with Bryce DeWitt that acceptance of quantum theory implies belief in a continually splitting universe



"Despite its enormous practical success, quantum theory is so contrary to intuition that, even after 45 years, the experts themselves still do not agree what to make of it . . ." noted Bryce DeWitt at the start of his September 1970 *physics today* article, "Quantum mechanics and reality."¹ His attempt to resolve the indeterminism question has resulted in a series of letters that appear to prove his opening statement right.

DeWitt's advocacy of the EWG view of reality (due to Hugh Everett,² John Wheeler³ and R. Neill Graham⁴), which pictures a universe that continually splits, so that each possible result of a measurement is equally "real," has been attacked on several points. Some writers believe that DeWitt's proposal is as arbitrary as are some earlier ones. Others discuss the difficulties presented by the "past" of such continually splitting universes.

We have chosen six letters that appear to represent the principal viewpoints. Here they are along with DeWitt's reply.

The formalism is not the interpretation

Leslie E. Ballentine
*Simon Fraser University
Burnaby, British Columbia*

DeWitt has given us a lucid account of the problem of measurement in quantum mechanics and of three quite inadequate attempts, two old and one new, to resolve the problem. The unsatisfactory nature of Eugene Wigner's method, which in effect supposes that the laws of atomic physics are drastically altered for those atoms that happen to make up a human brain, and of the Copenhagen interpretation with its

arbitrary "collapse of the state vector," need not be elaborated here because DeWitt is not defending them. I shall say more about the Everett-Wheeler-Graham (EWG) interpretation later. For the moment, I only suggest that the idea of infinitely multiplying, noninteracting worlds should be taken somewhat less seriously than the Ptolemaic epicycles. At least Ptolemy's theory "explained," in some sense, the one observable world without invoking infinitely many unobservable worlds.

My main purpose here is to point out that a fully viable interpretation of quantum theory, which encounters no paradoxes in the measurement process, was enunciated by Einstein at least as early as 1949.⁵ All three of the interpretations discussed by DeWitt assume that a state vector provides a complete description of an individual physical system (except for that of David Bohm, which I shall not discuss), this assumption being made tacitly and without any critical analysis in most cases. Of such interpretations Einstein observed⁶: "One arrives at very implausible theoretical conceptions, if one attempts to maintain the thesis that the statistical quantum theory is in principle capable of producing a complete description of an individual physical system. On the other hand, those difficulties of theoretical interpretation disappear, if one views the quantum-mechanical description as the description of ensembles of systems." The aptness of Einstein's observation even to the interpretations of Wigner and of EWG, which were invented after his death, is remarkable! I am surprised that DeWitt appears to be unaware of Einstein's interpretation, especially because he mentions Einstein's dissatisfaction with the Copenhagen interpretation.

I have discussed the statistical-ensemble interpretation of quantum theory at length elsewhere.⁷ To summarize briefly, a state vector, which is a function of the coordinates of some physical system, is an abstract mathematical representation of the result of a

certain state-preparation procedure. It describes the ensemble of all possible similarly prepared systems. Quantum theory, being a statistical theory, does not in general predict the outcome of a single experiment but only the statistical distribution of the results of an ensemble of similar experiments.

This statement is not altered if one takes as the "system" both the object of measurement and the measuring apparatus. The initial state vector, which is a product of a factor depending on the coordinates of the object and a factor depending on the apparatus coordinates (DeWitt's equation 2), represents an ensemble whose members are an independently prepared object and apparatus. If, in addition to this unspecified state preparation, we allow the object and apparatus to interact for a time, the ensemble will be described by a state vector (DeWitt's equation 5), that exhibits a correlation between the values of some dynamical variable of the object and of the apparatus (the "pointer" position). Once again quantum theory tells us the statistical distribution of the results of an ensemble of experiments. No paradox ever occurs as long as one consistently interprets the state vector as a description of an appropriate ensemble.

But if one has been unfortunate enough to have regarded the state vector as a description of an individual system, then the fact that the state vector is a superposition of terms corresponding to macroscopically distinct pointer positions will appear paradoxical. For surely Erwin Schrödinger's cat must be either alive or dead, not in some state of suspended animation awaiting someone to trigger a "collapse of the state vector."

When one observes a definite result in a single experiment (for example, that the cat is alive) there is no reason to suppose that the state vector changes from a superposition into one term of the superposition, much less to suppose that the world splits into several branches, because the state vector does

not describe a single experiment (or a single cat). But if one were to repeat an experiment of this kind many times and use the results to select a subensemble (for example, the subensemble of live cats), then this subensemble would properly be described by a reduced state vector (corresponding to this new form of state preparation involving measurement and selection). For further discussion of the statistical-ensemble interpretation see references 7 and 8.

I now offer some specific criticisms of the EWG interpretation as expounded by DeWitt. He begins by making a dubious distinction between the probability concept in quantum mechanics and in statistical mechanics, claiming that in quantum mechanics it represents "absolute chance," whereas in statistical mechanics it is only a measure of our ignorance. The "absolute chance" appears rather less than absolute when one remembers that quantum mechanics is compatible with a deterministic substratum of "hidden variables." But the more serious of the conceptual errors is the suggestion that probability in statistical mechanics is a measure of our ignorance. Are we to suppose that statistical mechanics would cease to be valid if Maxwell's demon were to whisper into our ears the values of the coordinates and momenta of all particles? Certainly not. Molecular-dynamics calculations for systems of many particles yield results in agreement with statistical mechanics, but Gibbs-ensemble theory is much more convenient whenever its formulas can be evaluated. In fact, statistical mechanics yields those *asymptotic laws* that become applicable in the limit of infinitely many particles and that in this limit become insensitive to the precise microscopic initial conditions. Our knowledge or ignorance of those details of the initial state is irrelevant.

