# Everett's "Many-Worlds" Proposal[☆]

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**Abstract**

Hugh Everett III proposed that a quantum measurement can be treated as an interaction that correlates microscopic and macroscopic systems—particularly when the experimenter herself is included among those macroscopic systems. It has been difficult, however, to determine precisely what this proposal amounts to. Almost without exception, commentators have held that there are ambiguities in Everett's theory of measurement that result from significant—even embarrassing—omissions. In the present paper, we resist the conclusion that Everett's proposal is incomplete, and we develop a close reading that accounts for apparent oversights. We begin by taking a look at how Everett set up his project—his method and his criterion of success. Illuminating parallels are found between Everett's method and then-contemporary thought regarding *inter-theoretic reduction*. Also, from unpublished papers and correspondence, we are able to piece together how Everett judged the success of his theory of measurement, which completes our account of his intended contribution to the resolution of the quantum measurement problem.

*Keywords:* quantum measurement problem, Hugh Everett III, many-worlds interpretation, preferred basis problem

Hugh Everett III presented his reformulation of quantum mechanics, often called the *many-worlds interpretation*, as a solution to the quantum measure-

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ment problem. As the measurement problem is now understood, a solution would involve providing an account of measurement processes in quantum mechanical terms; i.e., a quantum *theory of measurement*. Everett's proposal has gained notable support in the physics community; however, there is broad disagreement as to precisely what Everett's theory of measurement is—or what it ought to be. These are mainly reasonable disagreements; in fact, it is fair to say that Everett's proposal is ambiguous. As Jeffrey Barrett observes in his thorough examination of Everettian positions, "The fact that most no-collapse theories have at one time or another been attributed to Everett shows how much the no-collapse tradition owes to him, but it also shows how hard it is to say what he actually had in mind" (1999, pp. 90-1).

In the present paper, we do not attempt to *disambiguate* the many-worlds interpretation; rather, we show that Everett found certain details to be superfluous to the foundations of quantum theory. As a first analysis, we apply the framework of *inter-theoretic reduction* to Everett's overt method. We claim that (to a good approximation) he thought of his project as a *direct partial inhomogeneous reduction* of the standard (von Neumann) formulation of quantum mechanics.[1] Further, we claim that this reduction does not proceed by a direct appeal to *splitting worlds*, and that misconceptions on this point have been the primary source of controversy regarding Everett's proposed solution to the measurement problem. Recently uncovered papers and correspondence support these claims, and provide new insight on how Everett judged the success of his theory of measurement. We find that he repeatedly stresses the alleged empirical adequacy of his formulation and its potential application in cosmology, while eschewing any other question of interpretation. In some places, Everett argues broadly against realism in science, and he proclaims that "any physical theory is essentially only a model for the world of experience" (1973, p. 134). These generically antirealist theses complement our claim about the nature of his project, and we argue that together they give the best available account of his intended contribution to the resolution of the quantum measurement problem.

## 1. Interpreting Everett

Most controversies regarding the best way to understand Everett's formulation are of two (closely related) sorts: (1) disagreements over how to

[1] See Nagel 1961, Schaffner 1967, and Sklar 1967.

understand talk of "splitting" and "branches"; and (2) disagreements over how to resolve the so-called *preferred basis problem.*[2] These two issues have received much attention, and addressing them is often seen as a prerequisite to treating further puzzles; such as the meaning of quantum probabilities in many-worlds.

Rampant "splitting" is the best-known feature of many-worlds interpretations. But there are diverse opinions about *what* is being split, and about whether this splitting should be understood literally. The splitting that occurs could be of individual objects and systems (DeWitt 1970), of conscious observers (Albert and Loewer 1988; Lockwood 1989), of all of space and its occupants (Wheeler 1970, 1977; Davies 1980), or of entire histories (Gell-Mann and Hartle 1990). Alternately, one could read talk of splitting as a metaphor meant to illustrate Everett's notion of the *fundamental relativity of states*; in which case, the only things that multiply are perhaps "perspectives" or "relative facts" (Putnam 1981; Saunders 1993). In several influential Everettian positions, splitting is neither fundamental nor metaphorical; rather, branches are "emergent structure" that results from contingent (yet ubiquitous) processes, such as environmental decoherence (Zeh 1970, 1973; Zurek 1991, 2003; Deutsch 1997; Wallace 2002, 2003). There are also versions in which splitting does not feature at all, such as in Geroch's reading (1984), Bell's "Everett (?)" theory (2004), Healey's modal realist interpretation (1984), Albert's "bare theory" (1994), and Barrett's "many-threads" theory (1999). Of course, this list is not exhaustive and there is the possibility of various hybrid positions, such as interpretations in which some entities split and others evolve stochastically (e.g., Albert and Loewer's "single-mind" and "many-minds" formulations.) These differences in the metaphysics of splitting account for much of the variety of Everettian interpretations.

The second sort of controversy regards, in essence, the *conditions* of the professed splitting; which determine *when* splitting occurs and the *state* of each resulting branch. The prevailing opinion among both proponents and critics is that Everett's presentation of his theory of measurement presupposes a preferred basis (or preferred set of observables) for each split, with respect to which new branches are defined.[3] Such a basis would resolve many

[2]See Barrett 1999, chap. 4-6, for an accessible discussion of the preferred basis problem.

[3]See, for example, Zeh 1970, DeWitt 1973, Stein 1984, Deutsch 1985, Kent 1990, Albert 1994, Lockwood 1996, Saunders 1998, Barrett 1999, Wallace 2003, Zurek 2003, and Bell 2004.

questions of when splitting occurs and with what result, but there is disagreement over how to properly select a *measurement* basis. In fact, the preferred basis problem is closely related to the problem of specifying when and how the wave function *collapses* in the standard formulation, which is precisely the difficulty that Everett is trying to avoid.[4] The choice of a measurement basis must be made carefully, since an inelegant or *ad hoc* solution threatens to negate the advantages of the many-worlds interpretation over collapse and hidden-variable formulations. The several ontologies mentioned above give a sense of the various (inconsistent) ways that one might frame this problem and attempt to solve it.

