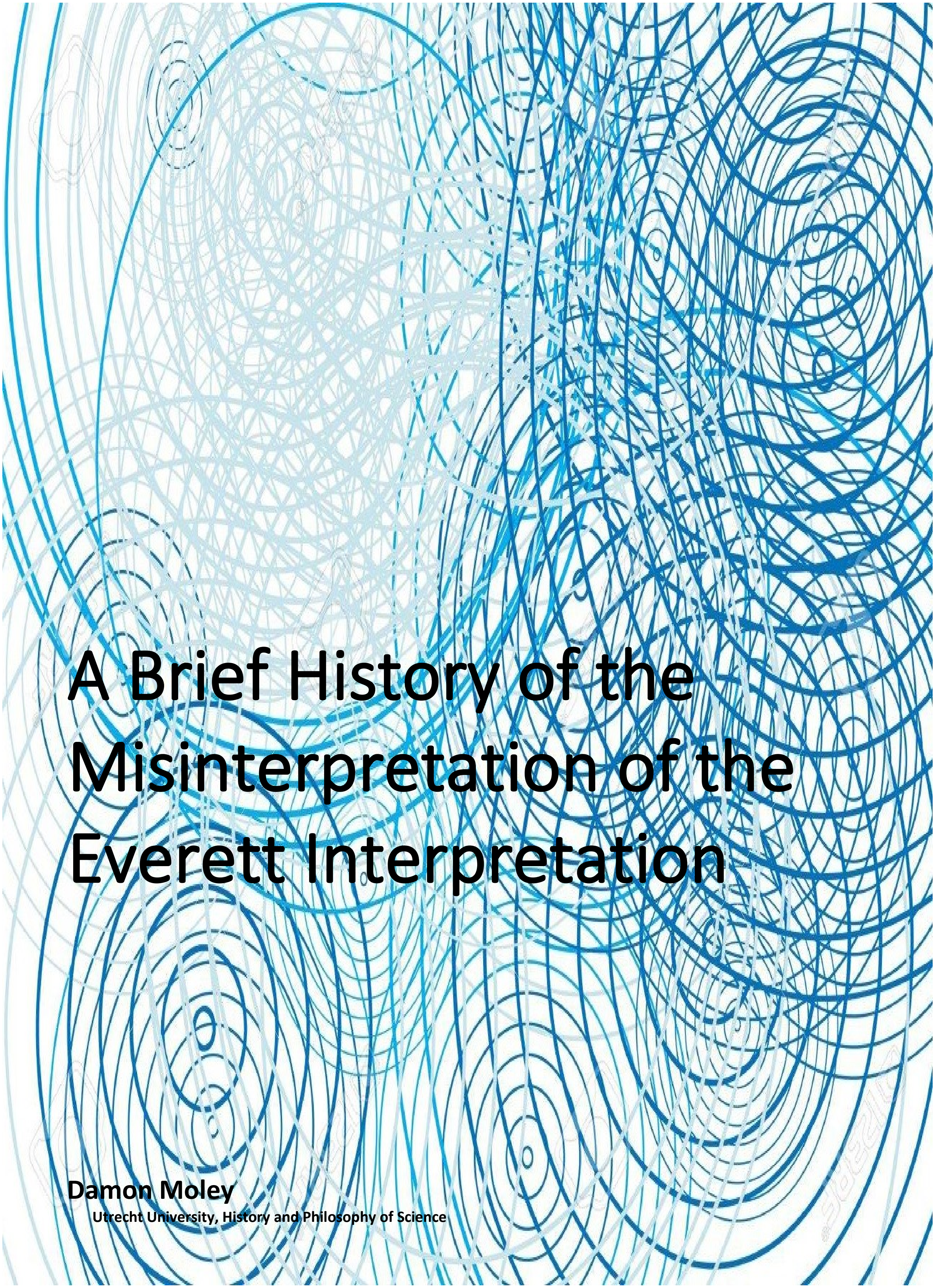

A Brief History of the Misinterpretation of the Everett Interpretation

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Epigraphs

“It ain't so much the things that people don't know that makes trouble in this world, as it is the things that people know that ain't so.”

Mark Twain

“A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die and a new generation grows up that is familiar with it. . . . An important scientific innovation rarely makes its way by gradually winning over and converting its opponents: it rarely happens that Saul becomes Paul. What does happen is that its opponents gradually die out, and that the growing generation is familiarized with the ideas from the beginning: another instance of the fact that the future lies with the youth.”

Max Planck

“Countlessly, innumerably, boundlessly, incomparably, incalculably, unspeakably, inconceivably, immeasurably, inexplicably many worlds…”

Ancient Buddhist text

Acknowledgements

I would like to thank Guido Bacciagaluppi and Ronnie Hermens for their careful comments and advice. Great credit is due to Jeffrey Barrett, Peter Byrne and James Weatherall for their work gathering and publishing Everett's papers.

A Brief History of the Misinterpretation of the Everett Interpretation

In April of 1956, Hugh Everett III's draft doctoral thesis, tentatively titled "Wave Mechanics without Probability", was circulated by his thesis advisor at Princeton University John Wheeler to several leading physicists in the West, including the godfather of quantum mechanics Niels Bohr. Everett aimed at resolving the measurement problem in quantum mechanics. Everett's solution to the mystery of how, when, where and why the wave function collapses in the measurement process was straightforward: it does not. Due to the opposition of Bohr and other adherents to the Copenhagen Interpretation of quantum mechanics, an objection by Feynman and Wheeler's growing ambivalence, Everett was required to abridge his thesis, resulting in the publication of "'Relative State' Formulation of Quantum Mechanics" in July 1957. Everett's machinery of interpretation, including his information and measure theoretic interpretation of the wave function, his concept of correlation within the superposition, as well as his philosophy of science which provides important context for his theory, are largely absent from the latter "Short Thesis". Perhaps due to the unusual publication history of his works, many critics of Everett and even many of his supporters have overlooked the significance of the arguments made in the "Long Thesis", which was published only in 1973. Everett's fuller exposition there answers often cited criticisms such as Adrian Kent's "Against Many Worlds Interpretations". Other popular misconceptions about Everett, that he was driven out of academia or that his thesis was redacted due solely or primarily to the opposition from Copenhagen, do not stand up to careful scrutiny of the historical evidence.

Section 1: Introduction

In April of 1956, Hugh Everett III's draft doctoral thesis, tentatively titled "Wave Mechanics without Probability", was circulated by his thesis advisor at Princeton University John Wheeler to several leading physicists in the West, including the godfather of quantum mechanics Niels Bohr.[1] Wheeler himself was one of the most important physicists in the United States: he had done significant early work in quantum mechanical atomic theory; already mentored several brilliant young scientists in the new paradigms of their discipline, including Richard Feynman; and led teams of theoreticians and engineers on the Manhattan Project and the development of the hydrogen bomb.[2] It was not every day that a draft Ph.D. thesis garnered such attention, but Wheeler would argue that Everett had reformed and

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[1] Barret and Byrne p. 18-19, 72, 203-224. Niels Bohr, Aage Petersen, Alexander Stern, H.J. Groenewold are known from correspondence to have received this draft. Stern held a seminar at the Institute for Theoretical Physics at Copenhagen on the subject of this draft at that time, and Everett's then still unpublished ideas were also discussed at the Conference on the Role of Gravitation in Physics held at the University of North Carolina, Chapel Hill, in January 1957 with Wheeler, Bryce DeWitt, Cecile DeWitt-Morette, Richard Feynman and others in attendance.

[2] Byrne, Peter p. 117-130

generalized the foundational principles of quantum mechanics in such a way as to make that theory more amenable to general relativity, an historic accomplishment if true.[3]

Everett conceived his theory with a different objective in mind. Everett aimed to solve the “measurement problem” in quantum mechanics which had been recognized as an inconsistency in the axioms of that theory by many in the textbook treatment of the subject since von Neumann set them down in 1932.[4] Von Neumann wrote that a physical system evolves according to a linear equation deterministically while unobserved, an evolution he labeled as “process 2”.[5] This process 2 is responsible for the theoretical reality of superposition which is inferred from the results of the double slit and similar experiments. Separately, when a measurement is made, that system evolves instantaneously by another process which is indeterministic, a process he labeled “process 1”.[6] The theory requires one accept an essential duality in nature insofar as a system obeyed one law when not observed and a different one when an observation is made. Further, von Neumann well understood that placing the boundary between process 2 and process 1 was an arbitrary exercise, *i.e.* what constituted an “observer” or “measurement” was not well defined, but that there had to be a privileged observer making a measurement was necessary to this construction.[7]

Since Copernicus and Newton, philosophers and scientists have been very wary of privilege in the formulation of the laws of nature, regarding such inconsistency as a sign that they have gone wrong. Einstein, in particular, objected to privileged “observers” and “measurements”. Two years prior to Everett completing this draft, it is thought he attended one of Einstein’s last lectures at Princeton. There Einstein remarked, “It is difficult to believe that the description [of quantum mechanics] is complete. It seems to make the world quite nebulous unless somebody, like a mouse, is looking at it.”[8] Further, this privileged process 1 is arguably at odds with the principles of locality and determinism, both objections that were raised by Einstein on other occasions. These are also crucial aspects of the “measurement problem” in quantum mechanics.

In his circulated draft, Everett writes, “The present thesis is devoted to showing that this concept of a universal wave mechanics, together with the necessary correlation machinery for its interpretation, forms a logically self consistent description of a universe in which several observers are at work.”[9] Everett’s solution to the mystery of how, when, where and why the wave function collapses in the measurement process was straightforward: it does not. Everett proposed that all physical systems evolve according to process 2, linearly and deterministically, with no privileged process involving arbitrarily designated “observers” or “measurements”. As he put it in a rare appearance before an

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[3] Barret and Byrne p. 201-202

[4] Von Neumann p. 271

[5] *Ibid*

[6] *Ibid*

[7] *Ibid* p. 272-273. “Now quantum mechanics describes events which occur in observed portions of the world, so long as they do not interact with the observing portion, and does so by means of process 2 (V.1). But as soon such an interaction does occur— i.e., a measurement is made—the theory requires application of process 1. This duality is therefore fundamental to the theory.”

[8] Barrett and Byrne p. 15

[9] *Ibid* p. 77

audience of eminent physicists in 1962, “My position is simply that I think you can make a tenable theory out of allowing the superpositions to continue forever.”[10]

Everett recognized the startling theoretical consequence of evolving physical systems by process 2 alone: the universe we observe directly is only one of a possibly infinite number of universes. Everett believed that the subjective appearance of probabilistic outcomes of experiments falls out naturally as a result. To rigorously define the structure within the wave function, he looked to the new theory of information. With these tools and those that measure theory provided, Everett described correlations within the superposition that map to our experience of reality and reproduced the Born rule for subjective probabilities that agree with experiment in his completely deterministic theory.

Everett anticipated that his theory and its counterintuitive consequences would be met with opposition. He was correct, although he could not have known the extent of the historic controversy he initiated. The original “Long Thesis”, circulated in 1956 under the title “Wave Mechanics without Probability” and ultimately published in 1973 as “The Theory of the Universal Wave function”, and the abridged “Short Thesis”, published in 1957 under the title “’Relative State’ Formulation of Quantum Mechanics”, provoked responses ranging from philosophical protests and scientific misunderstanding to charges of heresy and utter disbelief.[11] Some of his earliest notes on his thesis are not about physical theory but rhetorical strategies. On the reverse of his handwritten draft of the introduction to his thesis wherein he describes the inconsistencies of von Neumann’s process 1 and process 2, Everett writes in big bold block letters: “PUTTING OTHER FELLOW ON DEFENSIVE”.[12] Wheeler’s efforts to convince his mentor Niels Bohr and others of the importance of Everett’s theory were in vain, with the exception of Bryce DeWitt; and even DeWitt, who became the most important evangelist of Everett’s theory, had his moment of doubt.

As critical reviews came in and opposition mounted, Wheeler grew ambivalent about Everett’s draft “Wave Mechanics without Probability”. Nonetheless, Wheeler remained convinced that Everett’s theory could contribute to his own research program on quantum gravity, if Everett could present it in such a way as to placate its detractors. After a public criticism by his favorite former student Richard Feynman, Wheeler required Everett to rewrite his thesis, cutting it to a quarter of its original length, discarding entire chapters and omitting lines of argument in order to make it, in Wheeler’s words, “javelin proof”.[13] The abbreviation of Everett’s argument did not improve its reception.

Part of the opposition to Everett’s ideas is attributable to ideological objections by advocates of rival theories; some part, to aesthetic or philosophical objections to a multiverse; another part, to simple misunderstanding of Everett’s lengthy argument or ignorance of it. Some of the latter is due to the strange publication history of Everett’s two major works and the differences between them. The Long Thesis is devoted to dissolving the measurement problem in quantum mechanics. The Short Thesis is construed in such a way as to present a theory of quantum mechanics amenable to reconciliation with general relativity. Everett’s original objective is different from Wheeler’s directed revision, and appropriately, the supporting arguments are different. The Long Thesis is more often cited than read, even by prominent scholars who describe themselves as Everettians and authorities on the subject of

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[10] *Ibid* p. 273

[11] *Ibid* “Short Thesis” and “Long Thesis” are Barrett’s coinage from his comprehensive edition of Everett’s work.

[12] *Ibid* p. 329

[13] *Ibid* p. 4

the foundations of physics.[14] Although Everett's elaboration of correlation within the superposition, the role of information theory in defining those correlations and the construction of his measure occupies Everett for the majority of the Long Thesis, these concepts are barely mentioned or are entirely absent from the Short Thesis. Everett also wrote an appendix to the Long Thesis on the role of theory in physics as he saw it, which anticipated many of the philosophical objections his critics have made. These arguments are often overlooked completely or given scant attention in what is considered important critical scholarship on Everett's theory.

Unless noted otherwise, the reader may assume that quotes from Everett's theory are taken from the Long Thesis in this paper. In the next Section, I will describe Everett's philosophy of science and his views on the role of theory in physics as he laid them out in his appendix to the Long Thesis. Everett's philosophy provides important context for his theory, and this exposition will help us understand how Everett believed his thesis fulfilled the criteria of a good theory. In Section 3, I will show how Everett dissolved the measurement problem, employed information and measure theory to define correlations within the superposition and derived the Born rule for predicting the subjective experience of experimental outcomes in his wave mechanics without probabilities. In Section 4, I will sketch the early history of the misinterpretation of the Everett interpretation. Supporters and critics alike misconstrued or misrepresented Everett's ideas, either because of their own agendas, various philosophical objections, ignorance of Everett's complete argument or misunderstanding of the theory's logical consequences. I will also show how the popular narratives about Everett, that he was driven out of academia or that his thesis was redacted due solely or primarily to the opposition from Copenhagen, do not stand up to careful scrutiny of the historical evidence. In Section 5, I will examine criticisms made by Adrian Kent in "Against Many Worlds" and show how Everett's fuller explication in the Long Thesis answers his objections. Finally, I will conclude by discussing the historical narratives about Everett and his theory, how he understood his theory and the controversy over it in his time and how that controversy can be viewed from a sociological and historical perspective.

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[14] Carroll. In this podcast, Sean Carroll and David Albert discuss Everett's theory. In the course of their discussion, David Albert says that he has not read the complete published works of Everett, and Sean Carroll asks about the provenance of the measure theoretic interpretation of the wave function, which appears briefly in the Short Thesis but is elaborated on at length in the Long Thesis.

Section 2: Everett's Philosophy of Science

Putting Everett's Theory in Philosophical Context

Everett was cognizant of and influenced by the scientific and philosophical debates of his time. He had a keen interest in the philosophy of science and a subtle understanding of theory. Although he had little formal study of philosophy, he read the subject on his own and took the unusual step of appending a section to his thesis entitled "Remarks on the Role of Theoretical Physics" in which he lays out his views on theories as models, the structure of theories, the criteria for theory selection and causality.[15]

Everett understood quantum mechanics to be a partial theory and undetermined by the evidence, although he did not use these terms. A partial theory is a model limited to a certain extent within a domain. The partiality of quantum mechanics in Physics follows from the fact that it is not a theory of spacetime.[16] It is background dependent, space and time are taken as a given coordinate system. Making quantum mechanics more amenable to the theory of general relativity was one of his thesis advisor's hopes for his theory. A theory is underdetermined if more than one model might map to the evidence. The underdetermination of quantum mechanics is evident from the different formalisms and interpretations, *e.g.* matrix mechanics of the Heisenberg picture, the Schrödinger equation, von Neumann's statistical interpretation, the Copenhagen interpretation, Hidden Variables interpretations, all of which and more he discusses in the Long Thesis.[17] Everett acknowledges the merits of some of the different interpretations and that his interpretation is practically indistinguishable from them.[18] In writing about the stochastic process interpretation of Bopp, he writes, "(T)here can be no fundamental objection to the idea of a stochastic theory, except on grounds of a naked prejudice for determinism."[19] Everett put himself in the determinism camp but was clear eyed about why he was there.

Everett was writing in the wake of the logical positivist movement of the 1920's and 30's. He never identifies with this movement wholly, and he remarks on the limits of its success in his appendix.[20] Everett did see his views as closely resembling those of Philipp Frank, a professor in the physics department at Harvard University, an original member of the Vienna Circle and author of several books on the philosophy of science. [21] Everett wrote to Frank and sent him a preprint of the Short Thesis.[22] This correspondence is even more remarkable because Frank appears to have been one of the very few

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[15] Barret and Byrne p. 294. In correspondence with Max Jammer, Everett writes: "the only formal course in philosophy or psychology that I had was an introduction to epistemology at Catholic University."

[16] *Ibid* p. 176. Everett implicitly acknowledges the partiality of quantum mechanics in the Short Thesis: "How is one to apply the conventional formulation of quantum mechanics to the space-time geometry itself?"

[17] *Ibid* p. 75, 151. See Everett's exposition of alternate approaches to the measurement problem in the Introduction and his final Discussion section of the Long Thesis for his treatment of other interpretations.

[18] *Ibid* p. 149. Everett believed that his theory was in principle distinguishable due to the possibility of re-interfering branches of the wave function.

