DISCUSSION

MANY WORLDS ARE BETTER THAN NONE*

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The application of quantum theory to cosmology may make peculiar demands upon an interpretation of quantum mechanics. If it is now truly possible "to speak without embarrassment of the 'wave function of the universe,'" ([2], p. 1141), the time is at hand at which philosophers should devote some attention to these demands. Foremost perhaps is the demand that an interpretation be found in which observation and measurement assume a natural place entirely within quantum theory. In a recent article [1], C. J. S. Clarke has reviewed one such interpretation, the Everett-Wheeler-Graham (EWG) interpretation, or many-worlds interpretation.[1] Clarke also proposes an alternative interpretation of his own which retains the desirable feature of the EWG theory—the feature that the observer is part of the system—but dispenses with the ontologically undesirable feature of many worlds. It will be my purpose in this note to suggest some ways in which Clarke's alternative may be less suitable for cosmological purposes than the original EWG interpretation.

On Clarke's interpretation one defines macroscopically distinguishable (MD) states to be quantum mechanical states which "correspond to classical states, such as a live and dead cat, which can be clearly distinguished by casual observation," ([1], pp. 317-318). One also defines a classically interpretable (CI) state to be a state which is not a superposition of two or more MD states. Central to Clarke's interpretation is the "unique predecessor rule" which forbids confluences, or evolutions of two or more MD states into a single CI state. The only states representing physical reality are CI states, and Clarke attaches no interpretation at all to superpositions of MD states. If the unique predecessor rule is correct, any CI state which one measures is the result of at most one MD state. One does not therefore need to suppose that the universe splits into distinct branches whenever

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[1] For a presentation of EWG theory, see [3].

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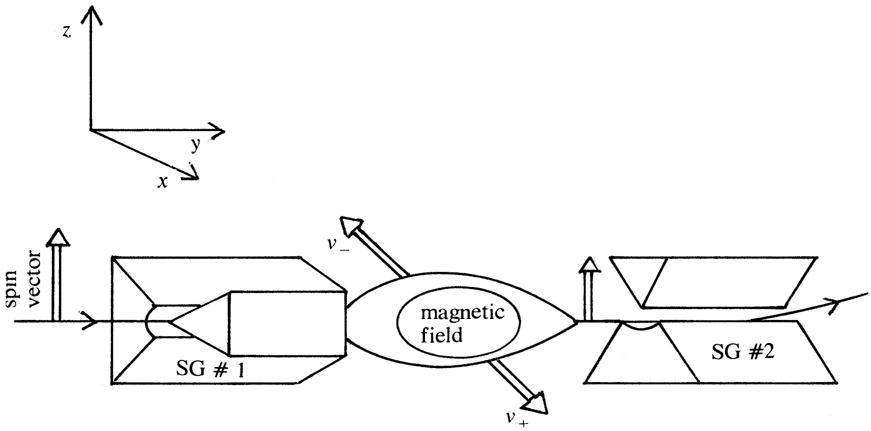
a measurement-like interaction takes place.

The difficulty with the above view is that it seems in principle possible for two or more MD states to evolve into a single CI state. An experiment, borrowed from Wigner [7], illustrates this point. Consider an incoming beam of atoms all of whose spins are in the $+z$ direction. This beam enters a Stern-Gerlach apparatus (SG #1) whose inhomogeneous magnetic field is in the x direction. This apparatus will split the incoming beam into two beams in one of which the atoms have their spins aligned along the $+x$ direction (spin state v_+) and the other of which they have their spins in the $-x$ direction (spin state v_-). If the magnetic field H_x of the apparatus is very strong and acts on the atoms for a very short time, the deflection of the two beams will be much greater than the spread of the wave packet of the atoms. In that case the two spin states v_+ and v_- will be macroscopically distinguishable in the sense that, if a measuring instrument (e.g. a fluorescent screen) were placed in the two beams, one could measure the spin state of an atom to be v_+ or v_-. If however, instead of a measuring instrument, one places a magnetic field between the two beams, one can recombine the two beams so as to restore the original spin state in the $+z$ direction. This recombination may be verified by having the single beam pass through a second Stern-Gerlach apparatus (SG #2) whose field is in the z direction. This second apparatus will deflect the beam upward in the $+z$ direction. The last result would be impossible to explain unless one regarded the atoms between the two Stern-Gerlach apparatus as being in a *superposition* of the two spin states v_+ and v_- since interference effects between these two states are required to account



for the reproduction of the original spin state. (If the atoms were simply in a *mixture* of the two spin states v_+ and v_-, no interference effects would be present, and the atoms could not be restored to the original spin state. This could be verified by noting that some of the atoms would be deflected downward in the $-z$ direction by SG #2.) One thus faces a situation in which two MD states, v_+ and v_-, evolve into a single CI state, in violation of the unique predecessor rule.

