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Author: [REDACTED]

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REPORT OF REFEREE

Before beginning detailed comments on this manuscript I must first set the historical record straight regarding Everett's work.

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In December 1956 my wife and I hosted GRG1, the first international conference on General Relativity and Gravitation, at the University of North Carolina. Shortly thereafter the Reviews of Modern Physics agreed to publish a set of papers submitted by conference participants dealing with the topics they had discussed at the conference. The papers were to be submitted to me, as acting editor for the set.

One of the papers that I received was Everett's "Relative State" paper. Although Everett had not been a conference participant and I had never met him, his paper was accompanied by (1) a strong letter from John Wheeler urging acceptance and (2) a paper by Wheeler assessing Everett's ideas. Since Wheeler had been a very active conference participant and since Everett's paper seemed to be relevant to the themes of the conference, I agreed to include it. This, of course, meant that I had to read it. In retrospect it seems to me likely that at that time, and for a number of years afterward, Wheeler and I were the only people, besides Everett himself, who knew what was in the paper. I read it very carefully and have a vivid memory of my reaction. First, I was tickled to death that someone at long last, after so many years and so many tiresome articles, had something *new* and refreshing to say about the interpretation of quantum mechanics. Second, I was deeply shocked.

I was so shocked that I sat down and wrote what turned out to be an eleven page letter to Everett, alternately praising and damning him. My damning largely consisted of quoting from Heisenberg regarding the "transition from the possible to the actual" and insisting upon the fact that "I do not feel myself split." The response I got from Everett was a Note to be added in proof to his article.

Before discussing this Note let me establish some conventions that will make referencing easy. Since all the papers to which I shall need to refer have been collected in "The Many-Worlds Interpretation of Quantum Mechanics," eds. DeWitt and Graham (Princeton, 1973), I shall use the pagination of that volume together with the letters MW. The coordinates of the Note in question are then (MW, bottom pp. 146, 147). In referring to [REDACTED]'s manuscript I shall use the letters MS. Thus (MS, p. 4, eq. (3)) refers to eq. (3) on page 4 of said manuscript.

The Note that Everett submitted was accepted by me and appeared in the published version. In it the word "splitting" appears. The word first appeared in my eleven page letter, but Everett accepted it without apparent qualms. He compared those who objected to his interpretation on the grounds that they don't feel themselves split, to the anti-Copernicans in the time of Galileo who did not feel the earth move. He also said "... *all* elements of a superposition (all 'branches') are 'actual,' none any more 'real' than the rest." He might equally well have said "... any *less* 'real' than the rest," and he *did* say this elsewhere (MW top p. 107).

His reference to the anti-Copernicans left me with nothing to say but "Touché!" His reply to my letter was succinct and to the point. I had no further ammunition to throw at him. There the matter might have rested had I not received a visit from Max Jammer a few years later. Jammer was preparing another volume on the foundations of quantum mechanics, attempting to bring the historical record up as far as the 1960s. I was surprised to discover that Jammer had never heard of Everett. As I was to learn later, Everett himself couldn't have cared less. As a graduate student he had puzzled over the foundations of quantum mechanics, had found an interpretation that satisfied him, and had then dropped the subject, going to work for the Department of Defense upon receipt of his degree (MW, bottom p. 141) and later forming his own consulting firm. I shall comment on some of his subsequent interests presently.

The meeting with Jammer started me thinking: This young man (Everett) is getting a raw deal; something should be done about it. It happened that I had a graduate student (Graham) at that time whom I had (reluctantly) permitted to

start a thesis on the foundations of quantum mechanics. I insisted that he have a close look at Everett. My initial shock at Everett's ideas had long since worn off, and whenever I thought about foundation questions (which was rarely) I found myself turning to Everett as providing the most sensible framework. As a result of discussions with Graham I resolved to start a publicity campaign. My initial act was to lecture on the Everett interpretation at the Battelle Rencontres in Seattle in 1967. Then I wrote the *Physics Today* article, which appeared in 1970. This article produced the results I desired; from then on Everett could not be ignored.

The *Physics Today* article was deliberately written in a sensational style. I introduced terminology ("splitting," multiple "worlds," etc.) that some people were unable to accept and to which a number of people objected because, if nothing else, it lacked precision. I tried to remedy some of the article's defects in a series of lectures I was asked to give, not long after, at one of the Varenna summer schools. The article based on these lectures (MW, p. 167ff.) tries to define carefully what a "good" measurement is and to deal with the question of imperfect measurements and with the case of observables having continuous spectra, measurements of which are always imperfect and for which the "splits" into multiple worlds are never clean. I regard this latter article as my most careful statement of my views on the Everett interpretation.