The interpretation of probability that applies to all of statistical physics, including quantum theory, is this: Probability statements are statements about

the relative frequencies with which various outcomes of an experiment may be expected to occur in an ensemble of independent repetitions. The definition of the ensemble must specify which factors are to be held constant from one repetition to the next, and which are to be allowed to vary.

DeWitt's claim that the formalism by itself can generate an interpretation is unfounded and misleading. The most one can legitimately say is that certain aspects of the formalism may suggest a certain interpretation, but interpretive assumptions are always necessary. DeWitt assumes, for example (in common with other interpretations), that the variable s represents a particular system observable and the variable A represents a certain apparatus observable. He assumes that the state vector provides a direct picture of the world (a world of many noninter-

Quantum theory fails the single system

Philip Pearle

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Clinton, New York

DeWitt has given a clear exposition of three interpretations that can be grafted onto the formalism of quantum theory. There is a fourth interpretation, due to Einstein, that is not discussed in the article, and it is a point of view that appears to be at least as reasonable as those which were discussed.

According to Einstein,⁶ quantum mechanics is incapable of describing the behavior of a single system, such as "an electron." He argues that the state vector in the theory corresponds to an *ensemble* of identically prepared systems in nature. This is in marked contrast to the three interpretations discussed by DeWitt, which require the state vector to be in one-to-one correspondence with the physical state of a *single* system in nature.

The description of the measurement process is a major factor in determining the form that interpretations can take. The difficulty for "single-system" interpretations arises because the state vector that ought to correspond to the physical state of the system after the measurement is not the state vector that is the solution of Schrödinger's equation. Each interpretation can be characterized by its method of overcoming this difficulty. One may search for physical or mathematical mechanisms to "reduce" the state vector arising from Schrödinger's equation until it becomes the proper state vector to describe the system. Alternatively, with Everett and Wheeler, one may "expand" what one means by a system in nature until the system becomes something so large that it can appropriately be described by the solution of Schrödinger's equation: This is the point of view espoused by DeWitt. The third interpretation, due to Niels Bohr, maintains that the state vector does not describe the system as such but rather all possible measurements that can be performed on the system. This interpretation requires one to reduce the state vector after an actual measurement has taken place, because the outcome of subsequent possible measurements is affected by the disturbance of the system due to the actual measurement. In the Einstein interpretation, the state vector does not need to be reduced, provided one is sure to include the apparatus coordinates in the state

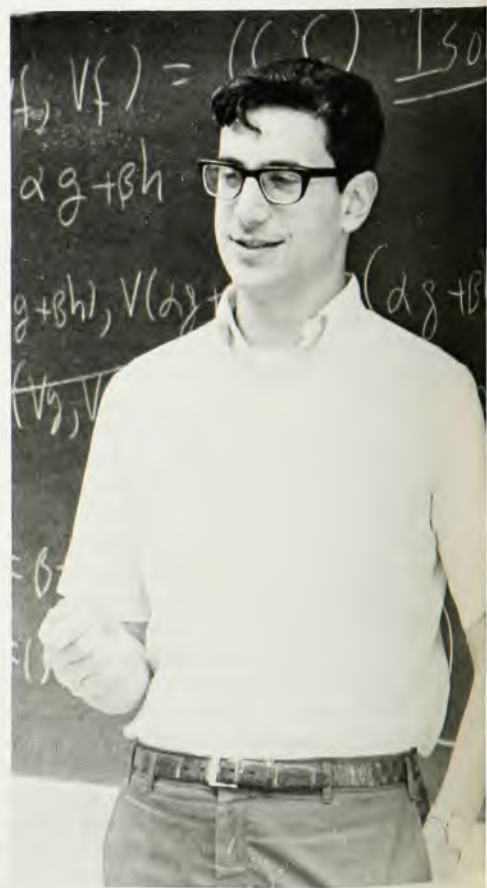
vector.⁹ There is no need for the Einstein-interpreted state vector to be adjusted in accord with the outcome of an actual measurement on a single system, because the state vector does not describe a single system.

None of these interpretations can yet be decisively accepted or rejected on the basis of an experimental test, so the question of which interpretation to choose becomes a matter of personal taste. To me, the "reducers" mechanisms appear to be rather artificial, and the "expanders" notion of a huge multiplicity of unobservable universes appears uneconomical. The Bohr interpretation suggests that there is no more to nature than what we humans can observe experimentally. Although this is an economical point of view, I find it hard to believe that the intricacies of nature are limited by the restrictions of interfacing with experimental physicists.

It is a consequence of the Einstein interpretation that, because we can observe a single system in nature and quantum theory can not describe it, quantum theory is not a complete theory. Einstein's notion that a more fundamental description of nature underlies quantum theory can be depressing, as no inkling of the form such a theory might take has yet appeared. Yet it is also a stimulating point of view, because it is one of the few indicators we have at present that nature may still have some remarkable surprises in store for us.

acting branches), rather than, say, a statistical representation of an ensemble. It is this assumption that yields the unusual features of the EWG interpretation, and one should not try to conceal that it is an *assumption* (and moreover, one for which attractive alternatives are available).