Again, the surprising fact is that Everett's presentation allows for such diverse interpretations of seemingly elemental details—the what, when, and how of the theory's most distinctive feature.[5] He simply does not make any straightforward statements about the nature of splitting that can settle these controversies. A common attitude among many-worlds proponents is that Everett's proposal represents a program for further research rather than an independent theory. This is a theme particularly among those offering a *decoherence-based* approach, who see themselves as answering remaining questions with resources already available to an unmodified quantum theory (See Wallace 2003, p. 88). But, on even a first reading, it is clear that Everett did not think of his project as setting up a program of research that would eventually fill in details about which systems undergo splitting, when or how quickly they split, and what sorts of branches are produced. Everett does not acknowledge any such omissions in his formulation, nor does he identify any outstanding problems. Rather, he confidently concludes that "the continuous evolution of the state function of a composite system with time gives a complete mathematical model for processes that involve an idealized observer" (1957b, p. 462). Accordingly, we assume that he intended to sketch

[4] Consider Bell's well-known criticism (2004, pp. 133-4). The relationship between the two problems becomes more complicated if one allows for the possibility of interference between branches, or if branch states are not required to be strictly orthogonal. These are features of certain decoherence-based approaches.

[5] In fairness, it should be noted that Everett did not intend to continue his work on the foundations of quantum mechanics after receiving his degree from Princeton. In fact, Everett left theoretical physics and took a job at the Pentagon in June of 1956, before officially submitting his dissertation thesis. The excellent work of Osnaghi, Freitas, and Freire (2009) gives an authoritative account of the events surrounding Everett's time at Princeton.

a *complete* theory of measurement, rather than just a promising avenue of research. At the same time, it is implausible that Everett was oblivious to the problem of selecting a measurement basis. In order to make good on his claim that "the theory itself sets the framework for its interpretation" (p. 455), something in his formulation must eliminate the need to *stipulate* a basis. Consequently, we will have to account for the apparent omissions in Everett's formulation. Why doesn't Everett give a characterization of the sort of system that undergoes splitting? Why doesn't Everett give a general account of when splitting occurs and what the state of each branch will be? How can one evaluate the empirical adequacy of the theory without these details? These are ultimately questions about methodology, so we will look for insights in the way that Everett sets up and executes his project.

## 2. Everett's project

The *semantic ascent* at key places in his thesis, unpublished papers, and correspondence is an important and overlooked aspect of Everett's writing. Quine coined the term 'semantic ascent' to name a familiar "maneuver" in which one shifts to talking about words, or other linguistic objects, especially while attempting to sidestep the supposed implications of *using* such words. This has the desirable effect of carrying "the discussion into a domain where both parties are better agreed on the objects (viz., words) and on the main terms concerning them" (1992, p. 169). When employing semantic ascent in scientific discourse, one draws narrow conclusions about axiomatic theories, mathematical models, logical entailment, etc., about which disagreement is less likely or, at least, more concrete. As we'll see, when Everett lays out his project or defends it to critics, he quickly resorts to semantic ascent. More significantly, *Everett seems to hold that this level of discourse is all that is required to carry out his project.* That is, Everett's proposal is itself an instance of semantic assent—and self-consciously so.

### *2.1. Puzzling semantic ascent*

In both his doctoral thesis[6] and the longer version published in the Graham and De Witt volume, hereafter referred to as the *short thesis* and *long thesis* respectively, semantic ascent is apparent in those passages where he

[6]The submitted thesis (1957a) and the article (1957b) published that same year are essentially identical.

states his project. In a passage from the introduction to the short thesis, Everett describes his formulation as an axiomatic theory that is partially interpreted in the "world of experience," and which differs from the standard theory *only* in that it lacks the "special postulates of the old theory which deal with observation" (1957b, p. 454). It is claimed that versions of the standard measurement postulates can be "deduced" within his formulation. Moreover, he characterizes his contribution to quantum theory as a logical one—showing the proper "logical position" of the measurement postulate, and, consequently, showing that his formulation serves as a "metatheory" to the standard formulation. The introduction to the long thesis reads much differently, beginning with a *Wigner's friend* type thought experiment and moving into a discussion of various strategies for resolving the measurement problem. However, when previewing his own approach, he uses similar language (1973, p. 9).

One would expect it to be the case that Everett interprets his results later on, and explains what it means for an observation to be a closed quantum process. Yet, conspicuous instances of semantic ascent persist where a direct statement of seemingly important details is expected. A prime example is a footnote in the section on observation found in the long thesis. In this section, Everett lays out his theory of measurement, which treats measurement as a quantum interaction between an *object* system and an *observer* system, leaving the joint *object-observer* system in an entangled superposition. The footnote reads:

> At this point we encounter a language difficulty. Whereas before the observation we had a single observer state afterwards there were a number of different states for the observer, all occurring in a superposition. Each of these separate states is a state for an observer, so that we can speak of the different observers described by the different states. On the other hand, the same physical system is involved, and from this viewpoint it is the same observer, which is in different states for different elements of the superposition (i.e., has had different experiences in the separate elements of the superposition). In this situation we shall use the singular when we wish to emphasize that a single physical system is involved, and the plural when we wish to emphasize the different experiences for the separate elements of the superposition. (e.g., "The observer performs an observation of the quantity $A$,

> after which each of the observers of the resulting superposition has perceived an eigenvalue.") (1973, p. 68, n. 1)

What is most odd about this comment is that the difficulty alluded to does not appear to be about *language* at all. The problem does not arise from any imprecision or ambiguity in language. It is perfectly clear that there is *one* wave function and that this wave function can be decomposed into a superposition of *many* components. The question is how this mathematical structure is to be interpreted, which is precisely the issue of how we are to understand splitting in the many-worlds formulation. If there is any problem with language, it is with the inscrutable distinction that Everett seems to be making between the number of physical systems and the number of distinct inhering states. He evidently wants to treat each component of a superposition as significant in its own right, but he appears ambivalent about how to characterize that individual significance. As a result, this passage becomes so convoluted that it can be read as consistent with a variety of positions, or ignored altogether. For our purposes, it is important to note that Everett attempts to resolve this issue by adopting a language convention. Rather than treating this as an interesting interpretive issue, Everett summarily decides to adapt his manner of speaking to the mathematical formalism. He stipulates that he will use the singular 'observer' when working with the wave function as a whole, and he will use the plural 'observers' when working with a particular decomposition of the wave function. Everett finds this maneuver sufficient for his purposes.