Precisely because I did not wish to appear as the sole spokesman for Everett I conceived the plan of collecting in one volume the total world commentary, up to that time, on Everett. I was also convinced that Everett's "Relative State" paper could not have constituted a complete statement of his views. Wanting to give him the chance to express himself fully I began a search for additional material. First I secured a copy of his thesis from Princeton, only to find, as I should have expected from the first footnote in the RMP article itself (MW, bottom p. 141), that the article and the thesis are identical. It should be stressed that up to this time I had never met Everett. Although he did not exactly shun publicity, he certainly did not seek it and was, in any case, bored by the interpretation controversies. With John Wheeler's help, however, I was able to get Everett to send me a thick, faded, dog-

eared manuscript entitled "The Theory of the Universal Wave Function." According to Everett this was his *Urwerk*, on which the Reviews of Modern Physics article was based. I therefore placed it first (MW, p. 3ff.) in the collection that Princeton University Press ultimately produced. Unfortunately Everett is now deceased, so an independent researcher such as [REDACTED] cannot ask him to verify that this *was* his Urwerk. But one need not rely on my statement. Internal evidence already indicates that it antedates the RMP article. For example, in it Everett writes "Einstein hopes..." (MW, p. 112, line 4), indicating that Einstein was still alive and that the work cannot have been written later than the spring of 1955.

On the question why this work, which is well written and brings Everett's views out more sharply than the RMP article, was never previously published, I can only speculate. I know that John Wheeler admires brevity and probably urged Everett to try to "sum up in a nutshell" the essential points of his new interpretation of quantum mechanics. It is also possible that Wheeler was reluctant to support a more blatant statement because it would mean setting himself into direct opposition to his hero, Niels Bohr. What is sure is that Wheeler long ago abandoned his support for Everett. What is equally sure is that if the Urwerk had been published Everett would not have been ignored for so long.

In 1972 I moved to the University of Texas and a few years later was enormously pleased to have Wheeler join our faculty from Princeton. One of the first things he did upon arrival was to persuade Everett to come to Austin and visit us for a few days, so that I was able to meet him at last. As he shook my hand he pulled out of his briefcase the original of my eleven-page letter and waved it under my nose. I assured him that I had forgotten neither the letter nor his reply. Everett was of small stature and a chain smoker. I was very saddened by his death not long after, as I would have loved to talk with him again many times. In none of our discussions in Austin was there any suggestion that our views on the interpretation of quantum mechanics were not in complete harmony, so I am a bit surprised to find that there are those, such as [REDACTED], who feel that I have not done justice to Everett. Of course, as I remarked earlier, Everett had long since ceased to think

deeply about the interpretation question; his attention then was on the problem of artificial intelligence. But this is not wholly unrelated to the interpretation question and to the question that seems to preoccupy [REDACTED]: Was Everett a "realist"?

In his Urwerk one may note that he uses the terms "observer" and "automaton" interchangeably, with complete indifference (MW, top p. 7; p. 64, line 8ff.). It was clear from our Austin conversations that, for Everett, the possibility of artificial intelligence and machines possessing consciousness was obvious. The only questions concerned details. For Everett there could be no distinction between "consciousness" and "the contents of a memory bank." They were identical. I remember us getting quickly into philosophical questions such as free will. He said "You give me an operational definition of an automaton that has free will, and I will design a computer program that will simulate your automaton to any degree of detail that you may desire. Therefore free will - *your* definition of free will, mind you - exists." For him, it seemed, a computer program was virtually synonymous with the print-out to which it ultimately led. They were interchangeable - equally "real." For him, whether we (i.e., the universe and all that is in it) have an independent existence or are merely solutions of some super differential equation is irrelevant. If there is an isomorphism between one and the other they are interchangeable. The words "one-one" and "isomorphism" already appear in Everett's Urwerk (MW, p. 109, line 3; bottom p. 133). Under an isomorphism between formalism and the "real" world, if something exists in the formalism then it "exists" in the "real" world. Does this make Everett a "realist"? In my opinion the views of both Everett and myself lie somewhere between realism and Platonic idealism. We both believe in the "reality" of the many worlds but we also believe that ultimately the abstract idea, theory, wave function, or ideal form behind it all is the true reality.