On a positive note, I expect that the mathematical part of DeWitt's article, the derivation of the usual probability assumption of quantum theory from an assumed measure in Hilbert space, will still be of interest if we discard the arbitrary (and in my opinion, silly) assumption that the world is splitting into many noninteracting branches. But to do this (and so avoid throwing out the baby with the bath water) we must reject DeWitt's unfounded claim that a formalism can dictate its own interpretation.



Consciousness as a hidden variable

Evan Harris Walker

Aberdeen Proving Ground
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The basis for the selection of one interpretation of quantum mechanics over another can be made on philosophical grounds—a preference for the “correspondence between formalism and reality” as opposed to an intervening “*a priori* metaphysics” in which “the state vector does not represent reality but only an algorithm for making statistical predictions”—but it would be preferable to make this choice on the basis of experimental results. To this end, I refer to a paper, “The Nature of Consciousness,”¹² whose results appear to have an experimental basis and suggest a solution to the question discussed by DeWitt, a solution that is essentially a combination of Wigner’s conscious observer and Bohm’s hidden variables. The paper does not deal specifically with the question posed by DeWitt, but

it indicates that conscious events are associated with, and serve as, hidden variables that cause the collapse of the state vector of every quantum-mechanical event.

The EWG hypothesis does require “one additional postulate to give physical meaning to the mathematics.” By Occam’s razor, it is immaterial whether the additional postulate is that required for the Copenhagen interpretation or for the EWG hypothesis. Both lead to some distressing conclusions: EWG requires the simultaneous splitting of the entire universe as a consequence of isolated (uncoupled) events, whereas Hooker¹³ argues that the Copenhagen interpretation leads to the “conclusion that, by changing our minds about which measurement to make” on one of two independent systems, “we are able to alter the state of the other system.”

DeWitt points out that Bohm’s and Wigner’s solutions to the problem require the suspension of the present formulation of quantum theory, without providing us with an alternative theory uniquely supported by experimental findings. Only if we assume that consciousness is associated with all quantum-mechanical events, that is, a combination of Wigner-like consciousness and Bohm-like hidden variables, does



any experimental evidence (neurophysical data), exist to support an interpretation of quantum mechanics. (Besides, being a cat lover, I want to object to the entire experiment!)

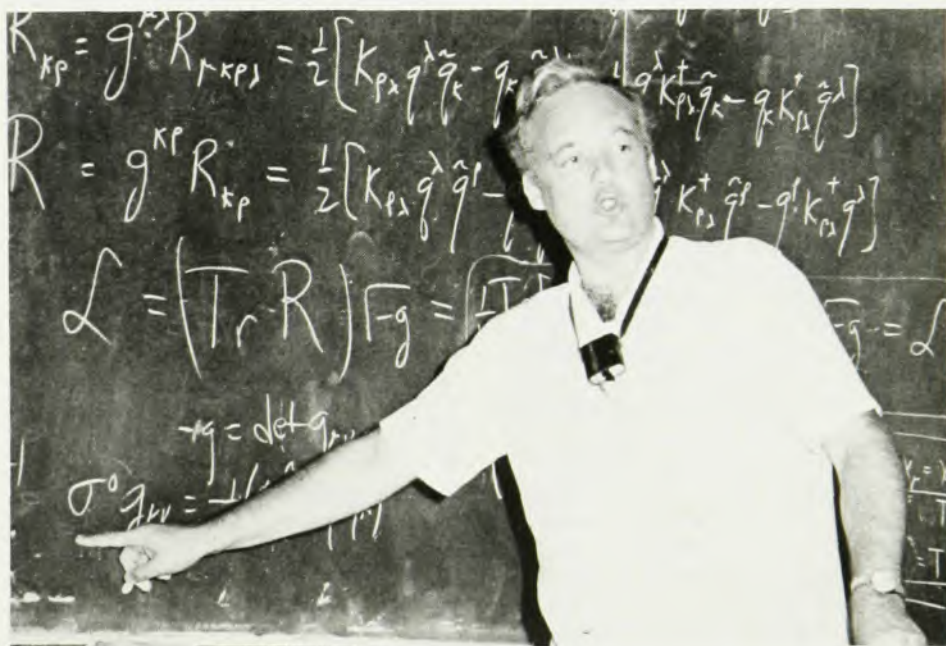
An alternative to quantum mechanics

Mendel Sachs

State University of New York
at Buffalo

DeWitt gives us a fascinating account of the recent interpretation of quantum mechanics by Everett, Wheeler and Graham. As he mentions, this is one of several contemporary interpretations of quantum mechanics that have been motivated by difficulties in interpreting the quantum formalism that have persisted now for about 45 years. There is another interpretation, not mentioned in DeWitt’s article, that I have been studying within my research program for the past ten years or so.