[19] *Ibid* p. 156

[20] *Ibid* p. 171. Everett writes: "The critical examination of just what quantities are observable in a theory does, however, play a useful role, since it gives an insight into ways of modification of a theory when it becomes necessary. A good example of this process is the development of Special Relativity. Such successes of the positivist viewpoint, when used merely as a tool for deciding which modifications of a theory are possible, in no way justify its universal adoption as a general principle which all theories must satisfy*."*

[21] *Ibid* p. 257-260

[22] *Ibid*

academics with whom Everett shared his ideas of his own volition. Wheeler urged Everett to defend his theory from critics in the Copenhagen school, but Everett was reticent, as we shall see.

Everett's Admiration of Philipp Frank

Everett writes to Frank:

> "As a result of membership in the "Library of Science" book club, I have received several of your works on the philosophy of science, I have found them extremely stimulating and valuable. 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." [23]

Barrett surmises that Everett may have read Frank's *Modern Science and its Philosophy*.[24] In that work, Frank describes the evolution of the thinking of the Vienna Circle and the influences on his own thought: from their admiration of Ernst Mach's view of theories as explanatory descriptions of sense experience and Poincaré's assertion that theories are logically consistent constructions, which became the foundation for their "scientific world conception", later known as logical positivism; to their subsequent discovery of agreement between themselves and American pragmatists and empiricists such as William James and C.S. Pierce; and Frank's own appreciation of operationalists like P.W. Bridgman.[25] Barrett writes that Frank's views in this late period of his work can best be described as "empirical pragmatism or operationalism." [26] Empirical pragmatism or operationalism were not terms that Everett ever used in his writing, but they do aptly describe Everett's philosophy, contrary to the realist philosophical stance that many critics and supporters alike have attributed to him, as we shall see.

Everett's Understanding of Theory Structure

For Everett, a theory was a model. In his appendix on the role of physical theory, he writes:

> "Every theory can be divided into two separate parts: the formal part and the interpretive part. The formal part consists of a purely logico-mathematical structure, i.e., a collection of symbols together with rules for their manipulations, while the interpretive part consists of a set of "associations," which are rules which put some of the elements of the formal part into correspondence with the perceived world. 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 (*i.e.*, the sense perceptions of the individual, or the "real world"— depending upon one's choice of epistemology)."[27]

His distinction between the model and the interpretative part is important to understanding how Everett saw the role that his machinery of interpretation plays, that is his concept of correlation information within the superposition and the construction of his measure which represents the

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[23] *Ibid* p. 257-258

[24] *Ibid* p. 260

[25] Frank p. 1-52

[26] Barrett and Byrne p. 260

[27] *Ibid* p. 169

experience of a typical observer. These concepts will map the model output to the world. In this regard, Everett's note on the word "isomorphism" as it appears above is revealing:

> "By isomorphism we mean a mapping of some elements of the model into elements of the perceived world which has the property that the model is faithful, that is, if in the model a symbol A implies a symbol B, and A corresponds to the happening of an event in the perceived world, then the event corresponding to B must also obtain. The word homomorphism would be technically more correct, since there may not be a one-one correspondence between the model and the external world."[28]

In his own theory, sense experience is captured in memory. Everett writes, "In order to make deductions about the past experience of an observer it is sufficient to deduce the present contents of the memory as it appears within the mathematical model."[29] This is the part of the model that is homomorphic to the world of experience.

Everett's Epistemology

As to his own epistemology, Everett again is never explicit about labeling his views, but one has the sense from his writings of a considered pragmatism and agnosticism on the subject. He was careful in his language regarding the relation of theory to the world. He almost always qualified it as a relation to "the perceived world" or "elements of the perceived world".[30] In other places, he writes about theoretical prediction relating to "experience" or "sense experience".[31] When Everett wrote about "reality" or the "real world", he offset those words with quotation marks. Apropos, Everett remarks on how successful theories become identified with "reality" over time, but this identity is false.[32] He writes:

> "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"), and furthermore no way of ever verifying that any model is completely correct, simply because the totality of all experience is never accessible to us."[33]

Everett points out that different models might have different practical applications, *e.g.* classical physics admits far simpler solutions than relativity or quantum mechanics in many cases where the latter provide more accurate results that are of negligible significance, and he advocates for the most useful theories in their respective domains.[34]

It is helpful to make a distinction between what Everett believed his theory described about "reality" and what he thought about the relationship of theories to "reality". Everett believed that his theory of pure wave mechanics described a non-denumerable infinity of branching worlds that were equally

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[28] *Ibid* p. 172

[29] *Ibid* p. 183

[30] *Ibid* p. 253. Examples of this phraseology are found in the Appendix II to the Long Thesis (p. 169, 172) and Everett's letter to DeWitt ().

[31] *Ibid* p. 252-255. Examples found in Everett's Letter to Dewitt

[32] *Ibid* p. 169-170

[33] *Ibid* p. 170

[34] *Ibid*

"real".[35] At the same time, he believed that theories, including his own, were only models that mapped to some part of the world of experience. So Everett might quite comfortably assert that his theory described many equally "real" worlds and, in the next breath, acknowledge that his theory is only a model and that the whole of "reality" is never epistemically available to us.

Everett's Criteria for Theory Selection and Thoughts on Causality

Having abandoned the idea of "the correct theory", Everett lays out his criteria for theory evaluation. He puts logical consistency and "correctness", which he defines as empirical faithfulness in the sense that the model is, at least, homomorphic to experience, first.[36] As it may be the case that more than one theory adequately meets these standards, Everett writes that one may resort to further criteria: usefulness; simplicity; comprehensiveness; and pictorability.[37] It is worth noting that simplicity for Everett is conceptual simplicity.[38] The example he gives of simplicity is Einstein's general relativity, which is conceptually simple and elegant, notwithstanding the fiendish complexity of solutions to its equations which model all but the most trivial systems.[39]

Everett closes his "Remarks on the Role of Theoretical Physics" with some comments regarding the concept of causality. His views on the subject are Humean. Everett writes:

> "It should be clearly recognized that causality is a property of a model and not a property of the world of experience… A theory contains relations of the form "A implies B," which can be read as "A causes B," while our experience, uninterpreted by any theory, gives nothing of the sort, but only a correlation between the event corresponding to B and that corresponding to A."[40]

Everett's view of causality and correlation, his understanding of theory structure, his epistemology and criteria for theory evaluation all fit neatly with his own theory, as we shall see.

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[35] *Ibid* p. 274-275. See Everett's discussion with Podolsy, Shimony et al at the Conference on the Foundations of Quantum Mechanics at Xavier University in 1962.

[36] *Ibid* p. 172

[37] *Ibid*

[38] *Ibid* p. 171

[39] *Ibid*

[40] *Ibid* p. 172

Section 3: Information, Correlation and Measure in Everett's Theory

The Origin of Everett's Thesis: the Measurement Problem, an Enticing Paradox; Everett's Influences

Everett begins the Long Thesis with an "extremely hypothetical drama" that illustrates the measurement problem.[41] In this thought experiment, we have an "observer A" in an isolated room performing measurements on a "system S".[42] The composite system A + S in turn is the object of "observer B". Everett claims that von Neumann's formulation of quantum mechanics leads to a contradiction: for observer A, system S evolves according to process 1 discontinuously and probabilistically when measurements are made; but for observer B, the composite system of observer A and system S evolves according to process 2 linearly and deterministically regardless of what observer A might do. Both descriptions cannot be complete and correct. Everett writes, "either A is incorrect in assuming Process 1, with its probabilistic implications, to apply to his measurements, or else B's state function, with its purely causal character, is an inadequate description of what is happening to A + S."[43]

Everett's story shows the inconsistency of the textbook formulation of quantum mechanics with its two processes and the arbitrary nature of what constitutes an "observer" or a "measurement" in that theory. Everett read von Neumann's seminal *Mathematical Foundations of Quantum Mechanics* at Princeton. In correspondence with Max Jammer who was researching for his book *The Philosophy of Quantum Mechanics* in 1973, Everett cited von Neumann as one of his most important influences in writing his thesis.[44] Everett was careful to structure his argument so as to reproduce the strengths, structure and rigor of von Neumann's theory without its faults, as he saw them.

As mentioned above, von Neumann understood that the distinction between observers and other systems was not well defined but necessary to his formulation:

> "We are obliged always to divide the world into two parts, the one being the observed system, the other the observer. In the former we can follow all physical processes (in principle at least) arbitrarily precisely. In the latter, this is meaningless. The boundary between the two is arbitrary to a very large extent. In particular, we saw in the four different possibilities considered in the preceding example that the "observer"—in this sense—need not be identified with the body of the actual observer: in one instance we included even the thermometer in it, while in another instance even the eyes and optic nerve were not included. That this boundary can be pushed arbitrarily far into the interior of the body of the actual observer is the content of the principle of psycho-physical parallelism. But this does not change the fact that in every account the boundary must be put somewhere if the principle is not to be rendered vacuous; *i.e.*, if a comparison with experience is to be possible. Indeed, experience only makes statements of this

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[41] *Ibid* p. 74-75

[42] *Ibid* p. 14-15. Everett's drama is what has become known as a Wigner's Friend story, after Eugene Wigner who made a similar illustration of the measurement problem more widely known in his "Remarks on the Mind Body Problem" published in 1961. Everett was a student of Wigner's at Princeton in 1954, when he was already researching his thesis. Everett's illustration of the measurement problem with two observers at work was written prior to Wigner's publication.

[43] *Ibid* p. 74

[44] *Ibid* p. 294

> type: "An observer has made a certain (subjective) observation," and never any like this: "A physical quantity has a certain value.""[45]

The principle of psycho-physical parallelism is an argument in the context of the mind-body debate that is mute on the question of whether or not there is mental reality dual to the physical but insists that if there is a mind, it supervenes on a functional physical structure. For von Neumann, this principle "is a fundamental requirement of the scientific viewpoint", that is "it must be possible to describe the extra-physical process of subjective perspective as if it were in the reality of the physical world."[46]

From the previous discussion of Everett's epistemology and his remarks to the effect that elements of the perceived world or memories of them are the true subjects of scientific theories, one can see how he was in agreement with von Neumann on the significance of psycho-physical parallelism and the subjective nature of experience forming the basis of our understanding of physics. However, Everett believed that von Neumann's theory violated the principle in so far as observed systems, for some (admittedly not well defined) definition of observer, have a physical privilege, evolving by process 1, that the rest of physical reality does not enjoy.[47] Everett writes, "To do justice to this principle we must insist that we be able to conceive of mechanical devices…, obeying natural laws, which we would be willing to call observers… Such systems can be conceived as automatically functioning machines (servomechanisms) possessing recording devices (memory) … which are capable of responding to their environment."[48] By turning the principle of psycho-physical parallelism against a privileged process 1 for observation, Everett preempts any argument to the effect that consciousness plays a special role in physics and clears the way for his dissolution of the measurement problem: "The behavior of these observers shall always be treated within the framework of wave mechanics", *i.e*. by process 2 only.[49]

Everett writes, "We shall deduce the probabilistic assertions of Process 1 as subjective appearances to such observers, thus placing the theory in correspondence with experience. We are then led to the novel situation in which the formal theory is objectively continuous and causal, while subjectively discontinuous and probabilistic."[50] As mentioned in the review of Everett's philosophy, the memory of the observer system is the record of experience for Everett and the subject of his theory. This follows from his model of observers and interactions, as well as we shall see.

Everett wrote to Max Jammer that, in addition to von Neumann, his major influence in writing his thesis was David Bohm's *Quantum Theory*, especially "the later chapters".[51] Barret comments that the influence of Bohm is apparent yet subtle.[52] In that book's second to last chapter "Quantum Theory of the Process of Measurement", Bohm writes, "(A)t the quantum level of accuracy the entire universe (including, of course, all observers of it), must be regarded as forming a single indivisible unit with every object linked to its surroundings by indivisible and incompletely controllable quanta."[53] Similar to Everett's mechanical observers, Bohm makes the point that any system functioning by the laws of

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[45] von Neumann p. 272-273

[46] *Ibid* p. 272

[47] Barret and Byrne p. 76

[48] *Ibid* p. 76-77

[49] *Ibid* p. 77

[50] *Ibid* p. 77

[51] *Ibid* p. 294

[52] *Ibid* p. 298

[53] Bohm p. 584

nature might be regarded as an observational apparatus.[54] Memory or records play an important part for Bohm as well: "For our present purpose of showing that the quantum theory is able to give a consistent account of the process of measurement, it will be adequate to confine ourselves to the cases in which the data are actually recorded."[55] In other places, Bohm states repeatedly how after an interaction systems are carried over linearly and deterministically into a correlated state of a composite system.[56]

Bohm's solution to the measurement problem in *Quantum Theory* is very different from Everett's. He proposes to describe observers separately as classical and shows how interactions strong enough to be called measurements destroy definite phase relations within the superposition.[57] Everett would not have agreed that either or both of these sufficed to answer the measurement problem satisfactorily. He would have objected to the classical description of the observer and noted that the destruction of phase relations did eliminate those components of the wave function. Nonetheless, one can see the influence of Bohm's arguments and how the germ of the idea for disposing of von Neumann's process 1 and describing measurement and observers wave mechanically might have been planted.

Everett's Machinery of Interpretation: the Relative State of the Wave Function; Correlation and Information

Everett introduces two concepts, the "relative state" of subsystems within the superposition described by wave mechanics and the "correlations" between them, that are essential to the interpretative part of his theory:

> "(W)e shall exploit the correlation between subsystems of a composite system which is described by a state function. A subsystem of such a composite system does not, in general, possess an independent state function. That is, in general a composite system cannot be represented by a single pair of subsystem states, but can be represented only by a *superposition* of such pairs of subsystem states. For example, the Schrödinger wave function for a pair of particles, $\psi(x_1, x_2)$, cannot always be written in the form $\psi = \phi(x_1)\eta(x_2)$, but only in the form $\psi = \sum_{i,j} a_{ij}\phi^i(x_1)\eta^j(x_2)$. In the latter case, there is no single state for Particle 1 alone or Particle 2 alone, but only the superposition of such cases. In fact, to any arbitrary choice of state for one subsystem there will correspond a *relative state* for the other subsystem, which will generally be dependent upon the choice of state for the first subsystem, so that the state of one subsystem is not independent, but correlated to the state of the remaining subsystem."[58]

Applied to a model of observation, the system of interest will be correlated to a relative state of the observer system, as shown below.

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[54] *Ibid* See Part VI, Chapter 22, Section 2 "The Nature of the Observing Apparatus".

[55] *Ibid* p. 590

[56] *Ibid* p. 602-603. *E.g.*: "After the spin has interacted with the apparatus that measures its value, there is clearly no single wave function belonging to the spin alone, but, instead, there is only a combined wave function in which spin and apparatus co-ordinates are inextricably bound up."

[57] *Ibid* See Part VI, Chapter 22 "Quantum Theory of the Process of Measurement" of *Quantum Theory* for Bohm's argument, as Everett would have known it.

[58] *Ibid* p. 76

Again, Everett may have been inspired by Bohm. Bohm introduces the idea of correlations within the wave function as useful to understanding the relation of observables in the superposition in *Quantum Theory*.[59] In this example, Bohm is contemplating a single free particle, and P(*k*) and P(*x*) are the canonical conjugates momentum and position, but his principle of correlation extends to composite, entangled systems in his theory as in Everett's:

> "Now, P(*k*) and P(*x*) are not independent of each other, but are related by the fact that they are both determined from the same wave function… Therefore it is not, in general, possible to give the two of them arbitrary sets of values independently of each other… The preceding result shows that the wave function ψ(*x*) determines at least two related probabilities. We shall see later that many more probabilities are determined by ψ(*x*); in fact, the probabilities of all possible physical measurements. The wave function has often been called a "wave of probability," but a more accurate term is "a wave from which many related probabilities can be calculated.""[60]

Bohm's wave of related probabilities is remarkably similar to Everett's wave of correlated, relative states.

The correlations between relative states will provide Everett with the tools he needs to begin to map our experience of the world to the superposition described by wave mechanics. He writes:

> "We now introduce the concept of a relative state-function, which will play a central role in our interpretation of pure wave mechanics. Consider a composite system $S = S_1 + S_2$ in the state ψ^S. To every state η of S_2 we associate a state of S_1, Ψ^{η}_{rel}, called the relative state in S_1 for η in S_2, through:

$$\text{Definition.}\Psi^{\eta}_{rel} = N \sum_i (\phi_i \eta, \Psi^S)\, \phi_i \ ,$$

where $\{\phi_i\}$ is any complete orthonormal set in S_1 and *N* is a normalization constant. The first property of Ψ^{η}_{rel} is its uniqueness, i.e., its dependence upon the choice of the basis $\{\phi_i\}$ is only apparent… The second property of the relative state, which justifies its name, is that $\Psi^{\theta_j}_{rel}$ correctly gives the *conditional expectations* of all operators in S_1, conditioned by the state θ_j in S_2."[61]

To make his concept of correlation between relative states rigorous, Everett looked to the new theory of information. Everett had read Claude Shannon's *The Mathematical Theory of Communication* published in 1949, and therein he found what he needed to replace probability in his deterministic theory.[62] He sketches his scheme in the introduction to the Long Thesis:

> "The mathematical development of these notions will be carried out in the next chapter (II) using some concepts borrowed from Information Theory… Throughout Chapter II we shall use the language of probability theory to facilitate the exposition, and because it enables us to

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[59] Bohm. See Part II, Chapter 10 "Fluctuations, Correlation, and Eigenfunctions".

[60] *Ibid* p. 95-96 In this example, Bohm is contemplating a single free particle, and P(*k*) and P(*x*) are the canonical conjugates momentum and position. The principle of correlation extends to composite, entangled systems in Bohm's theory as in Everett's.

[61] Barrett and Byrne p. 103-104

[62] *Ibid* p. 172

> introduce in a unified manner a number of concepts that will be of later use. We shall nevertheless subsequently apply the mathematical definitions directly to state functions, by replacing probabilities by square amplitudes, *without*, however, *making any reference to probability models*."[63]

As the title of the Long Thesis indicates, this is wave mechanics without probabilities. Because information theory is based on the same mathematics as probability theory, Everett can show that information describes the wave function but without any connotation of indeterminism.