Having completed this historical account I am now ready to discuss [REDACTED]'s manuscript. I shall take up the pertinent items in the order in which they appear in the manuscript, even though this is not a particularly logical order:

MS, p. 2, line 15ff. "It is the purpose of this article to show that DeWitt's "many-

"worlds" interpretation is completely different from what Everett originally had in mind."

I must leave it to an independent referee, after reading both this report and the pertinent articles, to judge whether [REDACTED]'s article in fact does this.

MS, p. 2, line 19ff. "if DeWitt's scheme can be regarded as only one (and probably not the best) interpretation of Everett's theory, then objections to it may perhaps be answered by adopting a different interpretation."

Having written these words [REDACTED] is under obligation to provide an alternative interpretation. He has not done so.

MS, p. 4, line 17ff. "DeWitt's 'worlds' are conceived by him as concrete objects, each possessing a well-defined self-identity so that they can be counted in an unambiguous manner."

This is not so. Ambiguities are there and must be dealt with, as I attempted to do in my Varenna lectures in my discussions of imperfect measurements and of cases in which a clean split does not occur (MW, p. 210ff).

MS, p. 5, line 2ff. "he does not admit any explicit fundamental distinction between a measurement and other kinds of physical interactions. Therefore, he ascribes this 'splitting' faculty to an undefined, but seemingly a very wide class of processes."

The processes are by no means undefined. In the lines that [REDACTED] quotes immediately after the above I explicitly say "*measurementlike* interactions," i.e., interactions that fall under the very special class that can be called *measurements*, which have been previously carefully defined (MW, p. 156, eq. (4); pp. 171, 172, eqs. (1.5) to (1.8)). [REDACTED] is quite right, however, to criticize the paragraph as a whole, for I go on to say "every quantum transition taking place on every star, in every galaxy, in every remote corner of the universe is splitting our local world on earth into myriads of copies of itself." This is sloppy. Moreover, I allow the sloppiness to creep into my Varenna lectures as well (MW, p. 178, bottom three

lines). Only transitions that fall into the class of *measurements* have the property of causing clean splits. I had hoped that the reader would indulge my poetic license at this point, but clearly this was an error.

MS, p. 6, line 8. “‘measurementlike interaction’ (a term which he never clearly defines).”

“Measurementlike” means “like a measurement.” What can be clearer?

MS, p. 6, lines 10, 11. “The worlds...can be counted.”

They cannot be counted if the measurement is of an observable having a continuous spectrum, and in any case the splits are not then clean. I attempt to deal with this situation in my Varenna lectures. It is curious that [redacted] pays almost no attention to this later work, preferring always to jump on my *Physics Today* article, which is a popularization of Everett’s ideas and admittedly lacks precision. There is a language problem in describing the Everett interpretation. In the last analysis the formalism has to speak for itself.

MS, p. 8, line 9ff. “Everett chose to publish...”

The speculations of this whole paragraph are false. (See the historical outline at the beginning of this report.) [redacted] needn’t have speculated. The real story is not a secret. I have told it to many people. [redacted] could easily have approached me. He chose instead to go to an editor with a paper attacking me. I should be charitable. Perhaps he is young. But here I am speculating. I am also puzzled that Professors Paty and Lévy-Leblond couldn’t have given him better advice.

MS, p. 8, lines 19 to 24 and all of p. 9.

[redacted] is quite right to call the reader’s attention to Everett’s criticism of the “conventional formulation of quantum mechanics” and to Appendix II of Everett’s Urwerk. However, he goes a bit astray in his comments. Everett’s main concern is not with the applicability of the mathematical formalism, which was never at issue,

but with showing that the concept of a "universal wave function" is fully meaningful and that the formalism is capable of yielding its own interpretation. My main concern was not with the description of objective physical reality but (besides showing that the concept of a "universal wave function" is fully meaningful and that the formalism is capable of yielding its own interpretation) emphasizing, as Everett did not in his RMP article, that the "reality" implied by the formalism is vastly greater than most people realize. [REDACTED] is wrong in suggesting that this "larger reality" was irrelevant to Everett. Everett insists that one of the tests of an adequate theory is its "isomorphism" with the "world of experience." Although the "world of experience," in Everett's view, may be regarded either as consisting of sense perceptions of the individual or as the "real world," his insistence on "isomorphism" means that the "world of experience" does not consist merely of the sense perceptions of a *single* individual in the superposition comprising the universal wave function but of *all* the "individuals" represented in the superposition. Moreover, he emphasizes elsewhere (MW, p. 105, line 15ff.) that there is always the possibility for yet another observer to make use of interference effects between elements of the superposition, and hence these elements must be regarded as quite "real." To gain a true appreciation of how Everett views the "many worlds" (i.e., the elements of the superposition) an interested reader would do well to read his discussion of reversibility and irreversibility in his Urwerk (MW, pp. 98 and 99).