The EWG view appears to imply an astounding conclusion: A possible interpretation of the quantum formalism is that, instead of the intrinsically statistical approach, all possible outcomes of a quantum-mechanical measurement may in fact be realized. Each of these outcomes is in a separate but real world,



which, nevertheless, can not interact with other worlds. That is to say, with this view, a measuring apparatus that tells an inquirer a given electron is in New York can not “know” that the same electron is in Paris, Disneyland, Fiji, . . . , all at the same time. Extending Schrödinger’s cat paradox to this case, as DeWitt demonstrates,

would lead us to conclude that, not only the microscopic world, but the entire world we experience, is only one of infinitely many simultaneously existing worlds, each characterized by a different linearly independent state vector, with its associated constants of the motion. For example, at the moment of this writing such an inter-

pretation would claim that there is, in reality, a (Mendel Sachs)' who is writing a letter' in Buffalo, N.Y., expressing full belief in this idea of a "degenerate universe." It would also imply the (happy!) feeling that if an airline passenger were in an aircraft about to crash, he need not really worry, for in another (linearly independent) world, this same aircraft, with all of its passengers (including the happy one!) is duplicated and will indeed have constants of motion that predict it will land at home, safe and sound.

Even though such a model does not appeal to my personal physical intuition, I must agree that DeWitt's statements are consistent with his mathematics. On the other hand, I ask whether it is really necessary to go to such extreme lengths of straining physical sensibilities (admitting that here I speak for myself) to resolve the logical difficulties of the quantum theory.

From the results of my own research program, I feel I can (happily) answer this question by saying such extremes are not necessarily required. In addition to the EWG study, DeWitt mentions the study of Wigner, who attempted to incorporate human consciousness in the measurement for a "complete" description, and also Bohm's hidden-variable theory. The latter two studies share with EWG the idea that the formalism of quantum mechanics, and the same mathematical forms that relate to physical observables, must, in principle, be contained within the formalism of any new theory. In contrast the approach I have been taking does not contain the formalism of quantum mechanics in an exact sense. It is rather an intrinsically nonlinear, relativistically covariant field theory of a closed system. It does not generally entail solutions that belong to a Hilbert space or incorporate the linear-superposition principle, the uncertainty relations, and so forth, of the quantum formalism. Nevertheless, the formalism of this theory does asymptotically approach that of quantum mechanics in the appropriate limit (corresponding to sufficiently small energy-momentum transfer within the closed system that one starts with). In this limit, the closed system only seems to consist of distinguishable parts that are weakly coupled, but in fact there is no actual separation into distinguishable parts. Mathematically, this limit corresponds to the feature that the nonlinear aspects of the equations are never "off," even though we can consider them to be as small as we please.

The resulting formalism has thus far yielded solutions corresponding to several predictions that either are not predicted by the conventional quantum field theory or are predicted by the lat-

ter theory in an (admittedly) unsatisfactory manner from the point of view of mathematical consistency.¹⁰

Thus the approach to the resolution of the difficulty in quantum mechanics that I have taken is one with many important precedents in the history of physics. It is based on the idea of a correspondence principle. For example, Einstein's theory of general relativity asymptotically approaches Newton's theory of gravitation as spatial separations become small compared with astronomical dimensions. But Einstein's theory can not be said to contain Newton's theory, either conceptually

or mathematically, in either of their general forms.

Similarly the formal structure of non-relativistic quantum mechanics approaches that of classical mechanics as the quantities of action considered become large compared with Planck's constant. But $\hbar$ is not actually zero, and quantum mechanics does not contain classical mechanics; it only seems to do so as a mathematical approximation applied to special cases. These two theories are separate, conceptually and mathematically, even though their predictions are the same in the appropriate limit. In this sense, the predic-

Theory versus practicality

Toyoki Koga

Redondo Beach, California

DeWitt reports that Everett, Wheeler and Graham have completed a proof that the mathematical formalism of the quantum theory is capable of yielding its own (conventional) interpretation, without resorting to the collapse of a state vector. He remarks that this proof has the pedagogical merit of bringing most of the fundamental issues of measurement theory clearly into the foreground, and hence of providing a useful framework for discussion. He also adds

"... even Einstein might have accepted..."

On reading this article I thought it rather unfortunate that the conventional theory of measurement is to be sanctified farther beyond our experimental reach. As is pointed out by J. L. Park and Henry Margenau,¹⁴ the necessity of the theory of measurement in quantum mechanics arises because we have the peculiar situation that the state of a system is not defined directly in terms of variables to be observed, unlike the situation in classical mechanics. (According to Margenau's terminology variables of a quantum-mechanical system are latent, whereas variables of a classical-mechanical system are possessed by the system.¹⁵) As Louis de Broglie, Margenau, Landé and others have suggested repeatedly, the issue of "collapse" appears to provide a clue for reforming the conventional interpretation of quantum mechanics, so that it can regain vitality to deal with practical and evident dif-

The past as backwards movies of the future

Joseph Gerver

Berkeley, California

I question DeWitt's statement that in a finite universe there are only a finite number of independent "realities." This statement is true if one only considers those worlds that split off as one goes forward in time. However, if it is possible for the universe to split into two slightly different realities by a quantum-mechanical event, then surely it is equally possible for two slightly dif-

ferent universes to become identical in the same manner. Thus one should also see worlds branching off as one goes backwards in time (indeed, this conclusion is inescapable because of the time symmetry of Schrödinger's equation). Because one can keep following branches of the universe backward and forward in time indefinitely, there must be an infinite number of realities, or, at the very least, every possible reality must exist, including those in which the second law of thermodynamics is violated.

