

THE UNIVERSITY OF NORTH CAROLINA
CHAPEL HILL

DEPARTMENT OF PHYSICS

M.B.S. pls send
photocopies to Everett
at return time to me
Jen 17 May
May 7, 1957

Professor John A. Wheeler
Palmer Physical Laboratory
Princeton University
Princeton, New Jersey

Dear John,

You had asked for comments on Everett's paper "On the Foundations of Quantum Mechanics," and, although preoccupation with other matters has kept me from meeting the May 1st deadline, I thought you might nevertheless be interested to receive an evaluation from Chapel Hill. I have studied the paper and your assessment of it rather carefully, and I have several remarks to make:

In the first place, it seems to me that the professional philosopher will have a greater appreciation of Everett's work than will the average physicist, at least for the present. I say this as a matter of perspective, for it seems extremely unlikely that current physics (including quantum gravodynamics!) will be much affected by the new point of view. On the other hand, since the days of Boltzmann and Poincaré it has become increasingly clear that physicists themselves are obliged to be their own epistemologists, since no other persons have the necessary competence. Therefore Everett's effort is to be praised.

Of the total lack of experimental motivation facing such an effort Everett is doubtless keenly aware. Even Einstein, in developing the special theory of relativity, was motivated by experiment as well as by a general world view. Consequently, his theory had an immediate impact upon experimental physics as well as upon philosophy. It is hard for me to believe that Everett's ideas, on the other hand, will appreciably affect experimental physics (even on a cosmical level) although this, of course, remains to be seen.

In any event it is not at this point that a criticism of Everett's work should be aimed. Even if his ideas should have no experimental consequences whatever, his work would still be valuable. It is important to clarify the world views underlying the structure of theoretical physics and to point out inconsistencies, wherever they occur. This is largely the motivation of the whole general attempt to quantize the gravitational field, even at a much more pedestrian level -- the lack of experimental data is identical.

As far as it goes, a parallel can validly be drawn between Everett's ideas and the theory of relativity. Your use of the term "relative state", in fact, seems to be a conscious effort to draw such a parallel. The role of the observer relative to the rest of the universe is emphasized in both theories.

Before coming to the real criticisms which I have of Everett's ideas, I should like to analyze this parallelism somewhat further by pointing out two of its aspects which seem to me rather suggestive, ^{and at which you yourself have} ^{pointed, without} ^{going into detail.} The first is an aspect of historical parallelism: The conventional interpretation of the formalism of quantum mechanics in terms of an "external" observer seems to me similar to Lorentz's original version (and interpretation) of relativity theory, in which the Lorentz-Fitzgerald contraction was introduced ad hoc. Everett's removal of the "external" observer may be viewed as analogous to Einstein's denial of the existence of any privileged inertial frame. The analogy fails, however, when one compares the short space of time which elapsed between Lorentz and Einstein with the three decades during which quantum mechanics, with its conventional interpretation, has managed to get along quite well. In answer to this, one may say that it was the pressure of experimental physics which in the case of relativity theory forced the application of "Occam's razor" and allowed the introduction of Minkowski's formalism, which led in such a naturally suggestive way to so many beautiful notions (including general relativity), whereas in Everett's case no such pressure exists. But does Everett's thesis constitute an entirely valid application of Occam's razor? Of this I am not so sure.

The second aspect of parallelism which struck me lies somewhat deeper: Consider the general theory of relativity. This is a physical theory based on Riemannian geometry, which in turn is a mathematical theory of continuous transformations of sets of continuous variables and of the idealized spaces of idealized points defined by such sets of variables. The use of such a geometry to describe the physical world has its basis in "the rough data of our senses" and in our crudest experiments. (Poincaré, "Science and Hypothesis.") The mathematical continuum from which the elements of this geometry are taken is, as has been pointed out many times, by no means identical with the physical continuum with which we start. However, we are obliged to introduce it in order to avoid the intolerable contradiction $a = b$, $b = c$, $a < c$ inherent in the description of any sequence of "events" a , b , c of the physical continuum which are sufficiently closely spaced so that a is experimentally indistinguishable from b , and b from c , although a is distinguishable from c . By removing the contradiction the mathematical continuum is able to serve as a good model of the physical world; it permits the results of experiments to be expressed in mathematical terms, and once this is possible, we may be tempted to suppose, the metaphysical problem ends.