MS, p. 10, line 12, "'tangible' description of physical reality."

The word "tangible" appears nowhere in my writings. Moreover, I had no idea that a "description" could be "tangible." I suppose one could touch the paper on which it is written.

MS, p. 10, line 16ff.

It is good to point out, as [REDACTED] does, that Everett introduces the notion of "relative state" before he specializes to the type of interaction called "measurement." [REDACTED] is hardly justified, however, in concluding from the fact that I

begin with the special case of a measurement interaction that the difference between Everett and me is "fundamental" (MS, p. 11, line 18).

MS, p. 12, line 18ff. "Everett now proceeds to consider a specific example of a measurement set-up. Still, he assumes no fundamental distinction between the "apparatus" and the "measured system": the asymmetry between them is introduced only by the interaction (Von Neumann type)..."

Nevertheless the asymmetry *is* introduced.

MS, p. 13, line 4ff. "but to imagine the corresponding continuous case, although possible, is much more difficult; indeed, DeWitt does not consider such an example.

False. I consider the continuous case in my Varenna lectures (MW, p. 210ff.).

MS p. 14, lines 16 and 17. "This can also be appreciated from the moderate tone of the following words..."

If I am accused of being less moderate in tone than Everett, I confess it. *Mea culpa.* I suggest only that part of the difference between our styles might be ascribable to Everett's youth and the possible inhibiting influence of his thesis adviser. I was under no such constraints.

MS, p. 15, line 16 to p. 16, line 11. "It is important to note that the term "branching" refers to the observer state, not to the observer himself... From these quotations it is clear that although Everett sometimes uses the plural form in this context his intention is to indicate a plurality of superposed states, not a plurality of physical systems..."

It is not at all clear. The whole point of the footnote which [redacted] quotes, is to stress that sometimes it is convenient to speak of a plurality of systems (observers) and sometimes (depending on the point from which the formalism is being viewed) of only a single system (observer). Ordinary language is just not adequate to embrace the formalism together with all of its implications.

MS, p. 17, line 3ff. "while DeWitt's countable 'worlds' behave like apples, Everett's 'branches' or simply 'elements of superposition' continue to behave like vectors."

I am not sure what "behave like apples" means, but my "worlds" or "elements of the superposition" are vectors just as much as Everett's are. [redacted] implies that I am disloyal to the formalism. He should put up or shut up. In which of my equations do I abandon the formalism?

MS, p. 17, line 10, ff. "Finally, we should observe that nothing in Everett's articles even resembles the cosmic dimension of DeWiit's scheme, in which the whole universe 'splits'... The mouse does not affect the universe – only the mouse is affected."

The final statement is true only in first approximation. Once the mouse has observed the system it is correlated with it. This correlation is expressed by a superposition of mutually orthogonal vectors, each of which represents the system observable as assuming a certain value and the mouse as having observed that value. There is nothing to prevent one from bringing the rest of the universe into the superposition, its quantum numbers being simply the suppressed labels in the (complete) description of the mouse. Each element of the superposition then takes the form: (system in a certain state) \times (mouse having observed that state) \times (rest of the universe in a state that is the same for all elements of the superposition). Up to this point it is stretching things a bit to state, as I do in my articles, that the whole universe, including the system, the mouse and all the rest of it, has split, because the rest of the universe is the same in every element of the superposition. However, after the observation of the system by the mouse, the system and the mouse do not cease to interact with the rest of the universe. In each element of the superposition the system and the mouse affect the rest of the universe in different ways. The rest of the universe gradually becomes correlated with the system, and thus do microscopic quantum effects become magnified to macroscopic levels. Of course the effect of system and mouse on the rest of the universe cannot spread faster than the speed of light, and hence the principle of relativity points up an additional inadequacy

in common language. The universe *does* split, but the split only manifests itself gradually. To speak of the split as occurring instantly for the whole universe is simply to try to maintain consistency with the standard canonical formalism, in which there is assumed to exist a Hamiltonian and a universal time for the whole universe. This is all that I ever meant by the words "splitting universes," "many universes," or "many worlds," and any intelligent reader should have understood this. As for the "cosmic dimension" of my scheme I should point out that sometimes my "world" consists of "just two dynamical entities, a *system* and an *apparatus*" (MW, bottom p. 169).