But now we can no longer say that we live in a "normal" or "typical" universe. For if we look at all possible branches going backward in time we discover that they look exactly like those going forward in time. That is to say, some of the branches look like the past that we remember, but the overwhelming majority look, more or less, like backwards movies of the future. (For that mat-

tions of my field theory contain those of quantum mechanics in the appropriate limit, although the theories are conceptually and mathematically apart.

DeWitt did not consider such an approach in his article because of his tacit assumption that the formal structure of quantum mechanics is, in principle, true. This is the same assumption that Wigner and Bohm make. (Bohm attempts to generalize the formalism of quantum mechanics by generalizing the space in which functions are mapped. But this is still a linear eigenfunction formalism that entails uncertainty relations, discreteness and so on, as in

ordinary quantum mechanics. In essence, then, he also maintains the formal structure of quantum mechanics, as DeWitt requires in his tacit assumption.)

Finally, I take issue with DeWitt's remark about the EWG approach as one that "even Einstein might have accepted." The features that Einstein anticipated in a fundamental description of matter were spelled out in his own writings. I have reviewed Einstein's anticipations in this regard in another publication,¹¹ and they were not at all contained in the type of theory that DeWitt discusses.

shrine itself in this manner. This doubt of mine is deepened by the following:

There are stable wavelets that satisfy the Schrödinger equation. A stable wavelet is distinguishable from the others by its definite trajectory. At the limit $\hbar = 0$, a wavelet yields the known representation of a classical material point. By superposing the representations of similar and free wavelets, we can obtain a de Broglie wave. Conventional eigensolutions are interpreted as stationary ensembles of wavelets. A similar interpretation is made with respect to the Dirac equation. If we accept William Duane's theory of particle diffraction instead of the conventional one,¹⁶ we can reinterpret quantum mechanics on a deterministic basis. The wavelet possesses the particle-like variables that we wish to measure. Thus the significance of the conventional theory of measurement is limited. Difficulties of quantum electrodynamics are most readily predictable from this point of view.¹⁷

ter, a small number of "atypical" future branches look like backwards movies of the past.) One might object that weak interactions are not time-symmetric; "everyday" life, however, depends mainly on electromagnetic and gravitational interactions. So perhaps, in most branches of the past, carbon-14 dates will come out wrong, but eggs will still unscramble.

This problem occurs of course, even without the hypothesis of multiple universes, but, interestingly enough, this hypothesis gives rise to a rather satisfying solution to the problem. If we accept multiple universes then we no longer need worry about what "really" happened in the past, because every possible past is equally real. Therefore, to avoid solipsism and insanity, we can, with clear consciences, arbitrarily define "reality" as that branch of the past that agrees with our memories.

DeWitt replies:

Bryce DeWitt

University of North Carolina
Chapel Hill

I apologize to the readers of *physics today* for not having indicated more explicitly in my article that I was confining my discussion entirely to those interpretations that either accept conventional quantum mechanics as a complete (I do not say *final*) theory or make fairly precise proposals for completing it. Having recently worked for the *American Journal of Physics* on a "Resource Letter on the Interpretation of Quantum Mechanics,"¹⁸ I was hardly unaware, as Ballentine implies, of Einstein's views. The point is simply that Einstein never came up with a scheme for completing quantum mechanics within his conceptual framework, and therefore I did not discuss his ensemble interpretation. Thanks are due to both Ballentine and Pearle for their summaries of his interpretation, which are clear and concise despite the fact that their respective conclusions appear to be contradictory. Pearle says that, within the framework of Einstein's interpretation, quantum mechanics is an incomplete theory. I agree. Ballentine appears to indicate that the Einstein interpretation is all that is needed. This I think is wrong, because we can, after all, make measurements on single systems.