In the second chapter of the Long Thesis "Probability, Information and Correlation", Everett writes:

> "The first three sections develop definitions and properties of information and correlation for probability distributions over finite sets only. In section four the definition of correlation is extended to distributions over arbitrary sets, and the general invariance of the correlation is proved. Section five then generalizes the definition of information to distributions over arbitrary sets."[64]

In these definitions of information and correlation for finite sets and arbitrary sets, including those with non-numerical values, Everett has the tools he needs to describe the information values and correlations between relative states for any observable in quantum mechanics. To begin, Everett recapitulates the notions of joint probability distributions, marginal distribution, conditional distribution, marginal expectations, conditional expectations, and independence from probability theory. In Section 2, he introduces the basic concepts from information theory: "Suppose that we have a single random variable *X*, with distribution $P(x_i)$. We then define a number, I_X, called the *information* of *X*, to be:

$$I_x = \sum_{\mathrm{i}} P(x_i) \ln P(x_i) = \mathrm{Exp}[\ln P\ (x_i)]$$

, which is a function of the probabilities alone and not of any possible numerical values of the X_i's themselves."[65]

Everett proceeds to define information for independent random variables:

$$I_{X,Y,\ldots,Z} = I_X + I_Y + \ldots + I_Z \quad (\mathrm{X, Y, \ldots, Z\ independent})$$

[66]

And "*conditional information…* a quantity which measures our information about *X, Y, . . . , Z* given that we know that *V. . . W* have taken the particular values $v_m, \ldots, w_n$" which lends itself to application to his relative states:

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[63] *Ibid* p. 79

[64] *Ibid* p. 80

[65] *Ibid* p. 82

[66] *Ibid*

$$\mathrm{I}^{V_m,\dots,W_n}_{XY\dots Z} = \sum_{i,j,\dots k} P^{V_m,\dots,W_n}\left(x_i, y_j, \dots, z_k\right) \ln P^{V_m,\dots,W_n}\left(x_i, y_j, \dots, z_k\right)$$
$$= \mathrm{Exp}^{P^{V_m,\dots,W_n}}[\ln P^{V\_m,\dots,W\_n}\ (x_i, y_j, \dots, z_k)]$$

[67]

Everett's concept of correlation information builds on Shannon's concepts of marginal and conditional information. Specifically, it is the difference between them for given dependent variables:

> "Suppose that we have a pair of random variables, *X* and *Y*, with joint distribution *P(*x_i*,* y_j). If we say that *X* and *Y* are *correlated*, what we intuitively mean is that *one learns something about one variable when he is told the value of the other. Let us focus our attention upon the variable X. If we are not informed of the value of Y, then our information concerning X,* I_X*, is calculated from the marginal distribution P(*x_i*). However, if we are now told that Y has the value* y_j*, then our information about X changes to the information of the conditional distribution* $P^{y_j}(x_i)$, $\mathrm{I}_{\mathrm{X}}^{\mathrm{y_j}}$. *According to what we have said, we wish the degree of correlation to measure how much we learn about X by being informed of Y's value. However, since the change of information,* $\mathrm{I}_X^{\mathrm{y}_j} - I_X$*, may depend upon the particular value,* y_j*, of Y which we are told, the natural thing to do to arrive at a single number to measure the strength of correlation is to consider the expected change in information about X, given that we are to be told the value of Y. This quantity we call the correlation information, or for brevity, the correlation, of X and Y, and denote it by {X, Y}. Thus:* [68]
>
> $$\{X, Y\} = \mathrm{Exp}\left[\mathrm{I}_X^{\mathrm{y}_j} - \mathrm{I}_{\mathrm{X}}\right] = \mathrm{Exp}\left[\mathrm{I}_X^{\mathrm{y}_j}\right] - \mathrm{I}_X\text{"}$$

And from the definitions for expectations from probability theory and the information equations previously established, Everett derives: [69]

$$\{X, Y\} = I_{XY} - I_X - I_Y$$

Correlation is symmetric, and the correlation information is equal to the amount of information gained from assuming the dependence of the two variables. Elsewhere, Everett shows that this definition can be generalized to correlated groups of random variables.[70] Everett's goal is to show how his linear and deterministic theory completely and correctly describes the world of experience as recorded in the memory of observers and recovers all the predictive power of the textbook interpretation.

Everett's Information Theoretic Interpretation of the Wave Function

Everett elaborates on how he will apply information theory to quantum mechanics. He writes:

> "We wish to be able to discuss information and correlation for Hermitian operators *A*, *B*, . . ., with respect to a state function ψ. These quantities are to be computed, through the formulas

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[67] *Ibid*

[68] *Ibid* p. 83-84

[69] *Ibid* p. 84

[70] *Ibid*

> of the preceding chapter, from the square amplitudes of the coefficients of the expansion of ψ in terms of the eigenstates of the operators…
>
> We can define the information of the basis $\{\phi_i\}$ for the state ψ, $I_{(\phi_i)}(\psi)$, to be simply the information of this distribution relative to the uniform measure:
>
> $$I_{(\phi_i)}(\psi) = \sum_i P_i \ln P_i = \sum_i |(\phi_i, \psi)|^2 \ln |(\phi_i, \psi)|^2$$
>
> …
>
> We define the *information of an operator A*, for the state ψ, $I_A(\psi)$, to be the information in the square amplitude distribution over its *eigenvalues*, *i.e.*, the information of the probability distribution over the results of a determination of *A* which is prescribed in the probabilistic interpretation."[71]

Everett considers non-degenerate operators and degenerate operators, noting that non-degenerate operators represent maximal refinement and possess maximal information.[72] He writes:

> "(L)et *A* have eigenfunctions ϕ_{ij} and distinct eigenvalues λ_i. Then define the projections A_i, the projections on the eigenspaces of different eigenvalues of *A*, to be:
>
> $$A_i = \sum_{j=1}^{m_i} [\phi_{ij}]$$
>
> To each such projection there is associated a number m_i, the multiplicity of the degeneracy, which is the dimension of the *i*th eigenspace. In this notation the distribution over the eigenvalues of *A* for the state ψ, P_i, becomes simply:" [73]
>
> $$P_i = P(\lambda_i) = \langle A_i \rangle_\psi$$

The information of the operator *A* is defined: [74]

$$I_A = \sum_i \langle A_i \rangle_\psi \ln \frac{\langle A_i \rangle_\psi}{m_i}$$

> "(F)or a pair of operators, *A* in S_1 and *B* in S_2, for the composite system $S = S_1 + S_2$ with state ψ^S, the joint distribution over eigenvalues is:
>
> $$P_{ij} = P(\lambda_i, \mu_j) = \langle A_i B_j \rangle_{\psi^S}$$
>
> …
>
> The joint information, I_{AB}, is given by:

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[71] *Ibid* p. 103-104

[72] *Ibid*

[73] *Ibid* p. 104

[74] *Ibid*

$$I_{AB} = \sum_{ij} P_{ij} \ln \frac{P_{ij}}{m_i n_j} = \sum_{ij} \langle A_i B_j \rangle_{\psi^S} \ln \frac{\langle A_i B_j \rangle_{\psi^S}}{m_i n_j}$$

> where m_i and n_j are the multiplicities of the eigenvalues λ_i and μ_j. The marginal information quantities are given by:
>
> $$I_A = \sum_i \langle A_i I^2 \rangle_{\psi^S} \ln \frac{\langle A_i I^2 \rangle_{\psi^S}}{m_i}$$
>
> $$I_B = \sum_j \langle I^1 B_j \rangle_{\psi^S} \ln \frac{\langle I^1 B_j \rangle_{\psi^S}}{n_j}$$
>
> and finally the correlation, $\{A_i B_j\}_{\psi^S}$ is given by:
>
> $$\{A_i B_j\}_{\psi^S} = \sum_{ij} P_{ij} \ln \frac{P_{ij}}{P_i P_j} = \sum_{ij} \langle A_i B_j \rangle_{\psi^S} \ln \frac{\langle A_i B_j \rangle_{\psi^S}}{\langle A_i I \rangle_{\psi^S} \langle I B_j \rangle_{\psi^S}}$$
>
> where we note that the expression does not involve the multiplicities, as do the information expressions, a circumstance which simply reflects the independence of correlation on any information measure. These expressions of course generalize trivially to distributions over more than two variables (composite systems of more than two subsystems)."[75]

Everett believed that his information theoretic interpretation of the wave function had other advantages, including but not limited to: a description of the "sharpness" of the distribution; independence from the coordinate system; and a conjecture about the uncertainty relation. He writes:

> "The information is essentially a measure of the sharpness of a probability distribution, that is, an inverse measure of its "spread." In this respect information plays a role similar to that of variance. However, it has a number of properties which make it a superior measure of the "sharpness" than the variance, not the least of which is the fact that it can be defined for distributions over arbitrary sets, while variance is defined only for distributions over real numbers."[76]

Everett proves a theorem that correlation information is preserved under functional translation. [77] Everett points out how this fact might be useful in the formulation of a relativistic quantum theory: "the position correlation is independent of the coordinate system, even if different coordinate systems are used for each particle!"[78]

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[75] *Ibid* p. 104-105

[76] *Ibid* p. 82

[77] *Ibid* p. 89

[78] *Ibid* p. 88-89

With regards to the uncertainty relation, Everett conjectured an information formulation of the principle and believed that it was stronger than the standard form stated in terms of variances insofar as the standard form is implied by the information form but the inverse is not true:

$$I_x + I_k \leqq \ln(1/\pi\,\mathrm{e}) \quad \text{for all } \Psi(x)$$[79]

Most importantly perhaps, his information interpretation provided a nice definition of measurement without any of the extraneous assumptions of the orthodox theory.

Everett's Definition of Measurement in Information Theoretic Terms

For Everett, measurement is not a privileged process:

"We now consider the question of measurement in quantum mechanics, which we desire to treat as a natural process within the theory of pure wave mechanics. 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."[80]

The question of exactly what defined a "good measurement" was the subject of many treatises on quantum mechanics. Everett found that his correlation and information theoretic interpretation provided a natural and precise definition:

> "An interaction *H* is a measurement of *A* in S_1 by *B* in S_2 if *H* does not destroy the marginal information of *A* (equivalently: if *H* does not disturb the eigenstates of *A* in the above sense) and if furthermore the correlation {*A*, *B*} increases toward its maximum with time."[81]

This simple definition does more work than first might meet the eye. The correlation {*A*,*B*} does the work of the projection postulate without resorting to a dichotomy in dynamics, *i.e.* collapse on measurement. The preservation of the marginal information of *A* provides the stability of experimental records on repeated measurement. Taken as a whole, this definition expresses the content of the eigenvector-eigenvalue link. Everett argues that his machinery of interpretation also provides a natural way to describe approximate measurements.[82] All of these nice properties fall out from an information theoretic interpretation; whereas the probabilistic interpretation cannot adequately explain or define such without additional axioms and assumptions. There are no dichotomous dynamics, no mysterious collapses, only wave mechanics:

> "The discontinuous "jump" into an eigenstate is thus only a relative proposition, dependent upon our decomposition of the total wave function into the superposition, and relative to a

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[79] *Ibid* p. 110

[80] *Ibid* p. 111 Interestingly, Everett adds "Nearly every interaction between systems produces some correlation however… We could, then, take the position that the two interacting systems are continually "measuring" one another, if we wished." In this author's view, Everett's description here, his view of the world as quantum mechanical and his information theoretic interpretation of the wave function suggest that he might have found the decoherence arguments that followed many years later to be congenial.

[81] *Ibid* p. 113

[82] *Ibid* p. 145 Everett devotes a subsection entitled "Approximate Measurement" to showing how his information theoretic interpretation is more explanatory than the probabilistic interpretation in this regard.

> particularly chosen apparatus value. So far as the complete theory is concerned all elements of the superposition exist simultaneously, and the entire process is quite continuous."[83]

Everett's Measure for a Typical Observer or Recovering the Born Rule in Wave Mechanics without Probabilities

Having defined the relative states of subsystems in the superposition, correlations between them in an information theoretic sense, it remained for Everett to show that a squared amplitude measure was the natural choice of a measure for his wave mechanical model so that it recovered the predictive power of the textbook theory. Everett writes:

> "In order to make quantitative statements about the relative frequencies of the different possible results of observation which are recorded in the memory of a typical observer we must have a method of selecting a typical observer.
>
> Let us therefore consider the search for a general scheme for assigning a measure to the elements of a superposition of orthogonal states $\sum a_{i\phi_i}$. We require then a positive function of the complex coefficients of the elements of the superposition, so that M(a_i) shall be the measure assigned to the element φ_i. In order that this general scheme shall be unambiguous we must first require that the states themselves always be normalized, so that we can distinguish the coefficients from the states. However, we can still only determine the *coefficients*, in distinction to the states, up to an arbitrary phase factor, and hence the function M must be a function of the amplitudes of the coefficients alone, (i.e., M(a_i) = M ($\sqrt{a_i^{\phi} a_i}$), in order to avoid ambiguities.
>
> If we now impose the additivity requirement that if we regard a subset of the superposition, say $\sum_{i=1}^{n} a_i \, \phi_i$, as a single element αϕ'… then the measure assigned to ϕ' shall be the sum of the measures assigned to the ϕ_i(ί from 1 to n):
>
> $$M(\alpha) = \sum_i M(\alpha_i) \,,$$
>
> then we have already restricted the choice of M to the square amplitude alone. (M(a_i) = $a_i^{\phi} a_i$, apart from a multiplicative constant.)"[84]

Everett's additivity requirement (elsewhere he calls it a consistency criterion) allows conservation of probability as the superposition evolves in time:

> "(F)or us a trajectory is constantly branching (transforming from state to superposition) with each successive measurement. To have a requirement analogous to the "conservation of probability" in the classical case, we demand that the measure assigned to a trajectory at one time shall equal the sum of the measures of its separate branches at a later time. This is

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[83] *Ibid* p. 116

[84] *Ibid* p. 129-130

> precisely the additivity requirement which we imposed and which leads uniquely to the choice of square-amplitude measure."[85]

As he has shown, Everett may freely interpret the normalized measure on the sum of the square amplitudes for defining correlation and information. The appearance of probabilities is just that for Everett, both subjectively and theoretically. In his interpretation, probability is not prior to information in the wave function but the subjective experience of a subsystem of the wave function, an observer. Probability is *a posteriori*.

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[85] *Ibid* p. 131. At the Conference on the Foundations of Quantum Mechanics at Xavier University in 1962, Everett is recorded as saying: "What I need, therefore, is a measure that I can put on a sum of orthogonal states. There is one consistency criteria which would be required for such a thing. Since my states are constantly branching, I must insist that the measure on a state originally is equal to the sum of the measures on the separate branches after a branching process. Now this consistency criterion can be shown to lead directly to the squared amplitude of the coefficient, as the unique measure which satisfies this. With this unique measure, deduced only from a consistency condition, I then can assert: indeed, for almost all (in the measure theoretical sense) elements of a very large superposition, the predictions of ordinary quantum mechanics hold." p. 284

Section 4: Misunderstanding Everett and his Theory

Famously, Richard Feynman said that no one really understands quantum mechanics. As Everett alluded to in his letter to Jammer years later, it is certainly true that his theory was met with almost perfect misunderstanding when "Wave Mechanics without Probabilities" was first circulated by Professor Wheeler to a select group of leading physicists. The disciples of Bohr at Copenhagen would not bring themselves to admit that there were any such faults as Everett described in their own interpretation. Some others simply did not read the thesis completely or carefully. More objected on aesthetic or philosophical grounds. Few prominent critics ever engaged with Everett's arguments on his own terms.

The correspondence between Wheeler and Everett and others reveals how difficult it is to communicate a theory, especially a theory which proposes a fundamental change in a domain of knowledge, even between experts in the discipline. From his letters and his marginal comments on the letters of others, the reader has a keen sense of Everett's frustration at being misunderstood and misconstrued in this period. Nonetheless, it is clear that Everett remained confident in his theory, despite what he saw as misinformed criticism and philosophical or aesthetic objections from prominent and influential figures in his field.

There is a popular narrative that the controversy over his thesis prevented Everett from pursuing a career in academia, but the record tells a different story. While the opposition of Bohr and others was certainly not helpful to those prospects and must have been discouraging, Wheeler remained optimistic and appears to have hoped and believed with some evidence that Everett might have had a productive career in academia. Everett was a psychologically complex and complicated figure, and it is difficult to say precisely why he chose not to pursue an academic career. However, we can confidently assert that his abilities, interests and dispositions were well suited for the career in government and consulting that he chose. In any case, it appears that he did have a choice, and he chose not to follow the path his thesis advisor laid out for him into academia but in the footsteps of his military minded father instead.

It is also often said that the opposition of Bohr and his disciples in Copenhagen is the reason why Everett was compelled to rewrite his thesis, but here the record tells a more nuanced story. Certainly, the objection of Bohr, who held sway over many in the physics community, including Wheeler, was important. However, that narrative underestimates the significance and timing of Richard Feynman's public objection to the concept of many worlds at the conference on "The Role of Gravity in Physics" at Chapell Hill in January of 1957, where Wheeler and Charles Misner presented some of Everett's ideas. And it overlooks the fact once Wheeler had the revision he wanted from Everett, he was able to publish the Short Thesis. The revised thesis reflects Wheeler's interest in Everett's theory as a means to further his quantum gravity research program, more than it is true to Everett's original vision and aim, the dissolution of the measurement problem in quantum mechanics.

Confronting the Copenhagen Orthodoxy

At the outset, Wheeler was clearly impressed by Everett's theory and eager to win over his colleagues and his mentor Niels Bohr. He endeavored to do so during his tenure as visiting professor at University of Leiden in the Spring of 1956.[86] For Wheeler, the importance of Bohr's imprimatur cannot be overstated. Many years later, Bryce DeWitt tells Kenneth Ford in an interview that Bohr was a "hero" to Wheeler.[87] As Oshnaghi, Freitas and Freire recount in their paper "The Origin of the Everettian Heresy", Bohr loomed large over the physics community in this period:

> "The reason why the standard view of quantum mechanics was commonly attributed to Bohr (and indeed termed the Copenhagen interpretation) is undoubtedly related to Bohr's intellectual charisma and to his role in the construction of quantum mechanics. Bohr's personal influence upon his colleagues is legendary and has been exhaustively analysed by Chevalley (1997). Beller has described Bohr as a ''charismatic leader'': "As the founder of the philosophy of complementarity, Bohr was declared by his followers to be not merely a great philosopher, but a person of exceptional — perhaps superhuman— wisdom, both in science and in life.'' Thus, for example, in a recollection of the 1980s, Wheeler, compared Bohr's wisdom with that of Confucius and Buddha, Jesus and Pericles, Erasmus and Lincoln."[88]

Wheeler wrote to Everett that he hoped to have Everett's thesis published by the Royal Danish Academy,[89] but in that same letter, Wheeler writes:

> "After my arrival the three of us (Wheeler, Bohr and Petersen) had three long and strong discussions about it. I will send you separately notes about specific points. Stating conclusions briefly, your beautiful wave function formalism of course remains unshaken; but all of us feel that the real issue is the words that are to be attached to the quantities of the formalism. We feel that complete misinterpretation of what physics is about will result unless the words that go with the formalism are drastically revised." [90]

Wheeler urged Everett to come to Copenhagen himself and defend his thesis in front of Bohr, stating that his final approval depended on it:

> "You ought not to go of course except when he signifies to you that you are picking a time when he can spend a lot of time with you. Unless and until you have fought out the issues of interpretation one by one with Bohr, I won't feel happy about the conclusions to be drawn from a piece of work as far reaching as yours. Please go (and see me too each way if you can!)
>
> So in one way your thesis is all done; in another way, the hardest part of the work is just beginning."[91]

To say this was an unusual graduation requirement is a gross understatement.