There are other important passages in Everett's published work where he shifts attention to language—specifically, to the way that certain assertions are meant to correspond to von Neumann's mathematical formalism. Not least among these is the section where Everett claims to deduce the probabilistic assertions of the standard theory. But Everett employs similar maneuvers when attempting to explain and defend his work to colleagues. Tellingly, he attaches the following in a footnote to his submitted dissertation:

> An earlier less condensed draft of the present work, dated January 1956, was circulated to several physicists. Their comments were helpful in the most difficult task of finding the right words to attach to the individual constructs of the present rather straightforward mathematical machinery. It would be too much to hope

> that the revised wording avoids every misunderstanding or ambiguity. (1957a, p. 1, n. ∗)

Everett's proposal met with resistance from the start, and we know that serious questions regarding interpretation were posed. Everett's response to this criticism was to fall back to his mathematical formalism and insist that its acceptance is a matter of finding the right words to use in conjunction. In correspondence with those who reviewed his paper, Everett qualifies his more metaphysically suggestive claims. For example, in a letter to Bryce DeWitt, he writes:

> When one is using a theory, one naturally pretends that the constructs of the theory are "real" or "exist." If the theory is highly successful (i.e., correctly predicts the sense perceptions of the user of the theory) then the confidence in the theory is built up and its constructs tend to be identified "elements of real physical world." This is, however, a purely psychological matter. No mental constructs[...] should ever be regarded as more "real" than any others. (May 31, 1957)

Everett clearly wants to avoid any firm metaphysical commitments. If he is not declaring that his talk of splitting observers is merely figurative, then he is at least indicating that he does not want his proposal to be judged on metaphysical considerations. Moreover, Everett shows his preference for framing his proposal in terms of language and its connection to mathematical formalism. Indeed, he prefaces his reply to DeWitt by saying that, for him, "any physical theory is a logical construct (model), consisting of symbols and rules for their manipulation, *some* of whose elements are associated with elements of the perceived world" (ibid.).

In a letter written to Norbert Wiener the same day, Everett qualifies his claim that *all branches are actual* in a slightly different way. Wiener had indicated to Everett that he has been concerned with the issue of how to represent "that a certain fact or a certain group of facts is actually realized," and he asserts that Everett has not adequately addressed this problem (letter to Wheeler and Everett, April 9, 1957). For Wiener, this is the problem of describing the *reduction of the wave packet* (See Wiener and Siegel 1953); which, as we have noted, is formally equivalent to certain statements of the preferred basis problem. In reply, Everett writes:

> You also raise the question of what it means to say that a fact or a group of facts is actually realized. Now I realize that this question poses a serious difficulty for the conventional formulation of quantum mechanics, and was the main motives [sic] for my reformulation. The difficulty is removed in the new formulation, however, since it is quite unnecessary in this theory ever to say anything like "Case A is actually realized." (May 31, 1957)

In the many-worlds interpretation, the splitting of the wave function is supposed to account for the "actual realization" of measurement outcomes. Yet, Everett's reply suggests that the fact that a certain measurement outcome (e.g., "Case A") has *occurred on some branch* does not imply that this outcome is *actually realized*. It seems that Everett has not merely qualified his claim that branches are real, but contradicted it. Fortunately, Everett's hand-written notes in the margin of his copy of Wiener's letter show how he means to reconcile his comment:

> In Theory the universal state function is the realized fact. [...] no such statements ever made in theory [like] "case A actually realized", except relative to some other state! All possibilities "actually realized", with corresp[onding] observer states.[7]

Everett apparently distinguishes between the statements 'case A is actually realized' and 'case A is actually realized relative to observer state B'; and statements like the latter are supposed to account for the realization of measurement outcomes. But Everett's semantic assent makes it difficult to draw any conclusions about the nature of splitting. We are not told how the first kind of statement fails. Everett's comments may indicate that there is a modal distinction between being realized *simpliciter* and being realized as a *relative state*, or he could mean that no measurement outcome is realized *to the exclusion of any other*. In the first case, 'case A is actually realized' is literally false; while, in the second case, it merely creates the wrong implication. And, in any case, we are no closer to understanding what is meant by 'all possibilities are actually realized.' Nor does Everett offer any further explanation—to Wiener or anyone else. He seems content to point out that certain problematic assertions do not feature in his formulation.

[7] Everett's handwritten notes found in the left margin of page 3. An electronic copy of the original, with *marginalia*, is available in the correspondence file of the forthcoming UCISpace Everett archive.

### 2.2. *The logico-linguistic nature of Everett's project*

Though Everett has several opportunities to address interpretive issues regarding his theory of measurement, ambiguities persist due to a curious and recurring shift in focus to language. Thus, we find that Everett stays very close to his stated project: He considers von Neumann's Hilbert space formalism without the measurement postulates and he develops language (i.e., notation) for working in this formalism. He then describes a mathematical model for measurement processes and a method for deriving statements of the standard quantum predictions from that model.

Our goal has been to build the case that Everett actually conceived of his "many-worlds" proposal in logico-linguistic terms; that is, in terms of the relationship between language (broadly construed) and quantum formalism. To make this claim more explicit, we will use the framework of inter-theoretic reduction. I am not endorsing inter-theoretic reduction as the best way to understand explanatory relationships between scientific theories; however, we will see that there are several instructive points of comparison between Everett's project and that of reduction. To be clear, the extent to which Everett was aware of the philosophy of science being done at the time is not certain; but there is evidence that he had some exposure.[8]

The claim, then, is that Everett conceived of his project as something like an inter-theoretic reduction of von Neumann's formulation. For our purposes, we can identify it as a *direct partial inhomogeneous* reduction. The term 'direct' is meant to convey that it is a reduction in Nagel's sense, rather than one that requires an intermediate (corrected) theory. On Everett's account, the formal conditions for direct reduction are certainly satisfied. The statements of each theory are formalizable, and there is considerable overlap among their expressions. Nagel's requirement that "[e]very statement of a science $S$ can be analyzed as a linguistic structure, compounded out

[8] When asked, several years later, if he had studied philosophy before 1957, Everett replied that he had only taken one epistemology course as an undergraduate student (letter to Max Jammer, September 19, 1973). Yet, other correspondence suggests that Everett had also read the work of one of the founding members of the Vienna Circle, Phillip Frank. Everett sent a copy of his short thesis to Frank, with a letter saying, "I have received several of your works on the philosophy of science [...] I find that you have expressed a viewpoint which is very nearly identical with the one which I have developed independently in the last few years, concerning the nature of physical theory" (letter to Phillip G. Frank, May 31, 1957). We will return to Everett's view on the nature physical theory and its relationship to contemporary philosophy of science.

of more elementary expressions in accordance with tacit or explicit rules of construction" (1961, p. 349), could easily be substituted for Everett's characterization of theories as logical constructs. Moreover, all of the predictions of the reduced theory are purportedly recovered, and much of its structure is preserved in the reducing theory. In this sense, we are meant to think of the standard formulation as being embedded in *many-worlds theory* (hereafter MW.) The thesis that Everett attempts a direct reduction accounts for his claim that he retains "all of the content of the standard formulation," and that his formulation serves as a "metatheory" that shows the validity of the usual methods (1957b, p. 462).