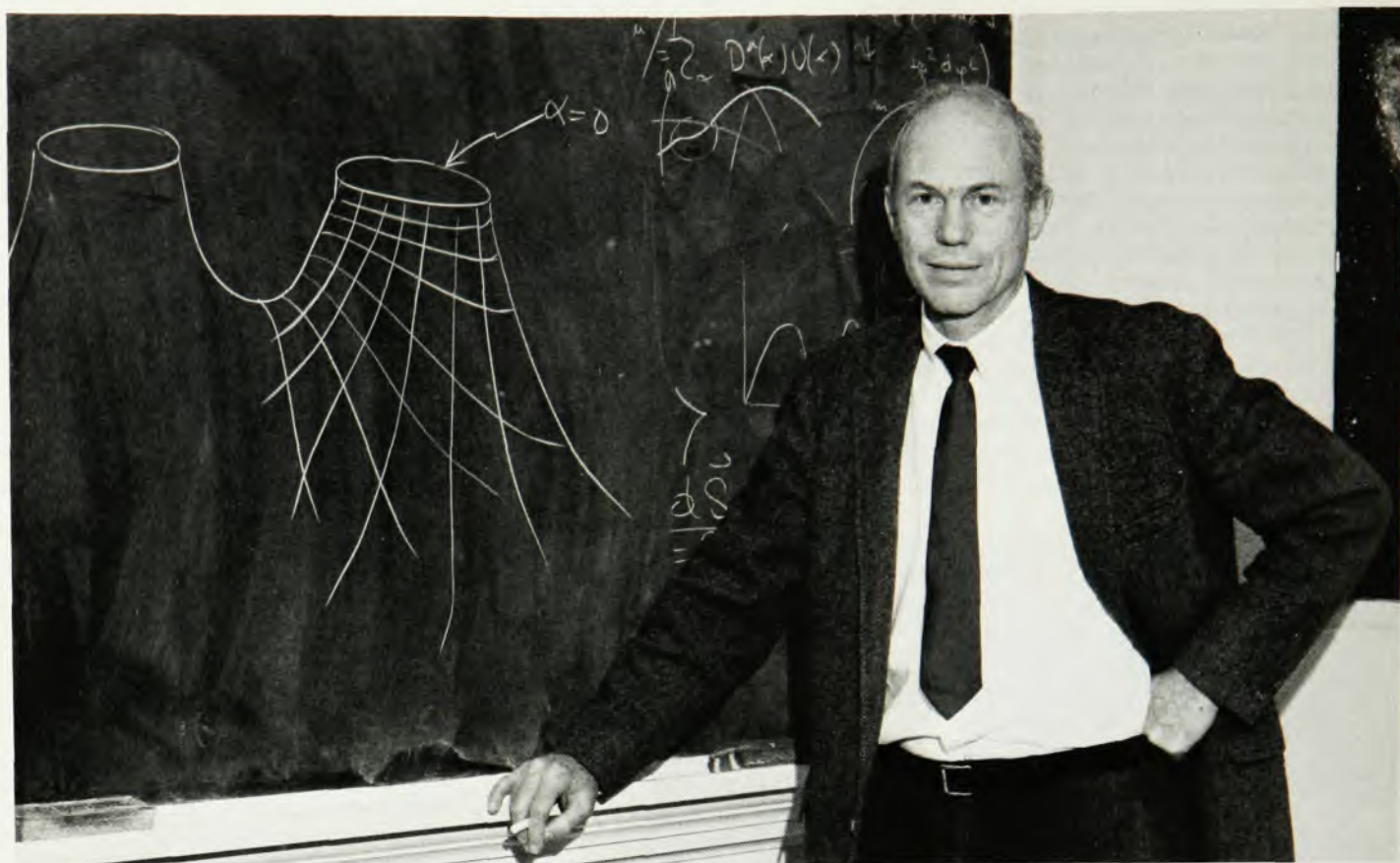
Ballentine objects to my statement that probability in statistical mechanics is a measure of our ignorance. Actually, Poincaré made that statement; I didn't originate it. But I'll stick with Poincaré. I completely agree with Ballentine that statistical mechanics would not cease to be valid if Maxwell's demon were to present us with values for all the canonical variables. However, we would then know much more than merely pressure, temperature, entropy and so on. We would possess information that would enable us to predict not only the mean value of microscopic fluctuations but also precisely when and where fluctuations of a given strength would occur. (An example might be the print-out of any high-speed computer calculation of the dynamics of a simulated particle-gas.) Analogous information is *in principle* unattainable in the world described by standard quantum mechanics. In this world chance is indeed *absolute*. This world may of course ultimately prove to be not the real world, but it is the world presently believed in by most physicists.

As an antidote to his belief that there



difficulties, such as in quantum electrodynamics. An innocent mind might be awestruck by the view of the vast and mysterious universe that unfolds in the EWG theory, and the issue would be attenuated, leaving practical difficulties unresolved. But I wonder if it is healthy for quantum mechanics to en-





is a single probability concept uniformly applicable to all of statistical physics, Ballentine should consult the works of such authors as Günther Ludwig, who distinguishes at least four different probability notions in quantum mechanics alone. His reference to the fact that the laws of statistical mechanics become rigorous for infinite systems is a red herring. Despite the undeniable elegance and importance of the C^* -algebra approach to statistical mechanics, none of us has even seen an infinite gas. And the real universe may, in fact, be finite.

While I am on the subject of infinity let me correct a slight mistake made by Sachs and Ballentine concerning the EWG interpretation. Both refer to the EWG state vector as splitting into an infinity of worlds. Perhaps they are only speaking figuratively; the number of worlds is actually finite—at least if the universe itself is finite. It is true that many observables have continuous spectra, but their values can never be determined with infinite precision. A measurement of such an observable does not cause the universe to split cleanly into an infinity of worlds, one for each eigenvalue. Because of limited space I could not discuss non-clean splits and imperfect measurements in my article. The subject is treated briefly in my Varenna lecture notes.¹⁹

I am also slightly unhappy at Sachs's reference to the same electron as being simultaneously in Paris, Disneyland,

Fiji . . . Although these electrons are all described by the same state vector and the same set of operators, they actually inhabit different, not the same worlds, and I prefer to think of them as different. So, when I'm on an aircraft about to crash I am *going* to worry. It's me I'm concerned about, not those other guys! In a more relaxed mood, of course, I am quite prepared to take my other selves seriously even if I can never know what they are doing. It is curious that Ballentine should compare them with Ptolemaic epicycles. Everett, in his thesis, compared the sensory testimony of those who claim the splitting idea to be absurd with that of the anti-Copernicans in the time of Galileo, who did not feel the earth move.

