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October 15, 1957

Dr. Hugh Everett, III
Weapons Systems Evaluation Group
The Pentagon
Washington, D. C.

Dear Dr. Everett:

Some time ago you were kind enough to write to me with some criticisms of my version of statistical mechanics, which I found to be helpful in showing the equivalence of several different viewpoints. With the appearance of your very interesting paper in the current Reviews of Modern Physics, I find it possible to reciprocate. Doubtless the following remarks have been, or will be, made also by others, but I offer them anyway.

Ever since I was a graduate student in Princeton, I have been convinced that the present form of quantum mechanics, if applied really consistently, would force us to do exactly what you have done. However, I never had the courage to think out the details as you have, because it seems at first glance that this 'splitting of the observer' is a <u>reductio</u> <u>ad</u> <u>absurdum</u>. It is a real pleasure to see that this is not so; on the contrary, when we do this, quantum theory is not only simplified conceptually (there is only a single process by which the wave function changes) but also improved logically (the statistical postulate is a consequence of the superposition principle). It seems fair to say that your theory is the logical completion of quantum theory, in exactly the same sense that relativity was the logical completion of classical theory. I have psychological reasons for considering it very important, because in two different ways it fits in beautifully with some work of mine on interpretation of quantum mechanics.

The only part of your paper which I am unable to follow is the argument which leads you to decide that $m(a_i)$ can depend only on the magnitude of a_i. It seems to me that, since the Schrödinger equation determines a definite phase for a_i, it could in principle be involved in the probability measure. When you say that the wave function ψ is the basic physical entity, this might be taken to imply that its absolute phase has a definite meaning. Whether or not you intended this to be so, the <u>relative</u> phases of the a_i are still definite 'physical' quantities. Of course, in terms of my 'subjective' probability, the equation $m(a_i) = m(|a_i|)$ says only that we have no

information about the phases, and is quite acceptable; but I think you intended your measure to have more than just a subjective meaning.

Your theory is similar in a vague way to my model of an irreversible process (a semiclassical version of which will be in the October 15 Phys. Rev.), but I have not yet decided just how close the correspondence is. Here we have a sequence of external observers σ_1, σ_2, --- σ_n, --- who communicate with each other in succession about the state of a (system + heat bath) which is 'in reality' just carrying out its motion according to a single Schrödinger equation. Fundamentally, there is no such thing as a 'quantum transition,' and the entire (system + heat bath) remains in a pure state for all time. nevertheless the <u>illusion</u> of transitions appears, because when σ_n communicates with σ_{n+1} he withholds the information he has about correlations between the two systems. The density matrix $\rho_n(t)$ which represents the state of knowledge of σ_n then approaches the Boltzmann distribution describing thermal equilibrium of the two systems, 'almost always' in the limit $n \to \infty$. The intermediate stages represent just the equations which one finds in the current theories of irreversible processes. In this way we see that the increase of entropy cannot be the really essential thing--observer σ_1 retains for all time a far more detailed knowledge about the true state of the (system + heat bath) than the other observers, and for him the entropy is always zero. His additional information surely cannot put him in a <u>worse</u> position to make predictions; but the surprising thing is that except in a set of cases of measure zero, σ_n, who ascribes the system a much higher entropy, is nevertheless able to make just as good predictions as σ_1, as long as they restrict their predictions to macroscopic properties. Therefore in a description of [redacted] irreversible processes an entropy increase is not <u>necessary</u>; the point is only that it is <u>permissible</u>, because the discarded information was irrelevant to macroscopic behavior.

Now suppose that what I called the 'heat bath' is renamed 'the observer.' Let the observer make repeated measurements of various noncommuting quantities in the 'system,' and consider all the different branches of the total wave function, after a very large number of measurements. We should find in the limit $t \to \infty$ that the collection of all states of the system in all these branches at time t approaches 'almost always' a generalized Gibbs ensemble, defined by the condition that it has maximum entropy subject to whatever constraints are imposed by the uniform integrals of the motion. I think the existing theory of Markoff processes will be adequate to prove this.

There is, however, one difference between my irreversible process and your quantum-mechanical measurement process which I find disturbing. This concerns the fact that the observer may have a volition; i.e. he may decide which quantity to measure <u>after</u> some of the interactions have already taken place. This is just the fundamental cause of Einstein's most serious objections to quantum theory,

and it seems to me that the things which worried Einstein still cause trouble in your theory, but in an entirely new way. At each interaction the observer's wave function $\psi^O[\ldots]$ splits into several different 'Riemann surfaces,' the $\psi^O[\ldots\alpha_i]$ being in general different functions, but still functions of the same variables. Now suppose we build into the observer a definite strategy, of the form, 'If measurement of α in system S_1 gives the result α_i, then I will next measure β in system S_2. If, on the other hand, it gives α_j, then I will measure γ in S_3.' Just before the second measurement we have a wave function

$$\psi = a_i\phi_i^{S_1}\psi^{S_2}\psi^{S_3}\psi^{O}[\ldots\alpha_i]$$

$$+\, a_j\phi_j^{S_1}\psi^{S_2}\psi^{S_3}\psi^{O}[\ldots\alpha_j]$$

$$+ \ldots$$

When the second measurement starts, the part of ψ on the first line undergoes an interaction coupling S_2 and O, while that on the second line sees one coupling S_3 and O. After the second measurement we have a wave function

$$\psi' = \sum_k a_i b_k \phi_i^{S_1}\phi_k^{S_2}\psi^{S_3}\psi^{O}[\ldots\alpha_i\beta_k]$$

$$+ \sum_r a_j c_r\, \phi_j^{S_1}\psi^{S_2}\phi_r^{S_3}\psi^{O}[\ldots\alpha_j\gamma_r]$$

$$+ \ldots$$

But the transformation $\psi \rightarrow \psi'$ is not the result of an equation of motion $i\hbar\dot{\psi} = H\psi$ with any single Hamiltonian. The terms of the first and second lines are function of exactly the same variables, and an operator which acts on the total wave function in such a way that it couples S_2 and O in the first line should do the same thing in the second. Thus, it seems to me that it is impossible in principle to describe the most general measurement process by a single Schrödinger equation. If we maintain the equation $i\hbar\dot{\psi} = H\psi$ as fundamental, we restrict ourselves to the case where the same Hamiltonian acts simultaneously on all branches of the wave function; i.e. the observer has lost the ability to choose different measurements on the basis of what is in his memory. In effect, the Hamiltonian is once more 'imposed from the outside.'

I am not sure that I have exhausted every possibility of other interpretations of the transition $\psi \rightarrow \psi'$. In any event, I would very much like to hear your reactions to this objection. However, assuming that it turns out to be valid, what conclusions can we draw? Probably the most obvious is that by a slight generaliztion

we can still maintain the basic viewpoint of your paper. Not only the states of the observer, but also the possible Hamiltonians, continually branch off into the various possibilities. Perhaps this is the right answer.

We would, however, certainly like to keep the theory in the more unified form you gave it, and I do not see at the moment how this can be done if we allow the observer to have a volition. I find it satisfying that the same situation causes trouble in both formulations of quantum theory, and intepret it, tentatively, as one more confirmation of Bohm's conclusion that Einstein's objections to quantum theory have never been answered satisfactorily. There is still something fundamentally wrong in quantum theory, and we will not find it merely by inventing more fancy mathematics.

In any event, your reformulation will certainly make it much easier to analyze the essential content of quantum theory, and incidentally will make quite a few of the standard remarks in textbooks obsolete!

Very truly yours,

E. T. Jaynes
Associate Professor
Microwave Laboratory

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