However, there is more to the matter than this. Not only crude observations but also our most refined experiments are describable within the framework of a mathematical continuum. Moreover, this

continuum is found to be Euclidean to a very high degree of accuracy, when one follows the natural procedure of using solid bodies in space and reproducible regularities in time to define the metric. It is owing to the existence of the symmetries and regularities implied thereby that one is able to make what may be called a "good" observation. (I do not insist on the approximately Euclidean nature of space-time; other types of symmetry might do as well.)

At this point Einstein enters the picture and, with the general theory of relativity, replaces the solidity of the Euclidean framework by the shifting sands of general coordinate transformations. The mathematical continuum which originally sprang from attempts to describe primitive sensory data is, however, still retained — this, in spite of the fact that the level of description at which the general theory of relativity aims is vastly more sophisticated. On the other hand, the sensation of chaos produced by the introduction of general coordinate transformations makes the mathematical continuum seem a much less real thing than before, and tempts one to wonder if it could not be dispensed with entirely. Solid ground is only regained when the "observer" is given the dominant role in the theory. If you ask what the result of a given experiment will be, general relativity will (or at least should) always give you a definite answer — although a simple scheme may not always be ready at hand for classifying the answer. (Incidentally, Misner's search for a "superpotential" and Bergmann's — and my own — search for "true" observables, are nothing but attempts to discover such a scheme for purely gravitational effects.)

In this way you will find that general relativity, at the classical level, contains its own theory of measurement. It will, in fact, give you back again the rough data of the senses from which its mathematical formalism sprang. This, I think, is abundantly clear already from the well known linear approximations of the theory, and should persist at an even more advanced level. But it is a result which was certainly not obvious at the outset. It is not merely a tautologous consequence of the original mathematical removal of the contradictions inherent in the continuum of physical experience. The fact that there exist covariant nonlinear equations which guarantee the self-consistency of the theory is striking indeed.

The parallel with Everett's theory is now plain. Although quantum mechanics is founded on experimental results which are somewhat remote from primitive experience, the inferences to be drawn from them are clear and unambiguous: Among the many statistical aspects which Nature displays, certain ones are of a fundamental, irreducible, "built-in" variety. We describe this situation by introducing a wave function ψ which has a certain probability interpretation. The results of experiments may be expressed — now on a statistical basis — in terms of this wave function, provided it be assumed to satisfy a certain linear differential equation, the Schrödinger equation, the existence of which is just as striking as that of Einstein's equation. It is not initially supposed that ψ expresses an independent physical reality any more than do the numbers which designate a point of the mathematical continuum. In fact, ψ suffers discontinuous changes depending on the amount of our knowledge (or that of an observing apparatus) about a system.

Page Four

However, as Everett has shown, when the mathematics of Hilbert space (of which Ψ is a "point") is combined with the Schrödinger equation, the whole scheme is found to possess more features of independent reality than were initially apparent and to mirror the physical world with a previously unanticipated fidelity.) In short, the scheme is found to contain its own theory of the measurement process, by giving back again — but at a new level — the same elements of statistical interpretation which were put into it at the beginning, just as the combination of the mathematics of a Riemannian space with Einstein's equation gives back again (in first approximation) the laws of motion from which one can construct the rigid Euclidean codification of distances and times, which in turn contains, incidentally, the physical interpretation of the mathematical continuum, i.e., everything that is pertinent for a description of the physical continuum. The parallel occurrence of this phenomenon of self-consistency, or "self-containment", in two quite different branches of physics is rather remarkable and deserves further study.

Now for the criticisms: The first is a major one and cuts, I think, to the heart of any controversy which is likely to arise over Everett's work. It concerns the question of what is meant or can be meant by the word "correspondence" — a better word would be "isomorphism," but you seem to have avoided using it — particularly when applied to the ensemble of Everett's relative state vectors $\Psi^0[A, B, C, \dots]$ as compared with the experience of a real physical observer. I think the history of the development of knowledge during the past century has so thoroughly conditioned the modern physicist that he will be quite willing to go along with you when you say that "terminology has to adjust itself in accordance with the kind of physics that goes on." However, there are limits.

Certain terms, such as "isomorphism", would seem likely to be useful under almost any conceivable circumstances and yet retain meanings which are held within fairly rigid bounds. I am afraid that it is at precisely the most crucial point in Everett's argument where many people, including myself, will be unable to swallow your implication that the word "isomorphism" applies.