Wheeler's notes from those "long and strong discussions" would have conveyed to Everett just what an uphill battle lay in front of him. Bohr and Petersen's response to Everett's thesis was to give Wheeler a

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[86] *Ibid* p. 205

[87] Ford

[88] Osnaghi, Freitas, and Freire Jr. p. 97-123.

[89] Barrett, Byrne and Weatherall "Wheeler to Shenstone 28-May-1956." In contemporary correspondence with Prof. A.G. Shenstone, Wheeler explains that publication of Everett's thesis "in the Danish Academy would be the best public proof of having passed the necessary tests", *i.e.* the test of Bohr's approval.

[90] Barret and Byrne p. 206

[91] *Ibid*

remedial lecture on Bohr's philosophy of complementarity and the finer points of their own theory of quantum mechanics.

Wheeler reports that Petersen and Bohr reject Everett's premise of treating observers, or even observational apparatus, quantum mechanically.[92] They say that quantum mechanics is not what Everett "thinks it is about".[93] Where Everett imagines a universal wave function, Bohr and Petersen would have the domain of quantum mechanics limited to the microscopic.[94] Hence, Petersen and Bohr admit no measurement problem at all in their interpretation because the observer and apparatus are described classically. Further, in their view of the complementarity of quantum mechanics, the wave function is meaningless without that classical description.[95] In the margin next to Wheeler's note to this effect, Everett writes: "Nonsense!"[96] Petersen asserted that Everett "talks of correlation but can never build that up by ψ" functions.[97] Everett jots in the margin: "obviously hasn't completed reading of thesis! it does just that."[98]

Just a few days later, Wheeler received comments on Everett's thesis from another member of the faculty at Copenhagen Alexander Stern, who had given a seminar on the Long Thesis there with Bohr and others in attendance. That seminar could not have been what either Wheeler or Everett would have hoped for judging from Stern's letter. Like Bohr and Petersen, Stern writes to Wheeler that a "universal wave function" "lacks meaningful content". Stern objects to the description and role of the observer in the theory. He writes that Everett "employs the concept of observer to mean different things at different times— the measuring apparatus, a servo-mechanism for registering experimental results, and its dictionary meaning, that is, common usage."[99] He continues, "I do not follow him when he claims that, according to his theory, one can view the accepted probabilistic interpretation of quantum theory as representing the subjective appearances of observers. At times he gives one the impression that he believes that, were it not for the interference of physicists (observers) quantum theory would be a continuous, deterministic, and elegant theory."[100] Of course, Everett did believe that quantum mechanics was a continuous, deterministic, and elegant theory, including observers, a concept he defined broadly and without any special physical privilege, as we have seen and as indicated by Everett's marginal notes.[101] From this litany and the remainder of his remarks, it is evident that Stern read the Long Thesis, or however much of it he did read, through the lens of Bohr's philosophy of complementarity and the Copenhagen interpretation.

Stern continues, emphatically denouncing Everett's model of measurement:

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[92] *Ibid* p. 207

[93] *Ibid* p. 208

[94] *Ibid*

[95] *Ibid* p. 210. Wheeler notes that Bohr and Petersen criticize von Neumann and Wigner on the same grounds: "Says Von N & Wig all nonsense; their stuff beside the point; doesn't come into; could overlook test body atomicity— Von N & Wig— mess up by including meas tool in system".

[96] *Ibid* p. 213

[97] *Ibid* p. 209

[98] *Ibid* p. 213

[99] *Ibid* p. 215

[100] *Ibid*

[101] *Ibid* p. 223

> “(T)o my mind, the basic shortcoming in his method of approach of his erudite, but inconclusive and indefinite paper is his lack of an adequate understanding of the measuring process. Everett does not seem to appreciate the FUNDAMENTALLY irreversible character and the FINALITY of a macroscopic measurement. One cannot follow through, nor can one trace the interaction between the apparatus and the atomic system under observation. It is not an “uncontrollable interaction”, a phrase often used in the literature. Rather, it is an INDEFINABLE interaction.”[102]

It is noteworthy that Everett never uses the adjective “uncontrollable” to describe interactions, but Bohm does very often.[103] It may have been that Everett’s theory which is causal and deterministic reminded Stern and others at Copenhagen of Bohm’s theory. The followers of Bohr had been batting down Bohm’s ideas for several years, and they might have seen Everett’s theory as yet another challenge arising from the other side of the Atlantic, from the very same university where Bohm had been an assistant professor and old academic adversaries such as Einstein and von Neumann had been ensconced.[104] In any event, Stern might have been less doctrinaire and more charitable on this point, as Bohr himself uses the phrase “uncontrollable interaction” in his famous rebuttal to the EPR paper.[105]

As to reversibility and irreversibility, Everett does discuss the topic at some length, devoting a section to it.[106] He concludes from the mathematical formalism that quantum mechanics is reversible in principle. The apparent irreversibility is subjective in the same sense that the appearance of probabilistic outcomes is. Everett’s exposition on this subject is toward the end of the Long Thesis. His marginal notes indicate that he believed that Stern either did not read the section on reversibility or did not understand it.[107] In any case, Stern did not address Everett’s argument.

Stern digresses through a tour of the virtues of the philosophy of complementarity as it might apply to biology, an example Bohr had often written and spoken about[108], before returning to his criticism of the Long Thesis:

> “The unobservables in a theory should have observable consequences. The unobservables and the observables together form the theoretical structure, and they must be logically connected. If Everett’s universal wave equation demands a universal observer, an idealized observer, then this becomes a matter of theology. If a complete knowledge of the state of the composite system (apparatus plus atomic subsystems) involves practically an infinite number of observers

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[102] *Ibid* p. 215-216

[103] Bohm uses the adjective “uncontrollable” on 48 separate occasions to describe interactions in *Quantum Theory*.

[104] For the controversy between the Copenhagen school, Bohm and Everett from a sociological perspective, see Freire Jr, Olival. "Science and exile: David Bohm, the cold war, and a new interpretation of quantum mechanics." Historical Studies in the Physical and Biological Sciences 36, no. 1 (2005): 1-34.

[105] Bohr, Niels. 1935

[106] Barrett and Byrne. See Chapter V Supplementary Topics, Section 3 Reversibility and Irreversibility in the Long Thesis.

[107] *Ibid* p. 224

[108] For contemporary examples of how Bohr thought his philosophy of complementarity might apply to biology and other sciences, see Bohr, Niels**.** *Atomic Physics and Human Knowledge*. Courier Dover Publications, 2010. First published in 1957, *Atomic Physics and Human Knowledge* contains Bohr’s essays on this subject up to the time of Stern’s writing. Bohr’s influence on Stern is apparent.

> which cannot communicate with one another, then we are talking metaphysics. One may invoke the image of a large number of mystics in different "resonant" states."[109]

Everett made no notes here, but his theory requires no such thing as a "universal observer". Perhaps Stern is imposing a Copenhagen interpretation on top of Everett's theory, and as such, the universal wave function would require some universal or ideal observer to give it meaning. This would be a failure to address Everett's argument in its own terms.

Everett's claim regarding "the state of the composite system" is only that it is modeled by process 2, the mathematical formalism of quantum mechanics. "Complete knowledge" does not enter into it. On the contrary, Everett states that it is his belief that the whole of experience is not ever epistemically accessible in his appendix on the role of theory.[110] This belief is consistent with the concept of correlation in his theory which entails that observers can only directly experience systems to which they are correlated. For Everett, incomplete knowledge is the only knowledge we have and sufficient empirical support for a theory. If Stern is suggesting that any theory which relies on incomplete knowledge or indirect evidence and inference therefrom is not a physical theory, strictly speaking, then the Copenhagen interpretation of quantum mechanics is not a physical theory either. Nor would many other generally accepted physical theories meet that test. If Stern is objecting to a theory which contains entities that are unobservable in principle, Everett argues that re-interference between any elements in the superposition is always possible, notwithstanding the practical difficulties of re-interfering branches.[111]

However, from his last remark about resonating "mystics", one might infer that Stern's strongest objection is to the superposition of observer subsystems modeled by process 2. That argument is not metaphysical. It follows from treating observers as physical systems and the linearity of the mathematical formalism in the theory. As Everett acknowledged in a mini paper prior to writing the Long Thesis, "The price, however, is the abandonment of the concept of the uniqueness of the observer, with its somewhat disconcerting philosophical implications."[112]

Stern suggests that Everett both somehow assumes his conclusion in his model or premises and also would have discovered a self contradiction if he had only elaborated further:

> "I cannot help believing that, if Everett went further and carried out his mathematical ideas, forgetting his preconceived model of the universe, which guided, channeled, and concluded his mathematical investigation, he would have come across a contradiction in his work. His claim that process 1 and process 2 are inconsistent when one treats the apparatus system and the atomic object system under observation as a single composite system and if one allows for more than one observer is, to my mind, not tenable."[113]

Here Stern appears to be reversing Everett's priorities. It is difficult to see how one could avoid modelling the universe as a wave function once one assumes that process 2 is the correct mathematical formalism for describing the observable properties of all things at all times. Stern does not offer any

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[109] Barrett and Byrne p. 217
[110] *Ibid* p. 172
[111] *Ibid* p. 149
[112] *Ibid* p. 70
[113] *Ibid* p. 217

argument as to why Everett's claim regarding the inconsistency of process 1 and process 2 with multiple observers is untenable. Why he chose not to do so, one cannot say; but this is not favorable terrain to fight Everett on.

Without any sense of irony after having accused Everett of theological and metaphysical speculation, Stern concludes his critique with a recitation of the philosophical axioms of complementarity complete with religious allusions and metaphysical assertions:

> "In the pursuit of truth the concepts of objectivity and subjectivity are no longer antithetical, but take on a complementarity aspect. Our formalism must be in terms of possible or idealized experiments whose interpretations thereby involve the use of concepts intimately connected with our own sphere of experience which we choose to call reality. The epistemological nature of our experiments and the objective nature of the abstract mathematical formalism TOGETHER form the body and spirit of science."[114]

Wheeler's political maneuvering with the Copenhagen Orthodoxy and growing impatience with Everett

Wheeler's reply to Stern, which he copies to Everett, reveals the difficult position Wheeler found himself in. Charitably, we can read this letter as an attempt to bridge the divide between Everett, a promising, favored student with revolutionary ideas and Bohr, his accomplished, revered mentor, who had famously brought about a revolution in physics generations earlier. Wheeler writes:

> "I would not have imposed upon my friends the burden of analyzing Everett's ideas, nor given so much time to past discussions of these ideas myself, if I did not feel that the concept of "universal wave function" offers an illuminating and satisfactory way to present the content of quantum theory. I do not in any way question the self consistency and correctness of the present quantum mechanical formalism when I say this. On the contrary, I have vigorously supported and expect to support in the future the current and inescapable approach to the measurement problem. To be sure, Everett may have felt some questions on this point in the past, but I do not. Moreover, I think I may say that this very fine and able and independently thinking young man has gradually come to accept the present approach to the measurement problem as correct and self consistent, despite a few traces that remain in the present thesis, draft of a past dubious attitude. So, to avoid any possible misunderstanding, let me say that Everett's thesis is not meant to question the present approach to the measurement problem, but to accept it and generalize it."[115]

Regardless of Wheeler's intentions, his assertion that Everett had come around to the Copenhagen interpretation of measurement is simply false, a fact to which Everett's marginal notes and later correspondence with Petersen attests.[116] To be fair to Wheeler, he may have truly hoped for such a

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[114] *Ibid* p. 218

[115] *Ibid* p. 219

[116] *Ibid* p. 236-240. See the Petersen Everett correspondence post publication of the Short Thesis in April and May of 1957 for an example of his continuing contempt for the Copenhagen interpretation. *E.g.*, "(t)he Bohr interpretation is to me even more unsatisfactory, and on quite different grounds. Primarily my main objections are the complete reliance on classical physics from the outset (which precludes even in principle any deduction at all of classical physics from quantum mechanics, as well as any adequate study of measuring processes), and the

reconciliation and believed that Everett's theory was a generalization of Bohr's concept of the quantum as indefinite and individual. Wheeler does push back hard against Stern and the other Copenhagen adherents' insistence on interpreting Everett's theory in terms of complementarity instead of evaluating the Long Thesis on its own terms:

> "(a) Nothing prevents one from *considering* a wave function and its time evolution in abstracto; that is, without ever talking about the equipment which originally prepared the system in that state, or even mentioning the many alternative pieces of apparatus that might be used to study that state. (b) A state function as used in this sense has absolutely nothing to do with the state function as used in the customary discussion of the measurement problem, for now *no means of external observation are admitted to the discussion*. (c) Why in the world talk of a wave function under such conditions, for it in no way measures up to the role of the wave function in the customary formulation, that we accept without question? (d) Because it is proposed, of Everett's free volition, to formulate *a new physical theory*, as a step of free creation. (e) In this theory, as in every new theory, the quantities that enter have roles and positions that will be defined and determined *by the logical structure of the theory itself*."[117]

Wheeler continues in the same pattern, alternating between politic concessions to Everett's Copenhagen critics and vigorous assertions of the potential utility, logical consistency and scientific legitimacy of Everett's theory. Wheeler concedes that the terms "'correlation' and 'observer' ought to bear quite different names, to emphasize the absolutely fundamental distinction between the model universe and the real physical world."[118] However, in the very next sentence, Wheeler claims that Everett's theory passes "the test of logic, by its internal self consistency" in that the model treats all systems wave mechanically.[119] Wheeler asserts that "Everett traces out a correspondence between the 'correlations' in his model universe on the one hand, and on the other hand what we observe when we go about making measurements"; then he concedes that, although Everett has "in large measure" demonstrated that correspondence, in Wheeler's opinion, "there are still logical loopholes left."[120] Wheeler concludes by assuring Stern that he believes Everett can "fill in the missing steps … *if* he can have the benefit of some weeks in Copenhagen to struggle out these problems."[121] With these last remarks, he casts Everett in the role of pilgrim seeking the wisdom of Bohr and his disciples at the Mecca of physics. Everett would not have characterized himself or Copenhagen that way, but Wheeler may have sensed that approaching as a supplicant was the only way for Everett to advance his heterodox theory.

Wheeler appears to be working hard to pour oil on troubled waters, putting his considerable reputation, influence and personal relationships on the line. His short letter to Everett enclosing his reply to Stern is by turns solicitous, demanding and encouraging.[122] Wheeler asks if Everett agrees with his reply and if he writes himself, would he copy Wheeler, Bohr and Petersen. Then Wheeler signals again that his thesis

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strange duality of adhering to a "reality" concept for macroscopic physics and denying the same for the microcosm."

[117] *Ibid* p. 220-221

[118] *Ibid* p. 222

[119] *Ibid*

[120] *Ibid*

[121] *Ibid*

[122] *Ibid* p. 223 - 224

will require revision before he gives it his endorsement.[123] In the encouraging vein, Wheeler compares Bohm's theory and Everett's: "As I have said before, I feel that your work is most interesting and am sure that it will receive discussion of a scope comparable to what has attended Bohm's publications."[124] Everett may have not found Bohm's example as heartening as Wheeler intended, as by this time Bohm had been unceremoniously let go from Princeton and was living in exile.

Wheeler writes to Everett that his European colleagues regard him as Everett's "promoter".[125] One has the sense that Wheeler feels that he is doing more to defend Everett's theory than Everett at this point, which is true, but only because Wheeler is insisting on obtaining Bohr's approval. He writes to Everett, "(T)his long distance writing I for one find a very inefficient way of using my time to forward your work; discussion would be much more to the point. Hence my great hope that you will arrange to come."[126]

Intransigence on either side of the Atlantic; Everett moves on.