On the other hand, Everett's reduction is partial because there are elements of the standard formulation that are not associated with elements of MW. The extent to which the standard formulation is incongruous with MW is bound to be contentious, but Everett is at least clear that *probabilities* are not found in his model. He promises at the outset to recover the standard quantum statistics without "*making any reference to probability models*" (1973, p. 11). Yet, Everett states that "the statistical assertions of the usual interpretation[...] are deducible[...] from the pure wave mechanics that starts completely free of statistical postulates" (1957b, p. 462). Therefore, the probabilities of the standard formulation are *emergent*, in Nagel's terminology; that is, probabilistic "properties" are "construed as a thesis concerning the *logical relation* between certain statements" (Nagel 1961, p. 372). Indeed, given that the reduction is direct, any property or entity that does not have a counterpart in MW is emergent in this sense.

Lastly, the relationship between von Neumann's formulation and MW is inhomogeneous, because several of the associated elements are conceptually distinct. Most notably, the notions of *relative state* and *correlation information* are genuinely novel.[9] Everett means the "fundamental relativity of states" to indicate a departure from the sort of state found in the "external observation" formulation. Moreover, Everett has an unusual understanding of correlations, which are defined as an aspect of the universal wave function *qua* "basic physical entity" (1957b, p. 455), rather than an interdependence among observed values. Even the wave function, which is taken directly from quantum theory, gains a new character when applied to macroscopic systems, much less the whole universe. As with other many-worlds interpretations,

[9]See section 2.3.

no part of our reading of Everett's proposal is likely to be confused with the standard formulation.

With these qualifications, Everett's overt project is to associate parts of the usual Hilbert space formalism with parts of MW, giving a method for replacing certain statements (including the standard predictions) with expressions from MW. In particular, Everett means to treat the whole universe (or any isolated fragment) as a closed quantum system, and to identify the standard collapse dynamics (which applies to open systems) with aspects of the interactions between subsystems. His stated aim is to show that MW subsumes traditional quantum theory and accounts for its success. We may be encouraged that these are the putative aim and method of inter-theoretic reduction, but our analysis must be carried further to be of any value. If our claim were simply that Everett offers statements about *splitting* to replace statements about *measurement* and statements about *branches* to replace statements about *measurement outcomes*, then we will have added absolutely nothing to our understanding of the many-worlds interpretation. That sort of move would merely reproduce the same interpretive difficulties at a meta-linguistic level, regardless of what Everett thought about the semantics of scientific theories.

Part of our analysis must be to show that splitting plays (at most) a secondary role in Everett's reduction. As has already been discussed, disagreements over how to understand Everett's proposal stem from the fact that a splitting process does not explicitly appear in his formalism. The machinery of MW does not rely on splitting of any kind. What must be further shown is that an intuitive notion of splitting is not essential to Everett's theory of measurement, by which he motivates and effects his reduction of the standard theory. Otherwise, we are unable to explain his silence on significant issues, such as the preferred basis problem. In the following section, we argue that *correlation* is the fundamental notion in Everett's account of the measurement process, and, consequently, that the main point of contact between the standard theory and MW is in their respective treatments of correlation. This will require us to engage with some of the details of MW. The discussion will presuppose some familiarity with the exposition from the short thesis, and with the Hilbert space formalism generally.

### *2.3. Correlation and Everett's model of measurement*

Much of the long thesis is spent developing an information-theoretic notion of correlation in the context of quantum theory. The resulting notion of *correlation information* had been a focus of Everett's doctoral research from early on. In a preliminary paper written for John Wheeler, Everett points out the conceptual advantages of quantifying correlation in terms of *Shannon information*, and he proposes an expression for the information contained in a quantum correlation that uses the squared amplitude of the wave function in place of a probability distribution.[10] Dozens of pages from Everett's Princeton notebooks show that he experimented with at least two different versions before settling on the bracket notation and manipulation rules that appear in the long thesis. Other evidence shows that correlation was, in fact, of primary importance to his project. In an early outline of the long thesis, Everett gives correlation pride of place.

> The New theory  Basic Postulates—Pure Wave mechanics, no stat[istical] interpret[ation]. Fundamental quantity the wave function itself.
>
> Interpretation through Correlations—existence of classical objects as correlations, etc. The ideal observer—
>
> The measuring process, effect on total wave function
>
> interpretation as splitting of observers.
>
> [. . . ]
>
> IV Measurement and observation
> In accordance with our plan to develop quantum mechanics along the grounds of pure wave mechanics, (and regarding correlations all holding between wave square amplitudes, not probabilities); we must investigate the result of regarding the process of measurement as a natural process, and treating it, in its entirety, wave mechanically. Measurement regarded only as introduction of correlation between system [and] observer ([or] apparatus) (same when obs[erver] looks at apparatus.)

[10] The Shannon information of a discrete probability distribution, $p(x)$, over a collection of elementary and mutually exclusive events, $\{x_i\}$, is given by $\mathrm{I}_X = \sum p(x_i) \log p(x_i)$. Everett's proposed measure of correlation between the observables $X$ and $Y$ is $C(X,Y) = \int\int \bar{\Psi}\Psi \log\left[\frac{\bar{\Psi}\Psi}{\int \bar{\Psi}\Psi\, dx \int \bar{\Psi}\Psi\, dy}\right] dx\, dy$, "with wave function in $x, y$ representation $\Psi(x,y)$" ("Quantitative Measure of Correlation," p. 3). This expression can also be written as $C(X,Y) = \mathrm{I}_{XY} - \mathrm{I}_X - \mathrm{I}_Y$; where $\mathrm{I}_{XY} = \int\int \bar{\Psi}\Psi \log \bar{\Psi}\Psi\, dx\, dy$, $\mathrm{I}_X = \int(\int \bar{\Psi}\Psi\, dy) \log(\int \bar{\Psi}\Psi\, dy)\, dx$, and $\mathrm{I}_Y = \int(\int \bar{\Psi}\Psi\, dx) \log(\int \bar{\Psi}\Psi\, dx)\, dy$.