It has been many years now since the logicians first began defining the natural numbers as "classes of all classes having so-and-so many members." I don't object when, advancing farther in the same direction, a philosopher like Russell ("The Analysis of Matter") defines a "point" (in space-time) as a certain class of events, or an "event" as a certain group of observations (both "real" and "virtual"). I understand what an isomorphism is, and am prepared to agree that for many practical purposes the word "isomorphism" may be replaced by the word "identity", and, indeed, that if there are any differences be-

or inanimate. As Everett quite explicitly says: "With each succeeding observation ... the observer state "branches" into a number of different states." The trajectory of the memory configuration of a real physical observer, on the other hand, does not branch. I can testify to this from personal introspection, as can you. I simply do not branch.

I do agree that the scheme which Everett sets up is beautifully consistent; that any single one of the states $\Psi^0 [\alpha_1^1, \alpha_1^2 \dots]$, when separated from the superposition which makes up the total or "universal" state vector $\Psi^{S_1 + S_2 + \dots + 0}$, gives an excellent representation of a typical memory configuration, with no causal or logical contradictions, and with "built-in" statistical features. The whole state vector $\Psi^{S_1 + S_2 + \dots + 0}$, however, is simply too rich in content, by vast orders of magnitude, to serve as a representation of the physical world. It contains all possible branches in it at the same time. In the real physical world we must be content with just one branch. Everett's world and the real physical world are therefore not isomorphic.

The central difficulty in interpreting quantum mechanics lies, of course, in the notion of "probability", that most elusive of all concepts with which mathematicians and physicists have to concern themselves. For Poincaré, who did so much to elucidate the delicate ideas underlying probability theory, "chance" was strictly "the measure of our ignorance." He writes ("Science and Method"):

"What is chance? The ancients distinguished between the phenomena which seemed to obey harmonious laws, established once for all, and those that they attributed to chance, which were those that could not be predicted because they were not subject to any law. In each domain the precise laws did not decide everything, they only marked the limits within which chance was allowed to move. In this conception, the word chance had a precise, objective meaning; what was chance for one was also chance for the other and even for the gods.

"But this conception is not ours. We have become complete determinists...."

One would give anything to know what Poincaré would have said if his life span had been shifted the few years necessary for it to have encompassed the advent of the theory of Heisenberg, Schrödinger and Bohr. I, for one, do not think he would have felt obliged to return to the absolute chance of the ancients. I think he would have resisted it, just as Einstein did.

The point at issue is quintessentially summed up in a single phrase of Everett's — in the section headed "Quantitative Interpretation, Measure....etc." — :

"Probability theory is equivalent to measure theory mathematically...."

Yes, but not epistemologically. There is a vast difference between the two. The trouble with quantum mechanics is that it drives us, at least formally, perilously close to the absolute chance of the ancients. I say perilously, because if you accept the concept of absolute chance,

and attempt to describe it mathematically, you have no choice but to do what Everett has done, namely, to introduce a mathematical world which branches. But the real world does not branch, and therein lies the flaw in Everett's scheme. Of the seductive nature of the path which Everett has taken there can be no doubt. It is the natural extension of the route which has led us to the triumphs of quantum mechanics, and which has its foundation in the absoluteness of the indeterminacy principle. But at the end of the trail one comes suddenly upon a vast contradiction.

Two other paths remain open for avoiding the contradiction. One is the traditional path espoused by Bohr and the Copenhagen school. According to Bohr the wave function Ψ has an interpretation only with respect to an "external" classically describable observer. This observer is not a member of an ensemble, nor is the observation he makes one of an ensemble of observations; both stand alone. Therefore it is meaningless to apply the mathematics of probability to the observer or his observation, although it is quite validly applied to the underlying world observed. Although Bohr's observer can never predict with certainty the outcome of his observation, he nevertheless always obtains only one result.

The existence of a classical level of description, although not always explicitly stated, is absolutely essential to the Copenhagen view. This fact is most forcefully expressed by Dave Bohm at the end of his book on quantum theory:

"Without an appeal to a classical level, quantum theory would have no meaning..... Quantum theory presupposes the classical level and the general correctness of classical concepts in describing this level. It does not deduce classical concepts as limiting cases of quantum concepts."

Such a statement, coming a few pages after his lucid analysis of the process of measurement (the results of which form the point of departure of Everett's work!), shows that Bohm saw the situation very clearly. It was a situation which bothered him, however, just as it has bothered Everett; and, as we all know, it eventually led him to abandon the Copenhagen view in favor of his theory of "hidden variables", which is the other path that may be taken in order to avoid the previously mentioned contradiction.