Despite Wheeler's efforts to bring the opposing sides together, they would only move further apart. Shortly after the Wheeler Stern correspondence concluded, Petersen gratuitously returned Everett's thesis to Wheeler by mail with a note indicating that Bohr would write to him soon "regarding the epistemological situation in quantum physics and especially about the status of observers in the complementary mode of description."[127] By returning the thesis, the Copenhagen disciples are clearly signaling that they do not believe Everett's theory merited more study. Bohr never did get around to writing that note, but Byrne remarks that Petersen and Bohr appear to be more concerned with bringing Wheeler back into the fold than exploring an alternative theory of quantum mechanics with his graduate student.[128]

At the end of May, Wheeler prevailed upon Everett to make the trans-Atlantic journey to defend his thesis in Copenhagen, but Everett required that he must return by June 15 to begin work at the Weapon Systems Evaluation Group (WSEG) at the US Department of Defense where he had already accepted a position.[129] This position at the Pentagon had been offered to Everett on the condition that he would receive his doctorate degree, putting Everett at the mercy of Wheeler.[130] Petersen rejected this proposal, writing to Wheeler that a "longer stay is desirable" and to Everett that he should try to come in the Fall when Bohr would have more time for him but before then he ought to write up a "thorough treatment of the attitude behind the complementary mode of description" that "as clearly as possible stated the points where you think this approach is insufficient."[131]

The delay and additional requirements could not have been welcome news to Everett, who is literally moving on at this point. Moreover, by asking Everett to address the philosophy of complementarity and the Copenhagen interpretation, Petersen is shifting the debate away from Everett's theory to Bohr's

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[123] *Ibid* p. 223
[124] *Ibid*
[125] *Ibid*
[126] *Ibid*
[127] Byrne p. 167
[128] *Ibid* p. 168 - 169
[129] *Ibid*
[130] *Ibid* p. 187
[131] *Ibid* p. 168 and Barrett, Byrne and Weatherall "Aage Petersen telegram to Hugh Everett, 28-May-1956."

theory, where his side enjoys the advantage of authorship. Everett's reply to his friend is polite, but he is careful not to allow Petersen to change the subject. He also takes a not so subtle jab at Stern for his superficial reading of the thesis and Petersen for returning it:

> "Thank you for your letter of May 28. I am sorry to be so remiss in replying, but I have been very busy moving from Princeton.
>
> I would like very much to come to Copenhagen in the fall, if I can arrange for a leave of absence then. I am looking forward to renewing our very enjoyable arguments.
>
> I am enclosing a copy of my work, so that you will have sufficient time to become thoroughly acquainted with it. In your letter you asked me to give a treatment of the attitude behind the complementary mode of description, and the points where I object. I have not done this yet, but while I am doing it you might do the same for my work. Judging from Stearn's *[sic]* letter to Wheeler, which was forwarded to me, there has not been a copy in Copenhagen long enough for anyone to have read it thoroughly, a situation which this copy may rectify. I believe that a number of misunderstandings will evaporate when it has been read more carefully (say 2 or 3 times)."[132]

One can only marvel at Everett's confidence. The young PhD candidate's reply to the established Copenhagen physicists who are telling him to brush up on Bohr's theory and philosophy is that it is they who need to study his thesis more carefully. The prospective Fall meeting did not occur. Everett would visit Copenhagen years later in 1959 and meet with Bohr along with his Princeton roommate and another advisee of Wheeler's Charles Misner, but the elderly Bohr would not be drawn into a discussion of Everett's theory but instead recounted his famous battles with Einstein.[133]

Despite Wheeler's persistent efforts, the debate with the Copenhagen orthodoxy, such as it was, was practically over before it began. Bohr and his colleagues were simply not receptive to an alternative theory. They never engaged with Everett's thesis in its own terms. From their point of view, there was no need. One can see how they might have been dismissive of Everett's theory. His thesis was aimed at solving the measurement problem, but there was no measurement problem in their theory as far as they were concerned. And Bohr had faced down challenges from Einstein before Everett was born. What did he have to learn from Wheeler's graduate student?

Wheeler versus Everett

With Everett at WSEG and his visit to Copenhagen deferred, the extraordinary activity and correspondence around his draft thesis wound down. At this point, the interests of the thesis advisee and advisor are diverging. For Everett's part, his focus at this time appears to have been on his employment at the Defense Department, which would include important work on national security issues and fruitful, groundbreaking research in game theory and the new, related field of operations research. He needed Wheeler's sign off on his thesis to get his Ph.D., a requirement for keeping his new job. More, Wheeler was an important patron with broad influence in the science and national security establishment. For Wheeler's part, he was looking forward to a conference at the University of North

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[132] Barrett, Byrne and Weatherall "Hugh Everett letter to Aage Petersen, June-1956."

[133] Byrne p. 218 - 221

Carolina at Chapel Hill to be held in January of 1957 on "the Role of Gravitation in Physics" that he helped organize along with Bryce DeWitt and his wife Cecile DeWitt-Morette.[134] Wheeler hoped that the conference would breathe life into research in general relativity broadly and advance his own quantum gravity research program. He believed that Everett's theory might play an important part in that program, if he could get Everett to rewrite the thesis in such a way as to make it "javelin proof". [135]

Wheeler versus Everett: Everett's Objectives

Years later in September of 1973, Max Jammer wrote to Everett in the course of doing research for his book *The Philosophy of Quantum Mechanics*. Jammer asked Everett, "Was there any specific motive or reason that induced you to propose your interpretation and measurement theory?" Everett replied:

> "I must answer in all candor the primary motive was, of course, to obtain a thesis. However, I must also admit to a strong secondary motive to resolve what appeared to me to be inherent inconsistencies in the conventional interpretation. I was of course struck, as many before and also many since, by the apparent paradoxes raised by the unique role assumed by the measurement process in quantum mechanics as it was conventionally espoused. It seemed to me unnatural that there should be a "magic" process in which something quite drastic occurred (collapse of the wave function), while in all other times systems were assumed to obey perfectly natural continuous laws."[136]

Everett's description of his "primary motive" might appear at first to be false modesty or a deflection, but we must bear in mind that, at the time of writing, the Long Thesis was still unpublished and Everett is referring to the Short Thesis. If we look at Everett's personal history, in particular his activities and circumstances contemporaneous to his thesis writing and its publication in 1957, his primary motive is more understandable in that context. Everett was being as candid as he claimed: the young husband and new father submitted the redacted thesis because he needed to fulfill his Ph.D. requirements to keep his job in the nation's capital. His work at WSEG was interesting, important and paid handsomely, especially compared to the salary of a junior academic.[137] As to his secondary motive, Everett's intellectual predispositions to puzzle solving and games with rules, his love of paradoxes and sense of the absurd certainly informed his attitude toward the measurement problem. The apparent paradox at the heart of quantum mechanics, a paradox that had humbled his heroes, must have tickled his fancy. Notably, Everett does not mention quantum gravity in his reply to Jammer.

Hugh Everett III, Washingtonian

Hugh Everett III was born, raised and lived almost his entire life in the nation's capital Washington, D.C. and its suburbs in Maryland and Virginia. [138] Peter Byrne's biography of Everett describes him as a

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[134] *Ibid* p. 179

[135] *Ibid* p. 211

[136] *Ibid* p. 296

[137] *Ibid* p. 187

[138] *Ibid* p. 25-41. With the exceptions of one year off from his undergraduate studies at Catholic University when he lived with his parents when his father was stationed in Germany and his years as a graduate student at Princeton, Everett was a Washingtonian.

precocious boy, interested in logic, puzzles, paradoxes, science and technology from an early age.[139] His father Hugh Everett II earned a bachelors in Civil Engineering, masters in Patent Law and a doctorate in Juridical Science, held a commission in the National Guard and worked as a contract employee or consultant for government agencies and the military before the outbreak of World War II in which he served as a staff officer in the US Army.[140] He would remain in the Army on active duty after the war, retiring a colonel. His mother Katherine Lucille Kennedy Everett earned a masters in Education and wrote fiction and poetry for magazines.[141] They separated when Everett was five.[142]

For almost all of his childhood, Everett lived with his father and stepmother.[143] He was educated at St. John's, a Catholic military preparatory school in Washington, D.C.[144] At the age of 12, Everett wrote to Einstein with a self-described solution to the paradox of what happens when an irresistible force meets an immovable object.[145] Einstein favored young Everett with a reply:

> "There is no such thing like an irresistible force and immovable body. But there seems to be a very stubborn boy who has forced his way victoriously through strange difficulties created by himself for this purpose."[146]

His friends at St. John's recalled that Everett devised his own method for calculating odds and implemented it at a nearby horse track but abandoned it when he found that his predictions were the same as the racing form.[147]

Everett matriculated at Catholic University, also in the capital, where he studied Chemical Engineering.[148] He excelled in mathematics, physics and a philosophy of science course.[149] Notably, he took a course in game theory at nearby American University taught by a visiting Princeton professor Albert Tucker.[150] He graduated *magna cum laude* in 1953.[151] That summer, he worked in a laboratory in Silver Spring, Maryland run by Johns Hopkins University, analyzing the performance of servomechanisms that corrected for errors such as bomb sights.[152] The position required a security clearance.[153] While Everett was there, the lab, working with the US Army's Operations Research Office, conducted the first computer simulation in the history of operations research, a study of North American air defenses.[154]

Everett applied to Princeton University with a 99th percentile score in the advanced physics section of the Graduate Record Examination and glowing recommendations from his mathematics and physics

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[139] *Ibid*
[140] *Ibid* p. 10-16
[141] *Ibid*
[142] *Ibid*
[143] *Ibid* p. 15
[144] *Ibid* p. 26
[145] *Ibid*
[146] *Ibid*
[147] *Ibid* p. 30
[148] *Ibid* p. 36
[149] *Ibid* p. 31, 35-36
[150] *Ibid* p. 38
[151] *Ibid* p. 36
[152] *Ibid* p. 37
[153] *Ibid*
[154] *Ibid*

professors.[155] He was accepted to the mathematics department.[156] The National Science Foundation granted his tuition and a stipend on the condition that he work on game theory.[157]

Princeton University; Everett's other Seminal Paper: Recursive Games

Arriving at Princeton in the academic year 1954-1955, Everett pursued his interest in the same sort of puzzles, paradoxes and problems that had captured his imagination as a child and undergraduate. He attended game theory seminars and conferences organized by Harold Kuhn and Albert Tucker with Oskar Morgenstern, John von Neumann, John Forbes Nash, and Lloyd S. Shapley.[158] Game theory and the closely related field of operations research were seen as tools for rationally deciding on optimal solutions for problems in various fields from economics and political science to military strategy.[159] The US government was particularly interested in training young scientists in these skills during the Cold War era.

Everett presented a paper *Recursive Games*, in which he proposed a method for determining an optimal strategy, assuming one exists, for games with possibly infinite number of steps.[160] In that paper, Everett demonstrates the applicability of his method to military scenarios and likens his subject to combinatorial problems and the analysis of servomechanisms.[161] Kuhn considered *Recursive* Games a seminal work, and he remarks that it was worthy of a Ph.D. in mathematics itself.[162]

In his second year, Everett formally entered the Physics department. He studied mathematical physics with Eugene Wigner.[163] That year, Niels Bohr was in residence at Princeton's Institute for Advance Studies accompanied by Aage Petersen, who became fast friends with Everett and his roommate Charles Misner.[164] Drinking one night with Petersen and Misner, Everett pointed out what he saw as the absurdity of the description of measurement in quantum mechanics.[165] The paradox presented by the measurement problem proved irresistible to Everett. Dissolving it would be the subject of his doctoral thesis. In his third year, the registrar of Princeton records Everett's dissertation work under the title

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[155] *Ibid* p. 37-38

[156] Barrett and Byrne p. 299

[157] American Institute of Physics "Interview of Hugh Everett by Charles Misner on 1977 May."

[158] Byrne p. 56

[159] *Ibid*

[160] Kuhn p. 87-118. *Recursive Games* was written contemporaneously to the Long Thesis. There are striking similarities in the structure and substance of the arguments Everett employs in both papers. In particular, his method of assigning expectation values to a deterministically evolving set of branching futures and his argument that non-terminating games approach measure zero as the number of games played goes to infinity. A detailed analysis of those similarities is outside the scope of this paper.

[161] *Ibid* p 115

[162] Byrne p. 65

[163] *Ibid* p. 58, 81 Everett studied quantum mechanics with Robert Dicke, reading von Neumann's *Mathematical Foundations of Quantum Mechanics* and Bohm's *Quantum Theory* the previous academic year while nominally in the mathematics department.

[164] *Ibid* p. 89-90

[165] *Ibid* p. 90

"Correlation Interpretation of Quantum Mechanics", again reflecting the influence of Bohm's *Quantum Theory* in which the concept of correlation within the wave function plays an important role.[166]

Weapons Systems Evaluation Group; Everett's Theory of Nuclear War

But even as Everett was in the midst of working on his solution to the paradox at the heart of quantum mechanics, he was already making plans to return home to Washington, DC after graduation and work on national security issues in a position where he could put his training in game theory to use. In the Summer of 1956, Everett interviewed with the Weapons Systems Evaluation Group (WSEG) of the Institute for Defense Analyses, a think tank within the Department of Defense that reported to the Secretary of Defense and the Joint Chiefs of Staff.[167] WSEG was chartered to provide analysis of national defense issues employing "advanced techniques of scientific analysis and operations research… from an impartial, supra-Service perspective."[168] To this end, WSEG recruited the best and the brightest from America's elite universities.[169] Wheeler, like his colleagues von Neumann and Wigner, had been deeply involved in organizing and advising on the new Department of Defense's scientific and research programs and policies.[170] WSEG would be called on to analyze Soviet missile and bomber forces, evaluate NATO defenses in Europe, assess Russia's satellite launch capabilities, and game scenarios for chemical, biological and nuclear war to name only a few of their more important assignments in the late 1950's.[171] To carry out these tasks, the young scientists at WSEG had access to state of the art computers, Top Secret intelligence and enormous resources.[172] For Everett, with his interests in the physical sciences, game theory, information theory and computers and his familial attachments to Washington, DC and the military, it must have seemed like a dream job.

Late in 1956, as Wheeler prepared his arguments in favor of using Everett's theory to reconcile quantum mechanics with general relativity for the Chapel Hill conference and Bohr and his acolytes in Copenhagen studiously ignored their copy of the Long thesis, Everett was hard at work at WSEG on another physics problem of historic import; but this was not a theoretical paradox in the foundations of physics, it was a practical problem upon which the fate of nations and even the survival of the human species might depend. WSEG had been tasked with analyzing the effect of radioactive fallout from a nuclear exchange between the United States and the Soviet Union, and Everett and his colleague George Pugh had been chosen to construct the model.[173] Everett wrote to his new wife Nancy in December:

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[166] Shikhovtsev

[167] Byrne p. 187, 191-192

[168] Ponturo

[169] Byrne p. 192

[170] *Ibid* p. 128 These scientific and research programs were a high priority for the US government, as its Cold War defense strategy relied on maintaining a technological superiority over Soviet Russia, the Warsaw Pact and Communist China to offset their large numerical advantage in conventional ground forces and shorter lines of communication to prospective battlefields in Europe and Asia.

[171] *Ibid* p. 192-194 and Ponturo p. 129-180

[172] *Ibid*

[173] Byrne p. 202

> "Still haven't completed work for Wheeler, but hope to have something whomped up to talk to him about. The trouble is I have been working (at work) on a very interesting problem, and have been unable to resist working on mathematical parts at home."[174]

According to Pugh, Everett's friend and colleague at WSEG, the US Air Force had originally estimated casualties from a nuclear attack considering only the blast effects of detonating nuclear weapons on their targets, amounting to a few hundred thousand deaths resulting from a US attack on the Soviet Union.[175] These casualty estimates, while very large, were of the order of magnitude of casualties that resulted from conventional bombing campaigns in World War II.[176] With these estimates and the relatively small US nuclear arsenal in the first few years of the post war period, it was possible for military and civilian leaders to envision employing nuclear weapons in a general war, not as a last resort, but as an adjunct to conventional military operations, in much the same way that the strategic bombing of the Third Reich and the Empire of Japan had been carried out. And they planned to do just that. At the outbreak of the Korean War in June of 1950, the US Joint Chiefs of Staff approved a war plan for execution in the event that the conflict on the peninsula escalated into a general war against the Warsaw Pact and Communist China, calling for a "strategic air offensive with atomic and conventional bombs … initiated at the earliest possible date subsequent to the outbreak of hostilities."[177]

However, by the time that Everett arrived at the Department of Defense, the strategic situation was evolving rapidly. The US and USSR nuclear arsenals were growing by leaps and bounds.[178] And the casualty estimates of the Air Force had ignored the effects of radiation and radioactive fallout. In 1956, no one was certain what a large scale nuclear war campaign would look like. Everett's "very interesting problem" was to determine how many people would die in the event of World War III. The answer his model gave shocked military and civil authorities and overturned the orthodox thinking about war in the nuclear age.

What Everett and Pugh found was that when radiation effects were considered, casualties were ten times as many, a few millions, not a few hundreds of thousands.[179] And when radioactive fallout effects were included, casualties rose to the tens of millions.[180] Further, they found that much of the conventional thinking about the targeting of the warheads was based on false premises.[181] Radioactive fallout was what killed most, and fallout rose as a function of megatonnage detonated.[182] Finally, their study only contemplated the effects of a nuclear exchange to 60 days out and did not factor in what might be reasonably assumed to be the consequences for the effected populations:

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[174] *Ibid* p. 189
[175] *Ibid* p. 202-203
[176] See United States Strategic Bombing Survey. *The United States Strategic Bombing Survey: Summary Report (European War) September 30, 1945.* Vol. 1. US Government Printing Office, 1945 and United States Strategic Bombing Survey. *Summary Report (Pacific War) Washington, DC, 1 July 1946*. Vol. 1. US Government Printing Office, 1946 for official accounts of the US bombing campaigns in World War II.
[177] Kaplan p. 39
[178] For the growth of nuclear arsenals in this period, see Robert S. Norris and Hans M. Kristensen, "Global nuclear stockpiles, 1945-2006," Bulletin of the Atomic Scientists 62, no. 4 (July/August 2006), 64-66.
[179] Everett and Pugh
[180] *Ibid*
[181] Byrne p. 203-204
[182] Everett and Pugh

> "(I)t must be pointed out that the total casualties at 60 days may not be indicative of the ultimate casualties. Such delayed effects as the disorganization of society, disruption of communications, extinction of livestock, genetic damage, and the slow development of radiation poisoning from the ingestion of radioactive materials may significantly increase the ultimate toll."[183]

Everett and Pugh's conclusions were deemed important enough to merit a briefing for President Eisenhower, which Pugh gave in July 1957. Fortunately, Eisenhower was more receptive to Everett's ideas than Bohr. Pugh recalls:

> "After I had finished, Sherman Adams [Eisenhower's senior adviser] asked the president if he thought he had understood the presentation. The president responded, "Yes, it seemed quite clear. In some ways, the effects of radioactivity are like an artillery bombardment. It doesn't matter much where you aim, the important thing is the total fire power that's delivered.' I left with the feeling that we had successfully delivered our message to the president and his staff."[184]

Recognizing that the strategic balance between the US and USSR required that both sides have the same appreciation of the dangers of a nuclear exchange, a declassified version of Everett and Pugh's report was made public at hearings before the Special Sub-Committee on Radiation of the Joint Congressional Committee on Atomic Energy and published in the March 1959 issue of *Operations Research*.[185] Everett and Pugh wrote a prologue for this publication:

> "It is the hope of the authors… that the results here indicated will illustrate the catastrophic effects of a large nuclear campaign, regardless of specific targeting doctrine. Perhaps the public release of this information will serve to reduce the probability that such conflicts will ever occur."[186]

Everett and Pugh's model of nuclear war had an important impact on US and USSR Cold War strategy and informed the public discourse about the danger of nuclear weapons. Linus Pauling, winner of the Nobel Prize in Chemistry in 1954, credited Everett and Pugh in his 1962 Nobel Peace Prize lecture for alerting the world to the existential threat of nuclear war.[187] The reception of Everett's theory of nuclear war stands in sharp contrast to the reception of his theory of quantum mechanics. Whereas the physics establishment had almost uniformly misunderstood and rejected his unorthodox thesis, the national security establishment, and even its opponents like Pauling, embraced the work of the young scientific rebel.