It is probably only wishful thinking, but I like to imagine that Einstein was sufficiently uncommitted to his program of completing quantum mechanics via some sort of nonlinear unified field theory that he might have been surprised and pleased at Everett's conception, which did not see the light of day, alas, until after Einstein died. For it is the only conception that appears capable of unifying general relativity in a profound way with the quantum theory, without changing either theory or adding any new formal elements. It is the only conception that, within the framework of presently accepted formalism, permits quantum theory to play a role at the very foundations of cosmology. (See the works on

quantum cosmology by Charles W. Misner and by myself.²⁰)

Both Sachs and Pearle rightly point out that the inadequacy of the conventional interpretation of quantum mechanics should be a stimulus to the discovery of some of the surprises nature still has in store for us. But both suggest that this will involve changing the rules (that is, the formalism). The trouble with this suggestion is that there are presently no rules about changing the rules. Sachs's own theory, for example, is just one of many change-the-rules schemes. Although rule-changes are certainly not without precedent in the history of physics, there are also important precedents for standing pat. Instead of tinkering with the formalism, why not ask what the formalism really says? Why not push it to its logical conclusion? This certainly paid off in the case of the Dirac wave equation and in the case of quantum electrodynamics.

Walker is distressed, as are most people who encounter EWG for the first time, by the splitting of the entire universe. He cites a paper,¹² which I have been unable to obtain in Chapel Hill or at neighboring institutions, that apparently tries to relate consciousness to quantum events. Because I do not know how the paper defines "consciousness" I can not guess how the quantum formalism gets into the act. "Consciousness" or "cognition" is certainly one of the most troublesome concepts that men have tried to grapple with.

As a definition that I believe is quite adequate for the purposes of physics, I recommend the one given by Leon Cooper and Deborah Van Vechten.²¹ Their definition is clear and precise, and I should of course mention that it fits the EWG interpretation beautifully.

Walker points out that both the EWG and Copenhagen interpretations require an additional postulate. What he fails to mention is that the world, in the Copenhagen interpretation, is merely *potential* (Heisenberg's word) before our observation of it, and only becomes *actual* afterward. Therefore it can not even exist without conscious automata being present. To EWG the presence or absence of such automata is irrelevant. The "postulate of complexity" cited in my article is needed only to give physical meaning to the mathematics. The world exists in any case. I can not imagine what good a quantum world would be without the complexity associated with consciousness, nor how one would attempt to describe it, but I see no good reason for doubting that it *could* exist.

This brings me to what I think is the most important philosophical question of all: *Is the mathematical formalism of the quantum theory really capable of yielding its own interpretation, assuming that the postulate of complexity holds? Is a positive answer to this question a valid metatheorem, as I have asserted in my article?* Ballentine says no. He says that a formalism can at best merely suggest an interpretation, and that interpretative assumptions are always necessary. In particular, he says that the notion that the world is splitting into many non-interacting branches is such an assumption, and an arbitrary one at that. But is it really so arbitrary? The number of distinct interpretations of quantum mechanics is, after all, rather small, and of these how many are there that don't change the rules? I challenge him to find another interpretation, besides Everett's, that takes the mathematical formalism as it stands, without adding anything to it, and that at the same time assumes that this formalism provides a complete description of quantum phenomena. The latter assumption, by the way, may be wrong, but it is *not* an interpretative assumption. It is merely abiding by the rules of the game. Drop it and you have either a different formalism or only a part of an as yet unknown formalism.

I do confess to having somewhat overstated the case in my article in implying that the EWG metatheorem has been rigorously proven. To be rigorous one would have to replace words like "system," "apparatus," "state," "observable," and even the statement of the metatheorem itself, by symbols that

are subject, together with the usual mathematical symbols of the quantum formalism, to certain formal rules of manipulation but empty of any *a priori* meaning. These words would acquire semantic content only *a posteriori*, after the consequences of the EWG postulates have been investigated. This remains a program for the future, to be carried out by some enterprising analytical philosopher.

Gerver's letter gives me a welcome chance to discuss a point that limited space prevented me from raising in my article—the question of time symmetry. If the temporal behavior of the wave function of the universe has a symmetry point, then indeed there will be worlds in which the arrow of time is reversed. These will be equal in number to those in which the arrow of time is parallel to our own.

I do not agree with Gerver, however, in his view of the past history of the universal wave function (not to be confused with our own past history, which involves only one branch). The overwhelming majority of past branches would look like "backwards movies of the future" only if the present state of the universe were the result of a fluctuation from a state of equilibrium in an infinitely old universe. But there are many reasons for supposing that the present state did not originate in this way and that cosmological boundary conditions must be considered.