MS, p. 18, line 19ff. "But from what has been said in Sec. 3 (especially in connection with what Everett calls 'a language difficulty') it should be clear that for Everett, the multiplicity of wave function branches does not represent any multiplicity of objects..."

It is not at all clear and is, in fact, false.

MS, p. 19, line 16. "When exactly within this period does the abrupt 'split' happen?"

In the Stern-Gerlach experiment the split "happens" gradually, during the time in which the atom is in the magnetic field. Neither I nor anyone else ever said the split was "abrupt." Nevertheless, the split is complete by the time the atom emerges from the magnetic field. The people who ask questions like this are those who appear to be mesmerized by the words I and others use to describe the formalism. There *is* a language problem, as everyone agrees. In the last analysis it is the mathematical formalism itself that counts. But this in no way implies that the "reality" described by the formalism is not vastly greater than that experienced subjectively by individual observers.

MS, p. 20, lines 6 and 7. "DeWitt's failure to define the term 'measurementlike interaction'..."

"Measurementlike" means "like a measurement," and I define measurements quite explicitly.

MS, p. 20, line 23ff. "But if we consider these 'apparatus states' to be just ordinary quantum states of ordinary quantum systems, it is not clear why they (and not other states) should give rise to the 'splitting' branch structure."

The people who make this objection (and there are many) tend to focus on the abstract mathematical structure of Hilbert space and to ignore the interaction between system and apparatus (observer). This interaction surely counts for something. Indeed it counts for everything. To refuse to accept that the interaction defines a "preferred basis" is to refuse to accept that an experimenter knows what he is doing. A good experimenter, in fact, knows quite well what he is trying to measure, and designs his apparatus (i.e., chooses the interaction) accordingly. You give me a measurementlike interaction and I'll tell you what observable is being measured and hence what the preferred basis is: namely, the eigenvectors of that observable. People like David Deutsch (and [REDACTED] should learn to how to spell Deutsch's name) want even more than that. Deutsch wants to associate a preferred basis – an "interpretation basis" – to *every* interaction, so that there will be no need to introduce a special class of interactions called "good measurements." I have no quarrel with this. But as a practical matter I know that experimenters are going to continue to be selective in their design work despite Deutsch's theorems.

MS, p. 21, lines 24 and 25. "Both suggestions make sense only in Everett's scheme..."

Nonsense. My use of the word "split" does not affect the formalism a bit, nor prevent me from using it. As I have said, it is the formalism that counts. Deutsch's work complements Everett's and my own quite nicely, and in my remarks above I do not mean to belittle him. Deutsch is one of the few people who have really gone beyond our work, particularly in the realm of quantum computers and in attempts to demonstrate the "reality" of the "other worlds." His papers deserve careful study. Incidentally I am quite happy with his attempts to abolish the "split" concept and

replace it with a continuum of "worlds." I have no hang-ups about terminology. Anything that will help people to understand what the formalism really implies is welcome.

MS, p. 22, line 19. "...as DeWitt admits..."

This makes me sound like a guilty schoolboy. I didn't *admit* it; I *pointed it out*, quite forcefully, in both the *Physics Today* article and in my Varenna lectures. It is a problem that I still worry about.

MS, p. 23, lines 20 and 21. "DeWitt's 'many-worlds interpretation' is strongly motivated by a general 'realistic' approach..."

My motivation was to make sure that Everett received proper recognition. I do not believe that I misinterpreted him nor did he ever indicate to me that he thought that I had.

It should be clear from the above comments that I do not regard this paper as fit for publication, but obviously an independent referee will have to decide. I do not believe that [REDACTED] has a case, but if he does it will need to be much more carefully constructed. The chief points of contention seem to center on my choice of words and verbal imagery. It might be helpful to have some version of my comments above brought to the attention of a wider audience, but a public scrap with [REDACTED] is not a vehicle I shall accept. The question whether I or Everett, or both, should be called "realists" is purely a philosophical question and should be addressed in a journal of philosophy, not a physics journal. As I am not a trained philosopher I would have nothing to contribute to this.

Bryce DeWitt