("Outline of the new theory," p. 1, 5)

The organization of the short thesis preserves much of this outline: the rejection of the special measurement postulates, the proposal to treat measurement processes as interactions governed by unitary dynamics, and the suggestion that the resulting model describes a splitting of observer states. However, the extended discussion of the role of correlation in MW is left out of the short thesis, and the notion of correlation information does not appear at all. In contrast, these notes show that Everett had intended to get a lot of mileage out of the notion of correlation in his full dissertation. He articulates the bulk of his project, including the application of MW to measurement processes, without mentioning any kind of splitting. Rather, Everett suggests that correlation is the defining characteristic of measurement. Perhaps more importantly, he claims that *classical objects*—those objects that populate "the world of experience"—emerge in MW *as correlations.* As Everett conceived of his project, the notion of a splitting observer only comes into play at the very last step, where he attempts to recover a non-literal sense in which the standard collapse dynamics is satisfied in MW. These notes demonstrate that correlations, "holding between wave square amplitudes," are central to the way that Everett conceived of his project.

Though the notion of correlation information is not developed in the short thesis, correlation does play an implicit role. The *relative state* notation, which features in both the short and long theses, is best understood as a tool for describing quantum correlations. First, the notation only applies in contexts where a closed quantum system is decomposed into two subsystems; i.e., when the relevant Hilbert space is expressed as a tensor product of two Hilbert spaces. Moreover, the only interesting applications of the relative state notation are to *entangled superpositions*—circumstances in which the (pure) state of the composite system cannot be expressed as a tensor product of pure states belonging to each subsystem. If the subsystems are not entangled (i.e., independent), then the "relative state" of each subsystem is merely its quantum state, as described by the standard *eigenvalue-eigenstate link.* Lastly, quantum entanglements are also the very circumstances in which the standard eigenvalue-eigenstate link fails to describe the state of each subsystem individually. The relative state notation supplements the standard theory in precisely those situations marked by the presence of quantum correlation, in which the eigenvalue-eigenstate link is deficient. Hence, Everett describes the fundamental relativity of states as the fact that "subsystem states are generally *correlated* with one another" (1957b, p. 456). As far as we are told, the absence of an absolute state, which occasions the use of the relative state notation, is manifest as the presence of a correlation.

Everett's treatment of the correlations between subsystems becomes the foundation for his theory of measurement. He states so explicitly in the long thesis:

> From our point of view there is no fundamental distinction between "measuring apparata" and other physical systems. For us, therefore, a measurement is simply a special case of interaction between physical systems—an interaction which has the property of *correlating* a quantity in one subsystem with a quantity in another. (1973, p. 53)

The reference to correlation is less prominent in the short thesis, but still significant:

> Let us consider the simple case of a single observation of a quantity $A$[...] The final result is, as we have seen, the superposition
>
> $$\psi'^{S+O} = \sum_i a_i \phi_i \psi^O[\ldots \alpha_i] \,.$$
>
> There is no longer any independent system state or observer state, although the two have become correlated in a one-one manner. [...] This correlation is what allows one to maintain the interpretation that a measurement has been performed. (1957b, p. 459)

Though Everett gives two descriptions of an ideal measurement—one in terms of relative states and the other in terms of correlation information—they are closely interrelated and formally equivalent. On both accounts, a measurement is an interaction between subsystems that produces a strong correlation. In an *observation*, the interaction produces a *measurement record* in the form of a pointer reading, a notebook tally, or the sensations, perceptions, memories, etc., of experimenters. Everett asserts that he maintains a "psycho-physical parallelism" in his treatment of observation, meaning that all measurement records are regarded in the same way—as a physical degree of freedom (1973, pp. 8-9). Hence, observation too results in a quantum correlation that can presumably be described in either the relative state or correlation information notations.

At least this much should be uncontroversial as a partial reconstruction of Everett's proposal. To summarize: MW is meant to consist of the von Neumann formalism, stripped of the postulates pertaining to measurement. The resulting "pure wave mechanics" is then supplemented with two sets of notation; one centered around the notion of relative state and the other around correlation information.[11] Each notation is well-suited for describing the entangled state that results from an interaction of subsystems, and they form the foundation for Everett's general model of measurement. The model of measurement can be formulated independently (and equivalently) in each notation.

It is an intuitive notion of correlation that guides the development and application of MW. This is a subtlety of Everett's presentation that is often lost in discussion.[12] Everett's proposal is not to simply replace talk of measurement with talk of splitting observers. Rather, he provides new tools for describing the measurement process, which becomes the primary point of contact between MW and the standard collapse formulation. The upshot is that our analysis does not immediately reproduce the same ambiguities that it is meant to address; though, it remains to consider what can be learned on this account about Everett's proposed solution to the measurement problem.

## 3. Everett's criterion of success

### *3.1. An example*

In an early paper, titled "Probability in wave mechanics," Everett gives a condensed presentation of his scheme for reducing quantum theory to pure wave mechanics. This paper is particularly important to the present task of investigating Everett's proposal, because it has several features that his more polished work does not (and *vice versa*.) A central part of this presentation

[11] One reason for speaking of new notations, instead of additional postulates, is to remain as neutral as possible regarding the interpretation of MW. Another reason is that Everett claims to arrive at his results from pure wave mechanics alone.