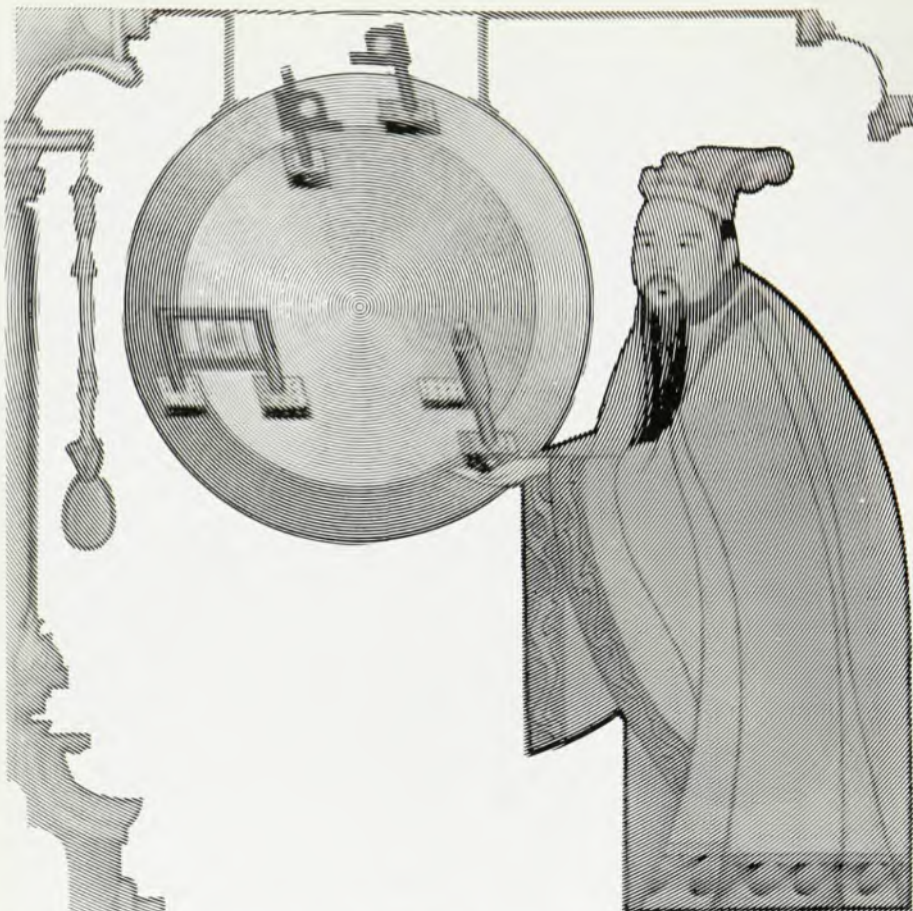
Currently the most popular cosmology describes the universe as exploding from a state of enormous, possibly infinite, density. What sort of wave function might be ascribed to such a universe? (I use the words "wave function" here rather than "state vector" because it is superfluous to postulate an entire super-Hilbert space for a universe that is described by a single "vector.") It must be a function of a very large number of variables (a complete set), which may be taken to be particle variables or field variables depending on one's point of view. If the universe is infinite, then the number of variables is infinite; if the universe is finite, then the number is effectively finite. (The infrared catastrophe disappears in a finite universe, and ultraviolet divergences are almost certainly damped out by gravity.) If we assume the universe to be finite, then, unless its topology is of a very special anisotropic form (the periodic box or three-torus), its lifetime is also finite; it ultimately reimplodes. This is a consequence of general relativity that was not brought out in my article.

Now, despite the gross time symmetry of the explosion-implosion behavior of the universe, and despite the time-reversal invariance, on the quantum level, of the grand Schrödinger equation (neglecting weak interactions),

there is no *a priori* reason why the wave function itself should possess a moment of time symmetry. (Gerver errs on this point.) It might, for example, represent a highly coherent state, with simple phase relations between all components, at the initial Big Bang, and a completely incoherent state at Final Collapse. Although the grand Schrödinger equation is completely deterministic, its solutions, because of the nonseparability of the grand Hamiltonian, are as strongly ergodic as those of the corresponding classical equations of Laplace. They too will possess their Poincaré cycles, but over spans of time vastly greater than those of the classical cycles, which in turn are vastly greater than the lifetime of the universe.

Suppose, however, we insist on a moment of time symmetry. One natural choice for this moment is the Big Bang itself. This choice implies that the universe had a history prior to the Big Bang, a history that is just the reverse of our own. Because the only difference between the two histories is the direction of the time arrow, it is a moot question whether they should be regarded as distinct. Therefore let us shift the moment of time symmetry to the moment of maximum expansion. Then, unless the universe is self-tuned to a highly improbable degree, neither Big Bang nor Final Collapse will find the universe in a coherent state. There will be worlds (branches) in which time flows one way and worlds in which time flows the other way and possibly a few maverick worlds. These worlds will be just as unaware of one another (absence of quantum interference effects) as the branches I discussed. In each case the number of branches into which a given world splits increases in the direction of time flow and not in the reverse direction, a rule that is violated only extremely rarely. Because of the overall time symmetry we can, of course, once again identify each world with a time-reversed counterpart. Only by destroying the exact symmetry (for example, by introducing the weak interactions) can we impart to the reverse worlds a truly independent "reality," a "reality," however, which differs from that envisaged by Gerver.

In reply to Koga I can only say that it did not occur to me, when I wrote my article, that I was enshrining anything. I was merely trying to take the conventional formalism of quantum mechanics as it stands and to push it to its logical conclusions without adding anything to it or modifying it in any way. Duane's "theory" is not a theory but merely a rule. Any attempt to elevate it to a deterministic theory must inevitably fall into the change-the-rules category of theories. Maybe one of these theories is right. I am certainly not opposed to attempts to build other theories. My



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point is simply that if conventional quantum mechanics is right then the EWG interpretation remains the most faithful to its formalism. I do not share Koga's belief that pursuit of interpretation questions is going to help us cure quantum electrodynamics of its technical ills.

In conclusion, let me say that I share Walker's sentiments about the cat. The experiment is Schrödinger's, not mine. In fairness, however, it should be pointed out that Schrödinger referred to the apparatus in which the animal is trapped as a "Höllmaschine."

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