I think Heisenberg is wrong, at least as far as Bohm is concerned, in saying of the opponents of the Copenhagen interpretation, (in his article in "Niels Bohr and the Development of Physics") "It would, in their view, be desirable to return to the reality concept of classical physics or, more generally expressed, to the ontology of materialism; that is, to the idea of an objective real world, whose smallest parts exist objectively in the same way as stones and trees, independently of whether or not we observe them." I think Bohm, like Everett, has reacted rather to the hybrid character of the Copenhagen view, which uses the classical theory as a "crutch" and which allows the continuity of the mathematical elements of the quantum theory to suffer damage as a result of this crutch. Heisenberg does have a cogent objection when

he points out that theories like Bohm's destroy the symmetry properties of quantum mechanics which are expressed in the principle of complementarity. On the other hand, Bohm's theory succeeds in maintaining continuity while at the same time describing "the transition from the possible to the actual" (Heisenberg's words) which Everett fails to do.

It is quite possible that Poincaré might have been attracted by Bohm's theory. As I have said, Poincaré regarded chance solely as a measure of our ignorance. According to him ("Science and Method") one is able to speak of the "laws" of chance only because small causes are able to produce large effects. If one is ignorant of the small causes one may then ascribe a random behavior to the large effects, provided the distribution of small causes is describable by an analytic function. In Bohm's theory this analyticity is insured by the underlying wave function. In accepting Bohm's theory, Poincaré would actually be able to loosen somewhat his rigidly deterministic pre-quantum views. For the small causes would now remain forever unknowable to any and all observers, despite their best endeavors, even if the whole universe were considered at once.

Let me sum up by again quoting Heisenberg (loc.cit.): [The] probability concept [of quantum mechanics] is closely related to the concept of possibility, the "potentia" of the natural philosophy of the ancients such as Aristotle; it is, to a certain extent, a transformation of the old "potentia" concept from a qualitative to a quantitative idea. On the other hand, the single quantum jump of Bohr, Kramers and Slater is "factual" in nature; it "happens" in the same manner as an event in everyday life..." That is to say, the real world does not branch. It is constantly in the process of passing from the possible to the actual — not many actuels, but one actual.

It is obvious that what we have here is a first class dilemma—or perhaps I should now say "trilemma", in view of what Everett has done. On the one hand we have the Copenhagen view with its disturbing invocation of discontinuities and a classical level. On the other hand we have theories like Bohm's which, although simultaneously self-contained and isomorphic with the real world, are repugnant because of the mathematical superfluousness of the "hidden variables." Although he never expressed himself in these terms I believe that Einstein felt keenly the existence of such an issue, and that this was basically the motivation for his famous controversy with Bohr. The merit of Everett's work is that the issue is now presented so clearly.

As far as your own espousal of Everett's views is concerned, while I sympathize with your remark that "terminology has to adjust itself in accordance with the kind of physics that goes on," if this means that I am not, for example, to be allowed to use such terms as "many actuels" and "one actual", (see above) or that it is meaningless to make a distinction between the two, I am afraid I can't go along with you. For me, Everett's world is not a faithful representation of the real world.

My other criticisms of Everett's work are relatively minor. I feel, for example, that some note should have been taken of Bohm's solution of the metaphysical problem, and of his general work on the

theory of measurement. Certainly no one working in this field has failed to read the closing chapters of Bohm's book.

* Also I should like to have seen a careful, abstract, mathematical demonstration that "good" observations can actually be made; that is, I should like to see a generalization applicable to arbitrary operators, of the von Neuman example, or of the old standby, the Stern-Gerlach experiment which everybody, including Bohm, uses. This would have allowed Everett to give a more careful definition of what a "good" observation is than he has done. I think that when Everett uses the term "good observation" he often means "perfect observation", and that when he says "all averages of functions over any memory sequence.... can be computed from the probabilities a_{α} , except for a set of memory sequences of measure zero," he really means "... except for a set of memory sequences having a measure which converges extremely rapidly to zero as the observations making up the memory sequence tend toward perfection." His attempt to obtain zero measure solely by taking the limit as the number of observations tends to infinity is not quite right I think. It is certainly incorrect if the observations are only "fair" to "poor".

Finally, it seems to me an unnecessary, although convenient, restriction to insist that the system state, if it is an eigenstate of the quantity being measured, shall remain unchanged after the measurement process is over. It is not necessary that an observation be repeatable to be significant, provided one can take the lack of repeatability quantitatively into account after a given period of time. The removal of this restriction would allow one to discuss the types of experimental observations and the types of memory sequences which are actually encountered in practice.

I hope that these remarks will serve as an indication to you that Everett's ideas are extremely stimulating. Let's have more of the same. Kindest regards.

Yours sincerely,



Bryce DeWitt

BDeW:cwj