[12] There are interpretations of quantum mechanics that capitalize on the notion of correlation, but they are articulated as metaphysical theses that are hard to square with talk of splitting, multiple observers, or even relative states. Consider, for example, the *correlation without correlata* position discussed by Barrett (1999) or the *relational quantum mechanics* of Rovelli (1996), Mermin (1998), and others. In contrast to those positions, my claim is that what I am calling Everett's *reduction* of quantum theory is largely mediated by an intuitive—but rigorously formalized—notion of correlation.

is a concrete example of how a composite system is treated in MW. Everett relates this example to a topic that is skirted elsewhere: *how macroscopic degrees of freedom are understood in MW.* As a result, we get a better sense for how the standard methods, which have depended on classical descriptions of macroscopic apparata, are supposed to be embedded in MW. The splitting and branch metaphors also appear in this paper, but they are joined by two additional metaphors: *smearing out* and *cross-section.* Conspicuously absent, however, is a fully developed notion of relative state or the fundamental relativity of states. Because this paper gives a comprehensive, big picture view of Everett's project, it should add to our understanding of how he judged the success of his proposal and provide a good test for our claims thus far.

The first several paragraphs of "Probability in wave mechanics" closely fit the lead-in of Everett's published work. He declares his intention to work within pure wave mechanics, and he promises to show that the remaining aspects of the standard formulation emerge from the measurement process itself. Everett then sets up the problem of describing the entangled state of an apparatus and object system after their interaction. But he has, evidently, not yet invented the relative state terminology, because he gives the following figurative account of the difficulty:

> If we look at a "cross section" of the total wave function for which the variable $x$ has the definite value $x_i$, we find that the apparatus has the definite value $y_i$ which corresponds, while if we choose to consider the "cross section" for $x_j$ definite, we immediately find that $y$ has the definite value $y_j$, etc.
>
> So we see that from the viewpoint of wave mechanics that when a measuring apparatus interacts with a system which is not in an eigenstate of the variable being measured that the apparatus itself "smears out" and is indefinite, no matter how large or "classical" it is. Nevertheless, it is correlated with the system in the above sense, and it is this correlation which allows us to give an adequate interpretation of the theory. ("Probability in wave mechanics," p. 4)

The term 'cross-section' is suggestive of the mathematical procedure of projecting the wave function onto a given subspace, which is how relative states are constructed. What's more, Everett's conclusion does not follow unless the wave function is expanded in terms of such projections. So we may infer that cross-sections and relative states correspond to the same mathematical objects. But 'cross-section' clearly does not have the same connotation as 'relative state', nor is describing a system as "smeared out" as loaded as the phrase 'fundamental relativity of states'. Though Everett has an eye on explicating pure wave mechanics, his characterization appears merely metaphorical at this point.

Everett's language becomes more evocative in the following paragraph, where he applies this line of thought to "human observers." He describes a smeared out observer as one that has "split into a number of observers," each described by a cross-section (pp. 4-5). But, in contrast with his later presentations, Everett attempts to clarify these ideas by stopping to consider a concrete example.

> In order to better illustrate the central role of correlations in quantum mechanics we consider the following example: In a box, say a one centimeter cube, we place a proton and an electron, each in momentum eigenstates, so that the position amplitude of each is uniform over the whole box. After a period of time we would expect a hydrogen atom to have formed. Nevertheless the position amplitude of the electron is still uniform over the whole box, just as that of the proton. All that has occured [sic] is that the position densities have become correlated. All that is meant by the statement "There is a hydrogen atom in the box." is the existence of this correlation.
>
> In fact, it is clear from the circumstance that the wave equation is in 3N dimensional space, rather than 3 dimensional, that whenever several systems interact some degree of correlation is produced. [...] Strong correlations will be built up, so that we might say that the particles have coalesced to form a solid object. [...] It is this phenomenon which accounts for the classical appearance of the macroscopic world, the existence of definite solid objects, etc., since we ourselves are strongly correlated to our environment. [...] We now see that the wave mechanical description is really compatible with our ideas about the definiteness on a classical level, due to the existence of strong correlations.

(pp. 6-7)[13]

It would be difficult to overestimate the importance of this passage, but we must proceed carefully. One thing that can be said is that *two particles interacting in a box* is not a circumstance that is naturally described in terms of splitting or multiple branches. The compound wave function will be far from *diagonalized* on any scale approaching the diameter of subatomic particles. Put another way, if we project the compound wave function onto a subspace in which the position of the proton is roughly localized, the position of the electron will not be localized to the same degree. This is apparent if we consider the motion of an electron in a fixed coulomb field. Consequently, the positions of the proton and electron are not even nearly correlated in a "one-one manner," so that we cannot conveniently speak of the "branch" on which the particles have such-and-such positions. Nor does it make sense to try to track the various "trajectories" by which the particles approach each other, since components of the wave function are constantly *interfering*. In fact, it is this very interference that ultimately produces the energy eigenfunctions characteristic of a hydrogen atom.

Understanding the lesson that Everett draws from this example is complicated (once again) by semantic ascent. It is argued that the classical appearance of the macroscopic world is explained by the correlations that build up among elementary particles. These are correlations in Everett's peculiar sense, that we have seen variously described using the terms 'relative state', 'correlation information', 'cross-section', 'branch', 'splitting', and 'smearing out'. But we are now told that this sort of correlation can also license more mundane assertions, such as 'There is a hydrogen atom in the box' or 'There is a solid object.' And, considering the correlations that arise among apparata and object systems, in certain circumstances one may be able to say 'A measurement of the observable $O$ has occurred.' This is all brought together by the fact that a human observer becomes strongly correlated with their environment, which is apparently what allows us to make assertions like 'An observer experiences a classical macroscopic world.' However, as Everett was well-aware, all of these "correlations" consist merely in the fact that the composite wave function is *skewed* toward certain regions

[13]This example and a similar argument also appear in a separate manuscript titled "The existence and meaning of classical objects." There, Everett elaborates on how the *centroid* of several entangled particles can be taken to represent a "solid body."

of configuration space. Moreover, the difficulties in trying to describe such a correlation as the result of a splitting process are all manifestly present in the example of a hydrogen atom in a box.