When we look at Everett, his family situation and his important work on national security issues in the Winter of 1956 – 1957, it is easy to understand why he might have been distracted from working on a revision of his thesis that he did not think needed revision. In conversation with his friend and

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[183] *Ibid* p. 247

[184] Byrne p. 204

[185] *Ibid* p. 204-205

[186] *Ibid* p. 205

[187] *Ibid*

collaborator at WSEG George Pugh, Everett laid out his thesis. [188] Pugh was impressed.[189] Everett said that he thought his theory might be ahead of its time.[190] Pugh asked why not pursue the idea of using his continuous and deterministic theory to reconcile quantum mechanics with general relativity.[191] Everett replied that he thought it might be useful in that regard, but he already had enough on his hands with his interpretation of quantum mechanics and was not eager to take on another "theoretical monster". [192]

Feynman objects to Everett's Theory at Chapel Hill; Wheeler directs Everett to rewrite his Thesis

Meanwhile, Wheeler and Misner and several of Wheeler's other students were preparing their papers for the conference at the University of North Carolina at Chapel Hill on "the Role of Gravitation in Physics". The conference was dominated by Wheeler and his students, present and former, including already legendary Richard Feynman.[193] Everett did not attend, but his influence was present.

Misner recalls that the major theme of the conference was the prospect of quantizing gravity by the means of Feynman's path integral method.[194] Wheeler and Misner believed that Everett's concept of a universal wave function might also be useful to this end. Misner presented his dissertation at the conference. He reports to Byrne:

> "My Feynman path integral approach to quantum gravity is mostly considered an attempt to calculate the operations necessary to evolve the wavefunction of the universe forward in time. A rigid adherent of the Bohr observer-driven collapse of the wave function would have anathematized any attempt to evolve a wave function which served no observer. Thus the awareness that Hugh's alternative view of quantum mechanics existed left me free to think about formulating the dynamics of quantum gravity."[195]

At the close of the conference, Wheeler suggested that Everett's "universal wave function" might be a solution to a number of problems in the quantizing of gravity, including the difficulty of elaborating all of the Feynman propagators in a sum over histories approach.[196] Earlier in the conference, Feynman had been willing to consider a no collapse interpretation of quantum mechanics in this context:

> "Feynman sketched an experiment on a blackboard showing a ball influenced by a gravitational field while entangled with a superposed quantum system. Taking up Wheeler's suggestion to ignore the collapse postulate, he concluded, "If you believe in quantum mechanics up to any level then you have to believe in gravitational quantization in order to describe this experiment."[197]

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[188] *Ibid* p. 205

[189] *Ibid*

[190] *Ibid*

[191] *Ibid* p. 206

[192] *Ibid*

[193] *Ibid* p. 178-180

[194] *Ibid* p. 180

[195] *Ibid*

[196] DeWitt and Rickles p. 269-270

[197] Byrne p. 181

But now when faced with an alternative to his methods, Feynman balked at following Everett's theory to its logical conclusion:

> "(T)he concept of a "universal wave function" has serious conceptual difficulties. This is so since this function must contain amplitudes for all possible worlds depending on all quantum-mechanical possibilities in the past and thus one is forced to believe in the equal reality of an infinity of possible worlds."[198]

Feynman here displays his extraordinary knack for summing up a complex theory and its consequences in simple and easily understandable terms, but for Everett, of course, these consequences, difficult as they might be to conceive, are not a bug but a feature of the theory. Wheeler must have been disappointed. Feynman was his most accomplished student, and like Bohr held a special place in his estimation. Now both his mentor and favorite former student had rejected Everett's theory.[199] Shortly thereafter, Wheeler directed Everett to revise his thesis.[200]

Wheeler versus Everett: Wheeler's Objectives

Almost four decades later, Kenneth Ford interviewed Bryce DeWitt for the Niels Bohr Library & Archives at the American Institute of Physics. DeWitt recalls:

> "And Wheeler, I asked him many years later why the original article, I mean the original thing, wasn't ever published. Wheeler said, "Because I sat down with Everett and told him what to say"".[201]

Wheeler's blunt admission raises the question to what extent the Short Thesis is truly representative of Wheeler's views as opposed to Everett's. The historical record shows how Wheeler's objectives were different from Everett's. Comparing the texts, we can see the differences between the two theses.

As we have seen in the Long Thesis, Everett takes aim at the measurement problem as it arises in von Neumann's interpretation of quantum mechanics. He does critique other interpretations, including the Copenhagen interpretation in the Long Thesis, but they are not his principal concern. In his Introduction to *Wave Mechanics without Probabilities*, Everett describes how von Neumann's dichotomous description of dynamics by process 1 and process 2 leads to a paradox when considering more than one observer. The rest of his argument is predicated on resolving that paradox by disposing of process 1 and modeling dynamics by process 2 only.

Contrast that opening argument with the Introduction to the Short Thesis:

> "The task of quantizing general relativity raises serious questions about the meaning of the present formulation and interpretation of quantum mechanics when applied to so fundamental

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[198] DeWitt and Rickles p. 270

[199] Wheeler remarked that his earlier collaboration with Feynman was one of the "most satisfying" in his life. Byrne p. 122. To put Wheeler's regard for Feynman in perspective, writing to a colleague at University of Michigan who asked for his assessment of his graduate students to inform his recruitment efforts, Wheeler placed Charles Misner first in ability, "the most able man I have seen since Feynman", and Everett second. Barrett, Byrne and Weatherall "Wheeler to Dennison, 21-January-1956."

[200] Barrett and Byrne p. 19

[201] Ford. Ken Ford says in this interview that he had not read the Long Thesis.

> a structure as the space-time geometry itself. This paper seeks to clarify the foundations of quantum mechanics. It presents a reformulation of quantum theory in a form believed suitable for application to general relativity."[202]

Everett does mention how his approach might be fruitful for reconciling general relativity and quantum mechanics in the Long Thesis but just once, and this mention is at the end of "Wave Mechanics without Probabilities" in the "Discussion" section. In the Short Thesis, quantum gravity is front and center. Even its title "The 'Relative State' formulation of Quantum Mechanics" suggests a deep connection between the theories of general relativity and quantum mechanics.

Even more telling is what did not make Wheeler's cut: Everett's chapters on "Probability, Information and Correlation" containing his information theoretic and the more extensive treatment of his measure theoretic interpretation of the wave function; his "Supplementary Topics" chapter describing how macroscopic objects with quasi classical trajectories can be expected to arise from wave mechanics and how reversibility and irreversibility and approximate measurement are treated wave mechanically; the theorems he proves pertaining to the information theoretic interpretation of the wave function and his remarks on the role of theory in physics in his two appendices are almost entirely excised. Even Everett's arguments in his chapters "Quantum Mechanics" and 'Observation" are abbreviated or redacted. "The 'Relative State' formulation of Quantum Mechanics" is only a quarter of the length of "Wave Mechanics without Probabilities".

The "Discussion" section in the Long Thesis includes Everett's criticism not only of the "popular" von Neumann interpretation; but also of Bohr's Copenhagen interpretation; hidden variables interpretations of Einstein, Bohm, Wiener and Siegel; and the stochastic interpretation by Bopp. It runs about eight pages. The "Discussion" section in the Short Thesis runs four paragraphs. One of those paragraphs suggests that the theory might "prove a fruitful framework for the quantization of general relativity."[203] There is no criticism of the other interpretations, but only the statement that the "relative-state" formulation allows for "investigation" of "the measuring process" (notably not a measurement problem but a process) and requires no additional postulate to explain the appearance of probabilities versus the "conventional or 'external observation' formulation" in which these subjects are problematic.[204] Since we know that Bohr and Petersen told Wheeler that they did not believe that there was a measurement problem in quantum mechanics and that they had denounced von Neumann's interpretation to which "the external observation formulation" refers, this statement seems to have been calculated so as not to offend the Copenhagen Orthodoxy.[205]

Months earlier, Wheeler had written to Everett, "I think your thesis is going to prove very important, but first it has to be made javelin proof."[206] From the revision that he dictated to Everett, we can see why Wheeler thought Everett's theory was important and what attacks had to be defended. The removal of most of Everett's machinery of interpretation and criticisms of rival interpretations other than von

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[202] Barrett and Byrne p. 175

[203] *Ibid* p. 196

[204] *Ibid*

[205] Petersen explicitly states that the Copenhagen interpretation is not an "external observer formulation", so that label appears to have been chosen so as to be innocuous to Bohr and his followers as well. Barrett and Byrne p. 237

[206] *Ibid* p. 211. On the occasion of Wheeler's correspondence with H.J. Groenewold on 17 September 1956.

Neumann's indicates that Wheeler wanted to avoid conflict with his mentor Bohr and his followers. The changes to the title, the Introduction and Discussion show that Wheeler believed that the importance of Everett's formulation was in regard to the task of reconciling general relativity and quantum mechanics.

The challenge of working out a theory of quantum gravity was at the forefront of Wheeler's mind. His papers published at the time and the conference he organized with DeWitt at the University of North Carolina, Chapel Hill attest to that fact. Wheeler wrote or co-wrote six papers that were presented at that conference and published in a special edition of the *Reviews of Modern Physics* guest edited by Bryce DeWitt featuring the proceedings of the Chapel Hill conference.[207] It was in this special edition that Wheeler prevailed on DeWitt to publish "The 'Relative State' formulation of Quantum Mechanics" along with his own companion paper.[208]

DeWitt recalls:

> "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 and (2) a paper by Wheeler assessing Everett's ideas. Since Wheeler had been a conference participant and since Everett's paper seemed to be relevant to the themes of the conference, I agreed to include it."[209]

Just as revealing as Wheeler's insistence on publishing the Short Thesis in this special edition devoted to a conference on quantum gravity that he helped organize is the thrust of his companion piece. Wheeler's accompanying paper concludes:

> "No escape seems possible from this relative state formulation if one wants to have a complete mathematical model for the quantum mechanics that is internal to an isolated system. Apart from Everett's concept of relative states, no self-consistent system of ideas is at hand to explain what one shall mean by quantizing a closed system like the universe of general relativity."[210]

Wheeler appears to have construed Everett's theory and arranged its publication so as to mollify the critics in Copenhagen and forward his own program of quantum gravity research. Feynman's public objection seems to have precipitated the publication of the thesis. Wheeler might have thought it was important to have Everett's theory published with the proceedings of the Chapel Hill conference as a kind of rebuttal to his prodigious former student. Up to that point, Wheeler may have been deliberately stalling the thesis committee process. One of Everett's classmates wrote to congratulate him on having his thesis posted for reading:

> "Incidentally, did you know that there was a rumor here that there were no faculty members willing to be second or third readers on it? On checking, this was scotched by Charlie [Misner] who claimed it to be a sort of ploy by Wheeler who wanted you to keep rewriting until it was in shape to convince the world."[211]

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[207] Byrne p. 179-180

[208] *Ibid*

[209] *Ibid* p. 176

[210] Barrett and Byrne p. 201-202

[211] Byrne p. 176

Everett's Academic Opportunities

Notwithstanding the cold response of many of the world's most prominent physicists to his theory, Everett had promising opportunities in academia. He might have begun by taking an instructorship at Princeton that was offered to him, a junior academic position but a sought after one at an elite university and the very same position that his roommate Charles Misner was offered.[212] Misner went on to have a very successful career in academia.

Wheeler appreciated Everett's interest in national security issues and excitement about working with the Department of Defense's state of the art technologies, but he felt strongly that Everett ought to make his career in academia. He wrote to Everett in May of 1956, right in the middle of the Copenhagen controversy:

> "I understood you prefer to go with the operation research group in the fall rather then take an academic position then (a) because of draft problems (b) because of desire anyway to do your bit for national defense (c) because equipment in Washington would allow you to do some special projects you are interested in. I respect all three wishes, but feel that in the following year you ought to start working towards a first class academic position that will allow you to stand with full freedom for a cause and subject of your own. You have something original and important to contribute and I feel you ought not to let yourself be distracted from it."[213]

In October of 1957, Wheeler wrote to Everett, reciting his praises from his new employers, but again encouraging him to take an academic position:

> "I saw General James McCormack at the Bohr Atoms for Peace Award Ceremony in Washington Oct. 24 and asked him how you were getting along. He said you were worth your weight in PU[239] and that you were one of the very top people in the whole organization in his view. However, I hope that Bob Sachs will succeed in luring you into quieter and more reflective areas at Wisconsin because I think you really have a lot of original things to give to the world which you cant do through the present set up. If you are hell bent on staying in Washington at any price why don't you let me see if George Washington University couldn't make a really attractive position for you?"[214]

Everett did not pursue either opportunity. Boston University would also offer Everett a teaching position on Wheeler's recommendation.[215] He did not take it. The chair was being held out for Everett in academia, but he refused to sit. He preferred his position in the capital with all its attendant perquisites, government resources and secrets, and the heady responsibility for gaming out Armageddon.

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[212] Byrne p. 187 and Barrett, Byrne and Weatherall "Wheeler to Dennison 21-January-1956."

[213] Barrett, Byrne and Weatherall "Wheeler to Everett 22-May-1956

[214] Barrett, Byrne and Weatherall "Dresden to Wheeler 23 October 1957 reply 30 October 1957, with note to Everett."

[215] Byrne p. 219

Section 5: The Case Against Kent's "Against Many Worlds Interpretations"

Adrian Kent's "Against Many Worlds Interpretations" is an often cited critique of Everett's theory. Published in 1990, this paper "is a critical review of the literature on many-worlds interpretations (MWI)" in which "arguments are presented against MWI proposed by Everett, Graham and Dewitt" and others.[216] Kent is a distinguished theorist who did important early work in quantum computing, and "Against" was and still is taken to be an authoritative, full throated rebuke of Everett and the Everettians. I will argue that Kent's paper is an example of how the publication history of Everett's theory has contributed to wide misunderstanding in the philosophy and physics community as to what Everett's theory is.

Kent's program in this paper is to axiomatize the various MWI theories, beginning with Everett's "to clarify the logical structure of the MWI, and so to pinpoint their essential problems."[217] Having established what he believes Everett's axioms are, Kent argues the following: Everett's measure is not derivable from his "physical interpretation"[218]; there is a preferred basis problem in Everett's theory[219]; "no physical meaning has been attached to the constants $|a|^2$ and $|b|^2$"[220] in the wave function ; there is no justification for "the corresponding components of ϕ as describing a pair of independent worlds"[221]. I believe that Kent never truly comes to grip with Everett's argument.

Kent concludes his section addressing Everett's theory by writing parenthetically that "(Reference [28] also contains a longer exposition [19] of Everett's ideas; this doesn't seem to depart from Everett's original paper on any point of principle.)"[222] Kent is referring to *The Many Worlds Interpretation of Quantum Mechanics* edited by DeWitt and Graham and Everett's Long Thesis which was first published in the former under the title "Theory of the Universal Wave Function". I suspect that this peculiar comment is a tacit admission that Kent did not read or closely examine the Long Thesis. Tellingly, Kent quotes or refers to the Short Thesis five times in his paper, but the only reference to the Long Thesis is that one reproduced above.

Perhaps owing to the strange publication history of the Long Thesis, Kent evidently believed that the Short Thesis was the original work. Of course, it is not. To be fair to Kent, there is no reason why he should have known the Long Thesis was original. The preface of DeWitt and Graham's edition is ambiguous as to the priority of Everett's writings, and Everett scholarship in 1990 was almost nonexistent.

The Long Thesis is not simply a longer exposition either, as has been shown. Everett wrote an entire appendix on the role of theory in physics. Everett's views on the subject provide important context for Kent's claims about Everett's "physical interpretation". Everett's description of amplitudes in the wave function in information theory terms and demonstration that this interpretation is more general and useful than a description in probability theory terms is entirely absent from the Short Thesis. These and

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[216] Kent p. 1
[217] *Ibid* p. 6
[218] *ibid* p. 10
[219] *ibid* p. 11
[220] *ibid* p. 12
[221] *ibid* p. 11
[222] *ibid* p. 1

other arguments put forward in the Long Thesis are relevant to Kent's claims about a supposed preferred basis, a subject which is only addressed briefly and elliptically in the Short Thesis but more completely and directly in the Long Thesis. The exposition of Everett's concept of correlation within the superposition, his definition of good measurement interactions in information theory terms, and rigorous derivation of his measure and the Born Rule in the Long Thesis, these arguments might have allayed Kent's concerns about the absence of "physical content" in Everett's theory.[223]

Or they might not have. Some of these arguments are also present in the Short Thesis, albeit abbreviated. I find Kent's claim that Everett's theory is lacking in physical content to be quite puzzling. All the more so because Everett was careful in his construction, and his views as to what was required of a physical theory in terms of mathematical and physical content as expressed in his appendix to the Long Thesis are similar at least in some respects to Kent's views as expressed in this paper.

Everett was not a Realist

Kent begins the second section of his paper under the title "The Case Against" by making the claim that "if a theory is not mathematically realist then it is not an MWI."[224] Kent quotes from Everett's Short Thesis and from DeWitt's "Preface" in *the Many-Worlds Interpretation of Quantum Mechanics* to support this claim:

> "This paper . . . postulates that a wavefunction . . . supplies a complete matehmatical *[sic Kent]* model for every . . . system without exception." and still more clearly by DeWitt: [3] "The real world . . . is faithfully represented solely by the following collection of mathematical objects. . . . The use of this word [faithfully] implies a return to naive realism and the old-fashioned idea that there can be a direct correspondence between formalism and reality. . . The symbols of quantum mechanics represent reality just as much as do those of classical mechanics."[225]

It is misleading to conflate Everett's and DeWitt's views on the relationship of theory to the world. It was a subject that arose in their first correspondence.[226] They had different views. DeWitt's remarks ought to be understood in the context of his opposition to the Copenhagen Interpretation and, specifically, Bohr's concept of complementarity.

It is fair to say that there are perhaps as many different definitions of realism as there are philosophers of science. Nevertheless, it is not reasonable to construe Everett's writings on the philosophy of science as realist. The word "realism" never appears in any of his published works or correspondence. On the contrary, Everett's epistemology most closely resembles an operationalist or empirical pragmatist view, as we have seen in Section 2 where Everett's appendix on the role of theory and physics and his correspondence with Phillip Frank are examined.