It might be objected that v_+ and v_- do not truly represent MD states since no measuring instrument, *ex hypothesi,* is placed in the beams between SG #1 and SG #2. In this sense the above Stern-Gerlach experiment does not constitute a measurement of v_+ and v_- since no device has registered and recorded these states. In reply, one might first note that "macroscopically distinguishable" does not ordinarily entail "macroscopically distinguished." Furthermore, while it is true that if a measuring instrument were placed within the beams, it would be extremely unlikely that any interference effects between MD states would manifest themselves, "extremely unlikely" does not mean "theoretically impossible."[2]

To say that, after measurement, interference effects are unlikely is to say that the trace of the square of the probability density matrix ($tr\rho^2$ where $\langle x'|\rho|x\rangle \equiv \Psi^*_{(x)}\Psi_{(x)}$) for a superposition of MD states is almost equal to the trace of the square of the density matrix ($tr\hat{\rho}^2$) for a mixture of these same states.[3] Since $tr\rho^2 \cong tr\hat{\rho}^2$ for systems involving interactions with complex, macroscopic objects such as measuring instruments, one may consider a given CI state after measurement to have evolved from at most one MD state in a mixture. In this sense the unique predecessor rule is correct. However, the theoretical possibility of interference effects from a superposition of MD states is always present even after measurement. This is so because for a superposition $tr\rho^2 = 1$, whereas for a mixture $tr\hat{\rho}^2$

[2]The possibility of such interference effects has been explicitly recognized by Loinger ([5], pp. 245-246) when he writes, with reference to an earlier paper by Daneri, Loinger, and Prosperi that "Actually, we only stated that we had proved, making essential use of ergodic theory, that *for all practical purposes* a macro-observer may describe the behavior of a global system, formed of micro-object plus macro-apparatus, at the end of the measuring process by means of a given mixture. More precisely, we proved that in the formal expression of the probability that the apparatus is found at the end of the measurement in one or in another of the possible macro-states, the 'interference terms' are *practically* absent . . . Of course, we did not assert that superpositions of vectors corresponding to different macroscopic states are impossible. Indeed, this possibility is firmly rooted in the formal structure of quantum theory and cannot be eliminated."

[3]For a discussion of measurement problems in terms of density matrices, see [4], pp. 174-189.

< 1, and the trace is a constant of motion. Limiting ourselves to the evolution described by the Schrödinger equation, a superposition can never evolve into a mixture nor vice-versa. To adopt the unique predecessor rule is to discard a theoretical possibility, however unlikely, which is part of the structure of quantum mechanics.

On at least one other ground, Clarke's interpretation would appear to be less acceptable than the EWG interpretation. As Clarke admits ([1], p. 331), his alternative gives us no picture of the way a system evolves from moment to moment. The overall wave function of the system seems to be solely a device for the prediction of CI states; it has no descriptive content when superpositions of MD states are involved. On philosophical grounds, those of us who would like to adopt a realist, rather than an instrumentalist, view of our most fundamental scientific theories should find this unfortunate. I think this feature also makes Clarke's interpretation a less than happy choice for cosmology. An interpretation suitable for cosmology should apply not just to the universe we presently inhabit but also to simpler, idealized models and to very early stages of our own universe where a high degree of homogeneity may have prevailed. Quantum cosmologists are already engaged in the study of simple models of the universe (e.g. the Friedmann model) which contain only a cloud of particles, represented by an ideal fluid.[4] In such situations the complexity of matter may not be sufficient to render unlikely the evolution of MD states into a single CI state. Further, the price of giving no interpretation to superpositions of MD states may be that we shall have no description of what is happening in such cases.

In the last section of his paper, Clarke appears to be sensitive to this objection.

> On this [Clarke's] approach we cannot say that the universe evolves through some sequence of conditions in the way in which one could before the advent of quantum theory. But we often feel that it should be possible to explain just how the universe gets from one configuration to another, though such an explanation cannot be given in my formulation of the last section. ([1], p. 331)

But Clarke then denies that the EWG theory gives us a "detailed picture" of the evolution either, since it gives us no "mechanism" to explain the splitting of the universe ([1], p. 331). In reply, it is not clear that a mechanism should be sought here. The splitting is described by the basic dynamical law of quantum mechanics (i.e.,

[4] See, for example, [2] and [6].

the Schrödinger equation). We may simply have to accept the splitting as a fundamental process, not explicable by any mechanism. (An analogy perhaps exists in the Special Theory of Relativity where we accept, e.g., time dilation and length contraction as fundamental and do not seek mechanical explanations for these processes.[5])

In sum, both the unique predecessor rule and the instrumentalist view of the wave function would seem to render Clarke's interpretation less acceptable for cosmological purposes than the EWG interpretation. The ontological price of many worlds may be high, but better many than none.

REFERENCES

[1] Clarke, C. J. S. "Quantum Theory and Cosmology." *Philosophy of Science* 38 (1974): 317-332.
[2] DeWitt, B. S. "Quantum Theory of Gravity. I. The Canonical Theory." *The Physical Review* 160 (1967): 1113-1148.
[3] DeWitt, B. S. and Graham, N. (editors). *The Many-Worlds Interpretation of Quantum Mechanics*. Princeton: Princeton University Press, 1973.
[4] Gottfried, K. *Quantum Mechanics I*. New York: Benjamin, 1966.
[5] Loinger, A. "Comments on a Recent Paper Concerning the Quantum Theory of Measurement." *Nuclear Physics* A108 (1968): 245-249.
[6] Ryan, M. P. and Shepley, L. C. *Homogeneous Relativistic Cosmologies*. Princeton: Princeton University Press, 1975.
[7] Wigner, E. P. "The Problem of Measurement." *American Journal of Physics* 31 (1963): 6-15.

[5] I am indebted to Joe Chassler for suggesting this analogy.