Everett has not given us a better idea of what smearing out is, or what cross-sections describe. But he has given us a rough-and-ready method for rendering classical descriptions as wave functions, and *vice versa.* Using the hydrogen atom as a paradigm, the general idea seems to be to express a classical state of affairs in terms of a relationship between subsystems and then to identify this relationship with certain *correlations*, in Everett's sense of the word.[14] Measurement, in particular, is reconceived as a relation that comes to hold between an object system and apparatus, then treated according to Everett's general model. For example, photographic film records *where* light has struck it. Thus, we are meant to model the film as a collection of photosensitive molecules whose respective states are overwhelmingly skewed toward being in a *definite location* and ready to absorb a photon. If our model evolves so that the wave function is overwhelmingly skewed towards a certain superposition, each element of which shows that a photon has been absorbed by one of the photosensitive molecules in the film (each at a particular location), then we are meant to conclude that *the location of a photon was recorded.*

Modeling measurement interactions in this way, especially when the observer herself is represented in the model, is supposed to demonstrate the consistency of MW with experience. Conforming to our claim that Everett proposes a kind of logico-linguistic reduction, this consistency is apparently won by determining alternate truth-conditions for certain statements, such that the predictions of the usual collapse theory are (in this alternate sense) preserved. On Everett's scheme for deriving classical descriptions from the wave function, MW predicts that an observation occurs under the same conditions that the standard theory predicts a collapse—when a system fitting the classical description of an apparatus interacts with the object system. As Everett pointed out to Wiener, at no point is it asserted that any par-

[14]Similar ideas have been proposed and explored by other commentators. Geroch (1984) speaks of regions of configuration space that are "precluded" under the unitary dynamics, and he proposes that we express the standard quantum predictions in those terms. Albert and Barrett's interest in the Bare Theory was piqued by the so-called *determinate result property* and *general limiting property* of Everett's ideal observer; which suggest a rationale for giving certain classical descriptions of the wave function (Barrett 1999, sec. 4.2).

ticular measurement outcome has obtained. Assertions like 'the photon was absorbed at location $x$' are generally not warranted under Everett's scheme. However, it is important to note that the *standard quantum theory makes no such predictions.* In his reply to Wiener, Everett can be understood as pointing out that quantum theory has never made determinate predictions, and so (strictly speaking) attributions of specific values to observables do not need to be recovered in a reduction of quantum theory.

Admittedly, Everett takes his account to be an adequate *foundation* for all of his more metaphysically suggestive talk. He does not hesitate to speak of an observer's "life tree" or claim that his formulation entails "the abandonment of the concept of the uniqueness of the observer, with its somewhat disconcerting philosophical implications" ("Probability in wave mechanics," p. 8). He also compares an observer to an intelligent amoeba, to illustrate how the measurement postulate is recovered in MW (ibid.); though, this analogy was dropped in later work—apparently on Wheeler's recommendation. But it should be clear that Everett's *formalism* (what we have called MW) and his *reduction* (the way that he embeds in MW what he retains from standard quantum theory) owe nothing to these ideas. Everett's use of the example of a hydrogen atom to clarify the nature of macroscopic degrees of freedom confirms that the notion of correlation guides the implementation of MW. Many figurative descriptions are used to represent the circumstance that is later labeled 'the fundamental relativity of states'; but none of these descriptions is codified in a way that would suggest that it has a fundamental significance. Arguably, Everett does not ultimately feel compelled to *choose* among these ways of describing "reality," as he hesitantly calls it.

### *3.2. Everett on the role of theory*

One advantage that might be attributed to a *splitting worlds* view is that it gives us some idea of *what it is* (or perhaps *what it's like*) for observation to be a quantum process. Supposedly, being involved in a superposition is *just like* being part of the world that we all experience, except that "our world" is not unique. This is likely why issues regarding the nature of worlds are often considered to be of principal importance to Everettian interpretations. The idea being that there ought to be something definite and constitutive of phenomena. This sort of concern is sometimes raised under the guise of the preferred basis problem.

The preferred basis problem can be compactly stated as the observation that drawing predictions from the Hilbert space formalism would seem to be hopelessly *ad hoc*, given that the formalism itself does not *say anything definite* about quantum systems. That is, the formalism does not indicate which observables one should draw predictions about, and it seems that we cannot make coherent predictions about the values of so-called *incompatible* observables in the same instance. Hence, it is often charged that one *chooses* to draw predictions regarding the observables that one is already accustomed to thinking of as determinate. The challenge for an Everettian interpretation is to show that it is less of a *just-so* theory than collapse or hidden-variable interpretations. And Everettians have most often tried to meet this challenge by showing how worlds (or minds) arise within the Hilbert space formalism. And most have assumed—with good reason—that this is what Everett was up to.

On our account, and as most would agree, Everett fails to show how definite worlds arise in pure wave mechanics.[15] We have argued, however, that Everett's project is not (at base) about "making worlds" out of the universal wave function. This is born out by the fact that he does not describe a genuine splitting process, which would *ipso facto* solve the preferred basis problem. In that case, one should ask: How could Everett have thought that he had succeeded in drawing predictions from quantum theory in an unobjectionable way? How *has* he addressed the measurement problem?

These questions ask for what we have called a *criterion of success*. There are important clues as to what Everett's criterion of success might be in his correspondence with DeWitt and Wiener. But a close examination reveals that his replies are based on comments that he makes in an appendix to the long thesis, titled "Remarks on the role of theoretical physics."[16] Here he lays out his position on the purpose of physical theories and, by extension, the qualities on which a physical theory should be judged. Everett makes a systematic attempt to support general antirealist theses that he gestures at elsewhere. In particular, we find the following argument:

[15]The preferred basis problem can be posed for correlations as well; because, if there is any correlation between observables for two subsystems, then there are infinitely many (mutually incompatible) observables that are also correlated. Even worse, correlations are sensitive to the way that the boundary between two subsystems is drawn, so that this choice will generally create a bias toward certain kinds of correlations.

[16]Everett's personal records show that this appendix began as a stand-alone document titled "Nature and purpose of physical theory"; though, it is not clear why this paper was originally written.

> Every theory can be divided into two separate parts, the formal part, and the interpretive part. [...] The essential point of a theory, then, is that it is a *mathematical model*, together with an *isomorphism* between the model and the world of experience[...] Once we have granted that any physical theory is essentially only a model for the world of experience, we must renounce all hope of finding anything like "*the* correct theory." There is nothing which prevents any number of quite distinct models from being in correspondence with experience (i.e., all "correct")[...] (1973, pp. 133-4)

This is the outline of a generic *underdetermination argument*. It is supposed that the only legitimate reason for ultimately accepting or rejecting a theory has to do with its *empirical adequacy*. Then it is hypothesized that there is an abundance of empirically adequate theories given any finite collection of observations. One is led to the conclusion that the *true* goal of science cannot be to identify *the correct theory*. Therefore, this could not have been Everett's goal either.