Kent does not elaborate at length on what he means by "mathematical realism". He writes, "any meaningful MWI must include mathematical axioms defining the formalism and physical axioms

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[223] *ibid* p. 12
[224] *ibid* p. 9
[225] *ibid* p. 8-9
[226] Barrett and Byrne p. 241

explaining what elements of the formalism correspond to aspects of reality."[227] Everett uses similar language, again in the second appendix:

> "Every theory can be divided into two separate parts: the formal part and the interpretive part. The formal part consists of a purely logico-mathematical structure, i.e., a collection of symbols together with rules for their manipulations, while the interpretive part consists of a set of "associations," which are rules which put some of the elements of the formal part into correspondence with the perceived world."[228]

If the above exhausts the content of Kent's mathematical realism, then he and Everett would seem to be in partial agreement here, notwithstanding the differences between a "physical axiom" and an "interpretative part" or "aspects of reality" and "the perceived world", but Kent might not have known it, as these arguments of Everett's did not appear in the Short Thesis. However, these apparently minor differences may account for a great deal of misunderstanding.

Kent's Axiomatization of Everett's Theory and his critique of Everett's measure

Kent claims the following to be Everett's axioms:

"Axiom 0 There exists a Hilbert space V , a hermitian operator H on V , and a continuum of states $|\psi(t)\rangle \in V$ for $-\infty < t < \infty$ such that $H|\psi(t)\rangle = ih\, \partial/\partial t|\psi(t)\rangle$ (1)"[229]; and

"Axiom 1E The graph of the state vector's evolution (that is, the set of coordinates $(|\psi(t)\rangle, t) \in V \otimes (-\infty, \infty)$) is a physical quantity."[230]

Then Kent makes the following peculiar claim regarding the interpretative part of Everett's theory:

> "Now Everett points out that the Hilbert space inner product defines a measure μ, by setting the measure of a state $\phi = \sum_i a_i\phi_i$ to be $\mu(\phi) = (\sum_i |a_i|^2)^{1/2}$, where the ϕ_i form an orthonormal basis of H. This measure μ is defined by the Hilbert space inner product, and so by Axiom 0 is part of Everett's mathematical formalism.
>
> However, μ does not appear in axiom 1E, and so is not a *fundamental* quantity in Everett's physical interpretation. Nor does Everett make any attempt to show that μ can be understood as a derived quantity in the physical interpretation. This leaves no way to deduce any statement connecting μ with real physics. Since μ is of course precisely what we need to describe the measurement correlations predicted by quantum mechanics, Everett's MWI is inadequate."[231]

From this point of divergence, the gulf between Kent and Everett begins to widen. Kent notes that his extended critique follows Graham's in *Many Worlds*, Bell's in *The Speakable and Unspeakable in Quantum Mechanics*, and others.[232] Perhaps he means Graham's assertions about Everett's measure

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[227] Kent p. 9

[228] Barrett and Byrne p. 169

[229] Kent p. 9

[230] *ibid*

[231] *Ibid* p. 10

[232] *ibid*

lacking sufficient motivation, having no way to "influence … the reading of a particle counter"[233] or Bell's comments regarding a preferred basis or memories and trajectories in Everett's theory[234], but he does not specify. Nonetheless, all of Kent claims about Everett's theory rely on his assertion that the above axiomatization is a complete or, at least, sufficient account of Everett's theory.

One referee objected that any axiomatic account was insufficient, that dynamics were an important part of the story, and Kent was asking "more from the axioms than they could reasonably provide."[235] Kent creditably includes this objection in his paper and attempts to answer this criticism in regards to his argument about a preferred basis, but I cite it here because I think it applies to Kent's assessment of Everett's measure as well. Perhaps it also illustrates the subtle differences between Kent's "mathematical realism" and Everett's philosophy about what a physical theory should be.

For Kent, Everett is not allowed to apply his measure because it is a part of the mathematical axiom but, he claims, not the physical axiom, and that is all there is to it. Everett's construction is different. He relies on a mathematical formalism and a physical interpretation of that formalism. For Kent there seems more of a bright line distinction between the maths and the physics, whereas Everett sees the formalism as a model which the physical interpretation allows to be mapped to experience. I will argue that Everett has a right to choose his own theory and its structure and that his choices are different from those described by Kent, but holding those objections in abeyance for the moment, let us follow Kent's logic.

Kent writes that "measure μ is defined by the Hilbert space inner product, and so by Axiom 0 is part of Everett's mathematical formalism. However, μ does not appear in Axiom 1E, and so is not a *fundamental* (Kent's emphasis) quantity in Everett's physical interpretation." Kent does not elaborate on what this distinction of "fundamental" means in this context, but strictly speaking, Everett's measure μ does not explicitly appear in Axiom 0 either. A unique measure μ on the sum of the squared coefficients of a superposition of orthogonal states is constructed by Everett relying on this property of Hilbert space.[236] The wave function in Hilbert space does also appear in Axiom 1E: "$(|\psi(t)>, t) \in V$". If Kent's claim is that μ is "defined by the Hilbert inner space product" and that Hilbert space inner product information is fundamental, for some definition of fundamental, to Axiom 0 but not to Axiom 1E, it behooves him to explain why.

Perhaps Kent is relying on "The graph of the state vector's evolution (that is, the set of coordinates … is a physical quantity" to do the work of excluding the "Hilbert space inner product" information from this

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[233] DeWitt and Graham p. 236. It is noteworthy that Neil Graham's "The Measurement of Relative Frequency", another often cited criticism of Everett's theory, was written by Graham, a supporter of the Everettian quantum mechanics, before he had read the Long Thesis. Graham's criticism of the physical motivation of Everett's measure is answered by the derivation of the squared amplitude measure from the equations of motion in the Long Thesis. See Everett's remarks to Jammer below found in Barrett and Byrne p. 295-296.

[234] Bell. Bell did not cite the Long Thesis in his paper, and unfortunately, Bell and Everett never met or corresponded.

[235] Kent p. 11

[236] Kent is not very specific here, but, *n.b.*, that Everett's measure is not a measure on Hilbert space itself, as shown previously, but a measure on the sum of superposed orthogonal states, as Everett explained in correspondence with Norbert Wiener, who mistaken believed that Everett had called for a measure on Hilbert space in his theory. Osnaghi *et al* make the same mistake as Wiener. Osnaghi *et al* p. 109. This distinction allows Everett to use a uniform measure on discrete observables and a Lebesgue measure on continuous observables.

physical axiom. This is a strange admonition that we should forget what we know about the structure of Hilbert space and what Everett knew and relied on, but let us pretend that this is the case and see if Kent's argument goes through.

Kent takes a Stern Gerlach spin experiment setup to illustrate his points, as does Everett. Kent writes:

> "Everett attempts to relate the measure μ to physics by a discussion of the memory of observers. But, given Everett's assumptions, the discussion is actually free of physical content. Thus let Φ be the state vector of an idealized observer who has witnessed N measurements of systems identical to the one above; let $\Phi(i_1, \ldots, i_N)$ describe the state of having witnessed results $(i_1, \ldots, i_N)$, where each i_j is 1 (or 0) if the j-th observation was spin +1/2 (respectively −1/2); we shall suppress the correlated state vectors of the measured particles. We have the expansion $\Phi = \sum a_{i_1,\ldots,i_N} \Phi(i_1, \ldots, i_N)$. Everett considers the vector
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> $$\Phi^{\varepsilon} = \sum_{\left|\frac{i_1 + \cdots i_N}{N} - \frac{2}{3}\right| > \varepsilon} a_{i_1,\ldots,i_N}\, \Phi(i_1, \ldots, i_N)$$
>
> and shows that $\mu(\Phi\varepsilon) \to 0$ as $N \to \infty$ for any $\varepsilon > 0$. But no new statement about physics can arise from this purely mathematical derivation. Even supposing the infinite limit were obtained, which in reality is not the case, the fact that $\mu(\Phi\varepsilon) = 0$ cannot imply that Φε is physically irrelevant, when no hypothesis has been made about the physical meaning of μ."[237]

Kent is adamant that Everett should have no recourse to deriving the meaning of μ from Axiom 0 or Axiom 1E, and on its face, this construal would appear to be fatal to Everett's argument. I will argue that Kent's axiomatization is not a good representation of Everett's theory below, but I claim that Kent's conclusion only holds if we also have no recourse to examine the properties of Axiom 1E and compare them to "aspects of reality".

Kent can hardly object to comparing the physical axiom to the world, since he has explicitly stated that this is precisely what his concept of theory calls for. If we are allowed to analyze "the graph of the state vector's evolution (that is, the set of coordinates $(|\psi(t)\rangle, t) \in V \otimes (-\infty, \infty)$)" and compare it to the records of experiments, *e.g.* Stern Gerlach experiment results, then the "physical meaning of μ" becomes evident.

We can recover the amplitudes of the wave function from the graph because the wave function is a function. Given this information, one might place some of those amplitudes into correspondence with the results of experiment. For large N, these results will converge toward limits that then may be interpreted in probability, information or measure theoretic terms. As Linda Wessels points out, this is not so different from what Born himself did when he first proposed what ultimately (with the help of Wolfgang Pauli) became known as the Born Rule.[238]

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[237] *Ibid* p. 12 N.B.: Kent uses unusual coefficients for spin, assuming that the ready state of the electron is prepared such that an observer will see spin +1/2 with a subjective probability of 2/3. This nonstandard setup is useful to Kent later in his paper where he argues against branch counting to determine probabilities in a Many Worlds Interpretation. It is not relevant to his argument against Everett.

[238] Wessels

Born was looking for a model of aperiodic motions in 1926, when Schrödinger first published his papers on his wave function.[239] He had success with Heisenberg's matrices in modeling periodic phenomena or stationary processes and inertial motion but modeling collisions with matrices eluded him.[240] Applying Schrödinger's equation to the collision problem resulted in a superposition of states of the atom and the electron. There was no consensus on how the amplitudes in the wave function ought to be interpreted. Born intuited their correspondence to experimental results. Wessels paraphrases Born, "If one thinks about the actual laboratory collision experiments, he pointed out, it is the probability or, borrowing a term from the laboratory, the 'yield function'… that is usually measured."[241] Hence the Born Rule.

In Kent's axiomatic construction, Everett is not allowed to analyze his mathematical model to make predictions about the world. Some would say that such analysis is essentially what a good theory should provide. Everett thought so. This property is what he means by a theory being "correct" or having empirical "faithfulness".[242] As mentioned above, for Everett, the interpretative part of the theory, in this case the measure μ, allows "some of the elements of the formal part [to be put into] into correspondence with the perceived world."[243] Here we see some of the implications of the differences between Kent and Everett's views on what a theory in physics consists of.

Further, in this claim about how "Hilbert space inner product" information is "fundamental" to Axiom 0 but not Axiom 1E, Kent would have us forget what we know and what Everett knew and relied on in his theory about the structure of Hilbert space and the time evolution of state vectors. It is difficult to see why Everett's or anyone's hands ought to be tied in these ways. Nonetheless, I claim that, even in Kent's own terms, his argument that Everett's theory is devoid of physical content fails here: the physical meaning of Everett's measure can be deduced from the graph of Axiom 1E when compared with the results of experiment.

The Preferred Basis Pseudo Problem

Kent continues to prosecute his case against Everett by claiming that his theory has a preferred basis problem. To illustrate his point, he takes the resulting wave function of a Stern Gerlach spin experiment again:

$$\text{“}\phi = a\, \phi_0 \otimes \Phi_0 + b\phi_1 \otimes \Phi_1\text{”}^{244}$$

And argues that "in trying to interpret this result we encounter the following problems:

Firstly, no choice of basis has been specified; we could expand ϕ in the 1-dimensional basis $\{\phi\}$ or any of the orthogonal 2-dimensional bases

$$\{\cos\theta\, \phi_0 \otimes \Phi_0 + \sin\theta\, \phi_1 \otimes \Phi_1,\ \sin\theta\, \phi_0 \otimes \Phi_0 - \cos\theta\, \phi_1 \otimes \Phi_1\} \qquad (3)$$

or indeed in multi-dimensional or unorthogonal bases. Of course, the information is [*sic*] in the wave function is basis-independent, and one is free to choose any particular basis to work with. But if one

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[239] *Ibid* p. 192

[240] *ibid* p. 187-192

[241] *Ibid* p. 194

[242] Barrett and Byrne p. 170

[243] *Ibid* p. 169

[244] Kent p. 10-11

intends to make a physical interpretation only in one particular basis, using quantities (such as $|a|^2$ and $|b|^2$) which are defined by that basis, one needs to define this process (and, in particular, the preferred basis) by an axiom. This Everett fails to do."[245]

This supposed preferred basis problem in various guises has been often cited as an objection to Everett's theory. Kent claims "trying to interpret" the above result with Everett's theory is problematic. There is something of a straw man here: Everett does not claim that all valid expansions or all of a valid expansion of the Schrödinger equation can be placed into correspondence with experience. Again, we appear to run into the consequences of the different views of Kent and Everett on the role of theory in physics. For Everett, it suffices that there is a homomorphism between the model and experience for the theory to be empirically faithful.[246]

In Everett's first letter to DeWitt, he writes:

> "First, I must say a few words to clarify my conception of the nature and purpose of physical theories in general. To me, any physical theory is a logical construct (model), consisting of symbols and rules for their manipulation, *some* [Everett's emphasis] of whose elements are associated with elements of the perceived world. If this association is an isomorphism (or at least a homomorphism) we can speak of the theory as correct, or as faithful. The fundamental requirements of any theory are logical consistency and correctness in this sense."[247]

In a footnote to his appendix on physical theory in the Long Thesis, Everett writes:

> "By isomorphism we mean a mapping of some elements of the model into elements of the perceived world which has the property that the model is faithful, that is, if in the model a symbol A implies a symbol B, and A corresponds to the happening of an event in the perceived world, then the event corresponding to B must also obtain. The word homomorphism would be technically more correct, since there may not be a one-one correspondence between the model and the external world."[248]

Kent appears to believe that Everett's account is either incoherent, in the sense that there are valid expansions of the Schrödinger equation, such as in unorthogonal bases, which admit no intelligible interpretation or cannot be placed into correspondence with experience; and or that Everett's account is incomplete insofar as he fails to provide an axiom to select only those bases that avoid such incoherent statements. These are not problems for Everett. For Everett, if the model can be placed into correspondence with experience, it meets the test; and it can.

Setting aside that Straw Man argument for the moment, Kent further claims that "if one intends to make a physical interpretation only in one particular basis, using quantities (such as $|a|^2$ and $|b|^2$) which are defined by that basis, one needs to define this process (and, in particular, the preferred basis) by an axiom. This Everett fails to do."[249] The suggestion here is that Everett has a preferred basis, but there is no preferred basis in Everett's theory. It important to understand how Everett interprets the

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[245] *Ibid* p. 10-11

[246] Barrett and Byrne p. 172

[247] *Ibid* p. 253

[248] *Ibid* p. 172

[249] Kent p. 10-11

wave function. To this end, it is illuminating to examine two important elements of Everett's theory: the "fundamental relativity of states"[250] and what Everett calls the "necessary correlation machinery for [the wave function 's] interpretation"[251] in the Long Thesis.

Everett describes the fundamental relativity of states in the Short Thesis:

> "The mathematics leads one to recognize the concept of the *relativity of states*, in the following sense: a constituent subsystem cannot be said to be in any single well-defined state, independently of the remainder of the composite system. To any arbitrarily chosen state for one subsystem there will correspond a unique *relative state* for the remainder of the composite system… (T)he states occupied by the subsystems are not independent, but *correlated*. Such correlations between systems arise whenever systems interact."[252] [Everett's emphasis]

Everett is explicit in both the Long Thesis and the Short Thesis that the relativity of states in his theory is independent of the choice of basis:

> "The first property of $\Psi^{\eta}{}_{rel}$ is its uniqueness, i.e., its dependence upon the choice of the basis $\{\phi_i\}$ is only apparent."[253]

And

> "This relative state for ξ_k is independent of the choice of basis $\{\xi_i\}$ ($i \neq k$) for the orthogonal complement of ξ_k, and is hence determined uniquely by ξ_k alone. To find the relative state in S_2 for an arbitrary state of S_1 therefore, one simply carries out the above procedure using any pair of bases for S_1 and S_2 which contains the desired state as one element of the basis for S_1. To find states in S_1 relative to states in S_2, interchange S_1 and S_2 in the procedure."[254]

The claim that Everett makes a "physical interpretation only in one particular basis" is simply false.

As to the quantities "$|a|^2$ and $|b|^2$", it is important to understand the role they play in the correlation machinery of Everett's interpretation. Contrary to what many critics and supporters of Everett have written, the squared coefficients in the wave function are not probabilities. Strictly speaking, there are no probabilities in Everett's deterministic theory. As noted previously, Everett writes in the Long Thesis:

> "We shall develop there the general definitions of information and correlation, as well as some of their more important properties. Throughout Chapter II we shall use the language of probability theory to facilitate the exposition, and because it enables us to introduce in a unified manner a number of concepts that will be of later use. We shall nevertheless subsequently apply the mathematical definitions directly to state functions, by replacing probabilities by square amplitudes, without, however, making any reference to probability models."[255]

There are correlations that map to subjective probabilities by virtue of Everett's measure on a superposition of orthogonal states. Everett writes, "If we say that X and Y are correlated, what we

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[250] Barrett and Byrne p. 103, 180

[251] *Ibid* p. 77

[252] *Ibid* p. 178

[253] *Ibid* p. 99

[254] *Ibid* p. 180

[255] *Ibid* p. 77-78

intuitively mean is that one learns something about one variable when he is told the value of the other."[256] In the case of an unorthogonal basis expansion of the wave function as in Kent's example, there is no superposition of orthogonal states to place a measure on, but there is still a relative state.

At first blush, it may seem that an unorthogonal expansion of a solution to the Schrödinger equation might prove that Everett's model is pathological, but that is not so. Everett is not claiming that such an expansion has a sensible interpretation. For Everett, it suffices that the model map homomorphically to experience. Kent is not the only author to make a claim about a preferred basis. As Greaves points out, even many self proclaimed Everettians have stipulated that there is a preferred basis problem in Everett's theory.[257] The misunderstanding may arise from clinging to the notion that "$|a|^2$ and $|b|^2$" are probabilities, for a probabilistic theory might only make sense for bases states which might happen. Although the exposition is absent from the Short Thesis, Everett construes these quantities in the terms of information theory in the Long Thesis.[258] Ironically, Kent himself points out that "the information is [sic] in the wave function is basis-independent". Finally, it is important to distinguish between the information in the wave function model, the correlation machinery for the interpretation of the model and Everett's measure on the resulting superposition of correlated orthogonal states from that model which makes predictions possible. Kent's argument does not take any of these distinctions into account. When one does, the preferred basis problem is dissolved.

Kent's Argument Against Many Worlds

Next, Kent aims at Everett's most profound and controversial claim, the interpretation of the wave function as representing many worlds:

> "(S)uppose that the basis ($\phi_0 \otimes \Phi_0$, $\phi_1 \otimes \Phi_1$) is somehow selected. Then one can perhaps intuitively view the corresponding components of ϕ as describing a pair of independent worlds. But this intuitive interpretation goes beyond what the axioms justify; the axioms say nothing about the existence of multiple physical worlds corresponding to wave function components."[259]

Again, we may begin by examining Kent's argument in his own terms. Kent uses the verb "to intuit" here without explanation. Philosophers often define intuition for their own epistemology, but Kent has not done so. A dictionary definition of intuition is "to know or understand something because of what you feel or sense rather than because of evidence."[260] But in this case, we do have evidence.

We have the evidence of the result of experiments. The double slit experiment and the Stern Gerlach experiment are evidence of the empirical faithfulness of the wave function as a model. The question is why an observer finds himself and or a determinate record corresponding to only one of the components of the wave function. This is the measurement problem.

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[256] *Ibid* p. 83, 146. See correlation for finite distributions and Everett's discussion of approximate measurement in the Long Thesis.

[257] Greaves

[258] Barrett and Byrne p. 77

[259] *Ibid* p. 11

[260] Merriam Webster. Internet source, retrieved October 2020.

Kent rests his case here on the assertion that “the axioms say nothing about the existence of multiple physical worlds corresponding to wave function components”, but clearly, at least one of those wave function components does map to a physical world, *ie* the world that is in evidence. And Axiom 1E does not say that one and only one of the wave function components is a “physical quantity” but the whole of it. Everett’s claim is precisely that the model gives us no reason to believe that any of the components is privileged above any other.

If we are not to treat each of the components of the wave function on equal terms according to Axiom 1E, then we need additional axioms like a collapse postulate and a probabilistic interpretation of the amplitudes in the wave function. (Or alternately, a construction like that of David Bohm’s). Of course, Everett explicitly rejects these: “This paper proposes to regard pure wave mechanics (Process 2 only) as a complete theory.”[261] If Kent claims the axioms he has chosen to represent Everett’s theory do not support Everett’s ontology, then it may be that his axiomatization of Everett’s theory is at fault.

A More Adequate Axiomatization of Everett’s Theory

Kent’s Axiom 0 and 1E do not exhaust the content of Everett’s theory, even as it is presented in the Short Thesis. One might give a more adequate axiomatization, and I will endeavor to do so below; but it is worth noting again that this approach is not consistent with Everett’s views on physical theory.[262] Everett believed the proper approach was a formalism and an interpretation which placed some elements of that formalism into correspondence with experience or, more properly, memory. In this schema, Everett was free to employ concepts and theorems from other physical theories to interpret the formalism and establish its correspondence with experience; and he did so liberally, appropriating arguments from statistical mechanics, measure theory, information theory, probability theory to name only the most prominent.

In attempting to axiomatize Everett’s formalism and machinery of interpretation, one runs the risk of failing to include a necessary postulate on which Everett relies or simply regurgitating the whole of physical theory up to Everett. However, Everett did neatly lay out what he thought were the essential elements of this theory for us in the Short Thesis in section 3, which I take as my guide with some additional references to the Long Thesis as noted in the following.

Axiom 0: A linear wave equation is a complete mathematical model for every isolated physical system, and “(e)very system that is subject to external observation can be regarded as part of a larger isolated system.”[263]

In his introduction to the Long Thesis, Everett describes a Wigner’s Friend setup that illustrates the difficulties that a collapse theory has in reconciling the experiences of different observers in the same world with the concept of superposition. This statement of Everett’s theory expresses his view that a world including more than one observer should be modeled by a wave function without resort to a

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[261] Barrett and Byrne p. 178

[262] Kent’s concept of an axiom is quite expansive, including what others might regard as theorems, but I follow his example below.

[263] *ibid*

collapse postulate, as he puts it, “pure wave mechanics (Process 2 only)”.[264] It is his solution to the measurement problem. Also, it is often overlooked that Everett was aware of different formulations of quantum theory. While he restricted himself to non-relativistic Schrödinger equations in his examples, he believed that his interpretation was applicable to any quantum theory that supported the superposition principle, *e.g.* quantum field theory.[265]

Axiom 1: “The wave function is taken as the basic physical entity with no a priori interpretation.”[266]

Note that the difference here is that Everett writes that the wave function is a “physical entity” not a “physical quantity” as in Kent’s reconstruction. In his own terms, the wave function is an entity, *ie* it has being by definition, the whole of it.

Axiom 2: There is a fundamental “relativity of states, in the following sense: a constituent subsystem cannot be said to be in any single well-defined state, independently of the remainder of the composite system. To any arbitrarily chosen state for one subsystem there will correspond a unique relative state for the remainder of the composite system… (T)he states occupied by the subsystems are not independent, but correlated. Such correlations between systems arise whenever systems interact.”[267]

Everett’s relativity of states and correlations between them provide a rigorous and precise method of representing the subjective experience of an observer within the wave function. Because correlation occurs with any exchange of energy and entanglement, DeWitt does not feel himself split and observes only one result. Further, the preferred basis problem is dissolved.

Axiom 3: The squared coefficients of the amplitudes in the wave function are interpreted in information theoretic terms.

Everett painstakingly demonstrates this point in the Long Thesis’ second chapter. I surmise that Everett anticipated objections along the lines of what Greaves calls the “incoherence problem” of probabilities in his theory and recognized that he required a different theoretical framework to quantify the correlation structure in the superposition that was consistent with a deterministically evolving wave function.[268] Information theory provided Everett with the tools he required.

Axiom 4: The sum of the square of the amplitudes of a superposition of orthogonal states defines a unique measure that describes the memory sequence of a typical observer as the number of observations $N \to \infty$.[269]

With the correlation information interpretation and the fundamental relativity of states, the measure provides the theory with predictive power.

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[264] *ibid* Everett’s statement of the problem of more than one observer in a world for the orthodox or collapse interpretation of quantum mechanics and his illustrative version of a Wigner’s Friend thought experiment are found on p. 73-75.

[265] *Ibid* p. 79, 134

[266] *Ibid* p. 178

[267] *Ibid* p. 178

[268] Greaves

[269] *Ibid* p. 123-125

The Verdict on Kent’s argument against Everett

Kent’s program to axiomatize Everett’s theory so as to “clarify the logical structure” and “pinpoint [its] essential problems” suffers from incompleteness. Like many, he has overlooked the importance of the Long Thesis, its fuller presentation of the theory and the significant context provided by Everett’s appendix on the role of theory in physics. However, many relevant elements of the theory presented in the Short Thesis were also elided from Kent’s sparse axiomatization inexplicably.

Kent’s four principal claims in his *Case Against* section were the following: Everett’s measure was not derivable from his “physical interpretation”[270]; there was a preferred basis problem in Everett’s theory[271]; “no physical meaning [was] attached to the constants $|a|^2$ and $|b|^2$”[272] in the wave function; and lastly, there was no justification for “the corresponding components of ϕ as describing a pair of independent worlds”.[273] The argument that Everett’s measure had no foundation in his physical axiom seems arbitrary. The assertions regarding a preferred basis and the physical meaning of the wave function ignored Everett’s arguments about the fundamental relativity of states, the concept of correlation in his theory and the information theoretic interpretation of the wave function. Kent’s assertion that the many worlds ontology is not supported by Everett’s own theory stretches credulity.

One might disagree with Everett’s choice of axioms or the arguments that follow from them, but Everett has a perfect right to make the choices and arguments. The principle of charity recommends that one ought to engage an author critically on their own terms, at least to begin with. Kent does not. Where he does engage Everett, he fails to make his *Case Against* stick.

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[270] Kent p. 10

[271] *ibid* p. 11

[272] *ibid* p. 12

[273] *ibid* p. 11

Section 6: Conclusions

On the occasion of the Misner's 18th wedding anniversary in 1977, just five years before Everett's premature death, Suzanne Misner cajoled her husband Charles and Hugh, both quite inebriated at the time, into recording their reminiscences about Princeton and the complicated birth of Everett's theory. Everett had just returned from the University of Texas, Austin where he gave his first public talk in more than a decade about his theory at the invitation of John Wheeler to an audience that fortuitously included David Deutsch. Everett is heard to say about his belated visit to Copenhagen in 1959 to make the case for his theory to Bohr in person: "that was a hell of a … doomed from the beginning."[274] "Doomed from the beginning" is an apt description, not just of his tardy sortie against the Copenhagen Orthodoxy, but of the entire imbroglio, which starts with Wheeler.

The popular narrative that Bohr's opposition was responsible for the publication of the redacted thesis is not the whole truth, as we have seen. Bohr was not a professor at Princeton, nor was he on Everett's thesis committee. [275] Bohr only had a veto at the discretion of John Wheeler. And Wheeler pushing the Short Thesis through committee approval, after it was rewritten to suit the needs of his research program, and prevailing upon Bryce DeWitt to publish it in the special edition of *Reviews of Modern Physics* devoted to the proceedings of the Chapel Hill conference on "the Role of Gravity in Physics", a conference Everett did not even attend much less present a paper at, demonstrates that Bohr's approval, which he never gave, was not necessary.

But before we cast him as the villain of the piece, we must recall that probably only John Wheeler would have signed off on Everett's thesis proposal to begin with. Wheeler was a prodigious physicist in his own right. He also possessed formidable political skills and an uncanny knack for finding and promoting creative and talented young physicists. His scientific credo was "radical conservatism", that meant following the formulae of post classical physics down whatever rabbit hole they lead, or wormhole, as the case may be.[276] Wheeler saw something in Everett and his ideas that no one else in that position was likely to see; or if they did see it, put their reputation on the line to defend. Notwithstanding his ambivalence about Everett's theory, which only grew as time passed, Wheeler continued to promote Everett and encourage him to return to academia. Wheeler was largely responsible for the several academic job offers that Everett declined in the years that followed.

Here again the received narrative about Everett being hounded out of academia by the Copenhagen Orthodoxy is false. It may be that this particular narrative persisted so long because academics have a difficult time believing that anyone who could get paid to think great thoughts would ever turn down the opportunity to do so, but Everett was getting paid very handsomely to think great thoughts inside a Top Secret think tank at the Pentagon, a position which held attractions for Everett due to his upbringing and interests that academia could never match. And as important as Everett's theory of quantum mechanics might be, which theory did our world need more in the late 1950's: "Wave

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[274] American Institute of Physics. "Interview of Hugh Everett by Charles Misner on 1977 May."

[275] As an historical aside, at the time of the Copenhagen Debate, Einstein had recently passed and von Neumann was dying of cancer. If either had been alive and well, they might have offered interesting contributions to the discussion of Everett's theory, as they had both been at Princeton's Institute for Advanced Study.

[276] Byrne p. 161 Wheeler did early work on wormholes and black holes.

Mechanics without Probabilities" or "The Distribution and Effects of Fallout in Large Nuclear-Weapon Campaigns"?[277]

The circumstances of the publication of the Short Thesis did put Everett in contact with Bryce DeWitt, who became the principal evangelist for Everett's theory in the following decades, so that is one more fortunate event that Wheeler can be credited with initiating. It must be concluded that the Short Thesis with its abbreviated arguments and orientation to reconciling quantum mechanics with general relativity is more the product of John Wheeler's research program than it is true to Everett's original vision. Everett was very clearly aiming at the measurement problem in "Wave Mechanics without Probabilities", and he had no interest in tackling another foundational problem. (Everett did not even take Wheeler's course on relativity at Princeton.)[278] As to the Long Thesis, its fuller exposition might have averted many of the misunderstandings that subsequently arose around his theory had it been published first. As DeWitt put it to Kenneth Ford in 1995, "The funny thing is, you have to read the *Reviews of Modern Physics* article very carefully, as I did, to see what's really there. Whereas in the Urwerk it's quite well spelled out, to me."[279] It must be admitted, however, that DeWitt was exceptional.

Bohr and his colleagues at Copenhagen did have the benefit of reading or at least the opportunity to read the Long Thesis. They simply could or would not consider an alternative theory like Everett's on its own terms. Their objection can be categorized as ideological. Others like Feynman rejected the theory not because of its presentation but because of its logical consequence: a multiverse composed of a possibly infinite number of universes. That objection can be categorized as aesthetic or philosophical.

Replying to Jammer in 1973, Everett writes that it was more often the latter, an aesthetic or philosophical objection, that his theory provoked:

> "The unwillingness of most physicists to accept this theory, I believe, is therefore due to the psychological distaste which the theory engenders overwhelming the inherent simplicity of the theory as a way of resolving the apparent paradoxes of quantum mechanics as conventionally conceived. Thus, the theory was not so much criticized, as far as I am aware, but simply dismissed."[280]

As remarked earlier, the source of this animus may lie in the psychological difficulty in abandoning the uniqueness of the self. When Everett spoke to the "Conference on the Foundations of Quantum Mechanics" at Xavier University in 1962 at the invitation of Boris Polodsky that psychological aversion was on display.[281] Abner Shimony asked if Everett attributed "awareness" to each universe.[282] Everett answered that he did.[283] Wendell Furry joked, "To me, the hard thing about it is that one must picture the world, oneself, and everybody else as consisting not in just a countable number of copies but somehow or another in an undenumerable number of copies, and at this my imagination balks. I can think of various alternative Furrys doing different things, but I cannot think of a non-denumerable

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[277] If Everett's theory is correct, there are worlds where Everett, Misner and Wheeler worked out quantum gravity only to perish shortly thereafter in a post nuclear exchange radioactive wasteland.

[278] Byrne p. 132

[279] Ford

[280] Barrett and Byrne p. 295

[281] *Ibid* p. 267

[282] *Ibid* p. 275-276

[283] *Ibid*

number of alternative Furrys."[284] Recall similar objections raised by Stern about resonating mystics. It is telling that seldom is the same objection made to superpositions of systems that do not have an ego.

Regardless of the motivation of the objection, no one has offered a convincing criticism to date that Everett was somehow mistaken in the way that Stern in his letter to Wheeler suggested he might be, *i.e.* there was some logical contradiction in his theory.

Everett told George Pugh that his theory might be "ahead of its time."[285] There was a generational aspect to the attitudes toward the foundational issues in quantum mechanics. In that recorded conversation between Misner and Everett, we hear:

> "Misner: Oh, actually I went through a very strange experience… I don't know whether you went through it, but I certainly did, as an undergraduate getting taught by people who had learned quantum mechanics in the thirties. And to them, quantum mechanics was really a big philosophical change, and they were shocked by the whole ideas and so forth. And somehow we were…
>
> Everett: didn't seem all that funny…
>
> Misner: ... and felt that well, you know, every new course in Physics you get some new kind of nonsense which seems to make sense a little bit later so, Q. M. is no worse than electromagnetic fields, or F = MA, or whatever it might be."[286]

Perhaps the collective feeling of being at sea in the quantum universe contributed to the community clinging so tightly to theories that assured them that the Classical world was what was real, without scrutinizing the paradoxes and contradictions that those theories entailed.

In the same vein as Misner, Barrett writes:

> "Information theory was a starting point for Everett, and it is not surprising that physicists of Bohr's and Stern's generation tended to think of information in terms of "meaning", whereas Everett thought of information as a formal notion that might be represented in the state of almost any physical system— in keeping with his background in game theory and the new science of "cybernetics." That is perhaps why Everett could easily conceive of an observer as a servomechanism, whereas Bohr (a neo-Kantian) and Stern (a Bohrian) could not separate measurement from human agency."[287]

The omission of the information theoretic interpretation of the wave function from the Short Thesis deprived the field of quantum information theory of the contribution of one of its earliest pioneers. Even today, Everett is not recognized for that aspect of his work. It attests to the fact that the Long Thesis is still more often cited than read.

As to his view of his own theory, Everett wrote to Jammer that he believed the major accomplishment of his theory was that he had derived the Born Rule from the dynamics of quantum mechanics:

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[284] *Ibid* p. 274

[285] Byrne p. 206

[286] American Institute of Physics "Interview of Hugh Everett by Charles Misner on 1977 May."

[287] Barrett and Byrne p. 224

> "I was somewhat surprised, and a little amused, that none of these physicists had grasped one of what I considered to be the major accomplishment of the theory— the "rigorous" deduction of the probability interpretation of quantum mechanics from wave mechanics alone. This deduction is just as "rigorous" as any of the deductions of classical statistical mechanics, since in both areas the deductions can be shown to depend upon an 'a priori' choice of a measure on the space. In classical statistical mechanics this measure is standard Lebesgue measure on the phase space whereas in quantum mechanics this measure is the square of the amplitude of the coefficients of an orthonormal expansion of a wave function. What is unique about the choice of measure and why it is forced upon one is that in both cases it is the only measure that satisfies a law of conservation of probability through the equations of motion. Thus, logically in both classical statistical mechanics and in quantum mechanics, the only possible statistical statements depend upon the existence of a unique measure which obeys this conservation principle."[288]

When Everett laid out his thesis to George Pugh inside the walls of the Pentagon in the Winter of 1956 while they worked on simulating World War III, Pugh asked if he believed in the reality of a multiverse. True to the philosophy of science he stated in the Long Thesis, Everett said:

> "In reality, all that we can ever know about any theory is the extent to which it seems to correspond to the real world observations we can make, and to the experiments we can do. Beyond that we never know the extent to which any of our theories capture the *real* reality of the universe, to the actual content of what *really* is out there. We hardly have a clue about what may really be out there. So we have no way of guessing how close any of our theories are to what may really be out there. All we can do is postulate our theoretical idea and then ask how well they correspond to experiments."[289]

Pugh pressed him for a straight answer. Everett replied, "70 percent probability."[290]

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[288] *Ibid* p. 295-296
[289] Byrne p. 205-206
[290] Byrne p. 206

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