But a problem immediately arises. Everett does not fault von Neumann's collapse formulation on the grounds of empirical adequacy, and he does not try to distinguish MW from the standard formulation by their predictions. Quite the opposite, his entire project aims at showing that the predictions of MW and the standard theory are identical wherever their domains of application overlap. So, by his own argument, he does not have any principled reason to reject the standard formulation. The measurement problem, the very reason for his alternate formulation, threatens to slip through his fingers. Everett recognizes this, and he appeals to the activity of *theory construction*.

> Two types of prediction can be distinguished; the prediction of phenomena already understood, in which the theory plays simply the role of a device for compactly summarizing known results (the aspect of most interest to the engineer), and the prediction of new phenomena and effects, unsuspected before the formulation of the theory [...] which is of most interest to the theoretical physicist, and supplies a greater motive to theory construction than that of aiding the engineer. (ibid.)

Everett identifies the competing ethics of *simplicity* and *comprehensiveness*, corresponding to the interests of the engineer and theoretical physicist respectively. Both the simplicity and comprehensiveness of a theory are capable of influencing one's subjective "confidence" in that theory, and so theory construction is sensitive to both (p. 135).

The measurement problem can be captured, in this way of thinking, as an inherent limitation in the comprehensiveness of the standard (external observation) formulation. In particular, Everett judges that this formulation is incapable of unifying cosmology and quantum theory. The theoretical physicist, who is responsible for what the next generation of physical theories will look like, loses confidence in the standard theory and is compelled to consider alternate formulations. One may worry that the theoretical physicist must be equally confident in various formulations, but now it is the engineer's turn to have her say. Everett has already argued, in the introduction to the long thesis, that pure wave mechanics is preferable to considered collapse and hidden-variable alternatives due to its "logical simplicity" (p. 6-8). In other words, the engineer demands that the many virtues of standard quantum theory be conserved as much as possible; particularly in pursuit of a comprehensive theory for which there are no known applications. Thus, the physics community should have more confidence in Everett's formulation than its competitors.

There are several things going on here. First, Everett's view on the structure and purpose of physical theories is not sympathetic to the suggestion that theories ought to represent a *determinate set of facts*. Recall that this is precisely the sort of project that Everett dismisses in his reply to Wiener. He indicates to Wiener that he does not intend to provide a *direct* solution to the preferred basis problem; that is, a formulation that determines which observables have objective values at each moment. Hence, he would likely reject the more robust understanding of the preferred basis problem sketched above; and, consequently, he would not acknowledge that his theory of measurement must describe a process by which the measured observable gains a determinate value. Next, supported by his antirealist argument, Everett asserts that there is something *ad hoc* about the entire scientific enterprise. He even characterizes established science as an efficient way of "summarizing" past observations; so that, in this sense, all theories are ultimately just-so theories. On the other hand, Everett asserts that theory construction is guided by a desire for unification; which can be a significant constraint on the form that a theory takes, and often competes with the desire for a tidy summary of past successes. *Therefore, Everett's complaint about von Neumann's formulation is not that it requires one to make an* ad hoc *choice of measurement basis, but that it codifies this choice in a way that frustrates the* *goal of unification.*

Everett's implicit response to the preferred basis problem was to remove any reference to a determinate measurement outcome (i.e., post-measurement eigenstate) from the *foundations* of quantum theory. In his thesis, he attempts to show that one can model quantum processes without having to impose a measurement basis at any particular point—i.e., by pure wave mechanics. He intends to secure the empirical adequacy of this formulation by a direct reduction of the standard formulation, in which all of the usual predictions are allegedly recovered. In Everett's words, this reduction generates an "isomorphism" between a model and "the world of experience," *via* the correspondence already established in the standard formulation.[17] Neither the model nor the reduction stipulate any *preferred* quantity. Thus, the selection of a basis (if necessary) becomes an artifact of the particular way that a model is *used*, or the particular *question* that is asked. Once again, for Everett, the purpose of a physical theory is not to describe a set of facts, but to *be used as a model of experience.* Of course, quantum theory has been used with great success; that is, it has given good answers to empirically motivated questions. Everett's argument is that the "engineer"—who was satisfied with the external observation formulation—must be equally satisfied with his proposal.

## 4. Conclusions

I contend that I have presented the best way to understand Everett's intended contribution to the quantum measurement problem. We have remained uncommonly faithful to his presentation, and have reconstructed the systematic development of his position. Our understanding of the way that Everett approached the measurement problem explains why he was silent on

[17] There are several points of agreement between the present account and that provided by Jeffrey Barrett (2010a, 2010b). Barrett argues that Everett held a general criterion of empirical acceptability for physical theories, which Everett calls *faithfulness* to experience. This criterion is also exceedingly easy to satisfy. A theory is *faithful* so long as one can find some substructure in a theoretical model that is isomorphic to some substructure in empirical experience. In comparison, I stress the close relationship that Everett maintains between his formulation and the standard theory, and I claim that he intends to co-opt the empirical adequacy of the standard theory for his own. This allows Everett some flexibility on the question of which features of quantum theory ultimately contribute to its empirical adequacy.

various details, without having to conclude that he was oblivious to central issues. In particular, we explain how difficulties related to splitting and the preferred basis problem would not seem pertinent on that approach. We showed that, on a careful reading, Everett's theory of measurement does not make any essential use of a splitting process, so that there is no inherent appeal to a preferred quantity. Moreover, we have argued that Everett held that his theory of measurement need only provide a method for (1) setting up a model of a measurement process, and (2) associating features of this model with the standard predictions—*without* creating an artificial limit to the applicability of quantum theory. His exceedingly modest criterion of success explains how Everett thought that he had addressed measurement without engaging in the problem of selecting a measurement basis.

I do not claim to have presented the most satisfying many-worlds interpretation. Anyone who sees the preferred basis problem as a particularly deep or persistent difficulty will likely hold that it cannot be sidestepped in this way. Others will find it easy to reject this approach because of its reliance on ideas that have gone out of favor in the philosophy of science. However, to the extent that our reading of Everett's project differs from previous elaborations, it may represent a distinct class of many-worlds interpretation. At the very least it shows that Everett's conception of his formulation differed importantly from those many-worlds interpretations that attempt to explicate the nature of splitting and branches.

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