Revisiting The Genesis of 'Many-Worlds'

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Abstract

It is often claimed that Wheeler insisted on a revision and consequently shortening of Everett's PhD thesis, partly because of his anticipation to reconcile the theory with Bohr's approach. In this paper, I want to propose a reading of the historiography that renders Wheeler's tenacity as admittedly benevolent but ultimately condemned to failure from the start. To this end I focus on the time of the genesis of Everett's thesis, his associated dissatisfaction with the 'Standard' and 'Copenhagen' formulation of Quantum Mechanics and Wheeler as the driving force in denying his doctoral student to be part of the debate about measurement that happened in the second half of the 1950's. If it hadn't been for a lack of knowledge and miscommunication (philosophical failure) we would presumably find ourselves in a different 'branch' of history.

1 Historical Background

This paper tries to clarify that one can't narrow the events to be discussed that accompanied the genesis of Everett's thesis down to overt dissensions. I argue that there were severe and multi-layered misunderstandings foremost rooted in deep misconceptions about epistemological considerations. In order to get a clearer picture about how 'Copenhagen's' inconclusive statements that initially preserved its 'monocracy' led to its downfall in the late 1960's, one might have to consider a) a kind of triangulation, rather than focusing too much on one side of the coin, and b) a thorough examination of the reasons for the changes in the attitudes of physicists in the US in the latter half of the 1960's. However, this is beyond the scope of this essay.

This section serves as the historical framework in which the narrative of the events took place. Specifically, I focus on the general attitude towards Quantum Mechanics' (QM) foundational issues within the United States, providing a fruitful basis for Everett's own approach.

Contrary to Europe, discussions about the conceptual foundations of QM were not remotely as popular in the US, which became the western centre for research in physics after the second World War.[1] Apart from the differing scope of interest American physicists, to a large extent, did not worry about metaphysical repercussions of QM, as is displayed in "the operationalist-pragmatist style philosophy that a good many of them shared" [1] (cf. [2]). However, the differences in respect to foundational issues were instrumentally rational, as they were propelled both by the political context of the Cold War era and the need of the application of QM to a variety of atomic and subatomic phenomena (cf. [3]). Nevertheless, even before the War, epistemological considerations hadn't been paid much attention to in the US. As a matter of fact, "[a]mong forty-three textbooks on quantum mechanics published between 1928 and 1937, forty included a treatment of the uncertainty principle; only eight of them mentioned the complementarity principle" [3]. This reflects the lack of attention that was paid to Bohr's epistemological considerations, albeit Wheeler praised them as "the most revolutionary philosophical conception of our day" [4].[2] That the attitude after the War didn't change, at least not rapidly, can be detected with Bohm's *Quantum Theory* (1951) being one of the few textbooks that included interpretational discussions about the theory; although this lack applies elsewhere too. One of the main reasons for omitting the concept of complementarity from textbooks in particular, and its nearly complete non-consideration in the US in general, can be said to have been due to its perception "as an eminently philosophical approach, an especially obscure one indeed" [5]. Bohr's doctrine therefore never had the chance to directly influence young physicists, as it only circulated at conferences or scientific journals. In spite of being called upon taking on the task of clarifying and spreading his fundamental idea by e.g. Rosenfeld (1957, in [5]) - one of Bohr's closest collaborators - who told him that there is "not a single textbook of quantum mechanics in any language in which the principles of this fundamental discipline are adequately treated, with proper consideration of the role of measurement", Bohr never complied to the suggestion.

In respect to these considerations, it seems kind of doubly paradoxical that up to the end of the 1940's the 'monocracy' of the Copenhagen view on QM was almost uncontested. On the one hand, according to Osnaghi et al. (2009), notwithstanding major differences within the 'Copenhagen group', the approach was taken for granted as Bohr, Heisenberg, Jordan or Pauli shared similar theoretical backgrounds as well as conceptions about the philosophy of science; e.g. the discreteness of atomic phenomena or neglecting an isomorphism between the mathematical formalism and physical reality. To them, the main question posed by QM was an epistemological one (cf. [7]). On the other hand the 'Copenhagen' view was associated with Bohr, mainly because of his

[1] Freire (2004) describes a "rising interest on foundational problems after World War II, when a new intellectual and social context emerged among physicists worldwide". Nonetheless, the so-called 'monocracy' of Copenhagen was not fundamentally criticised until shortly after the 'Einstein-Bohr-debate' in 1948.

[2] This is partly because of the fact that Wheeler held Bohr in high esteem - apart from working together with him on several occasions, e.g. nuclear fission projects. Wheeler compares Bohr's judiciousness "with that of Confucius and Buddha, Jesus and Pericles, Erasmus and Lincoln" [5] (cf. [6]).

"intellectual charisma" [5]. However, this description of 'monocracy' "does not tell us how physicists adhered to such a school, nor how they understood complementarity, [its] conceptual core" [2]. Even among Bohr's inner circle (e.g. Heisenberg, Pauli, Rosenfeld) and its most prominent antagonists (Schrödinger, Einstein), according to Heilbron (2001), most physicists merely made use of the formalism to examine the structure of micro-physics instead of deliberately sticking to or criticising complementarity. In this sense both the missing clarification of Bohr's approach and the historical circumstances explain not only the trend in the US towards pragmatism, but also indicate that the vagueness of 'Copenhagen' may have, counterintuitively, fostered its position as 'the' approach to QM.

It was not until the early 1950's, hugely influenced by the 'Einstein-Bohr-debate' in 1948, that the dissidences about foundational issues experienced a revival. While the first half of the decade was predominantly engaged with the causal interpretation of Bohm, the latter half was occupied with a surge of research papers on the problem of measurement in QM. It's exactly in this second half of the 1950's that I claim Everett could have been part of the debate; his ideas have been in theory, so to say, 'in the right place at the right time'.[3] However, as will be discussed in the following, multiple intertwined reasons rendered this serious opportunity impossible.

2 Everett on the 'Standard' and 'Copenhagen' Interpretation of QM

> "[T]he particular difficulties with quantum mechanics that are discussed in my paper have mostly to do with the more common (at least in this country) form of quantum theory, as expressed for example by von Neumann, and not so much with the Bohr (Copenhagen) interpretation" (Everett to Petersen, May 1957, in [8]).

That Everett does not distinguish Bohr from 'Copenhagen' can be read as reflecting the common atmosphere among physicists of that time, as explained above. Independently, in a letter to Jammer (1973, in [9]), Everett states that his "primary influences [while his ideas took shape] were von Neumann's book and the later chapters of Bohm's Introduction to Quantum Mechanics". He furthermore reports that his essential ideas for the theory arose from debates with his fellow graduate student Charles Misner and Aage Petersen who was spending a year at Princeton and worked as Niels Bohr's assistant at the time. Encouraged by Wheeler, he worked those conceptual deliberations out into his PhD thesis.

The English translation of von Neumann's *Mathematical Foundations* was published in the US in 1955, exactly fitting the time of Everett's idea-forming process; however,

[3]Additionally, Princeton was home to many expert figures of QM: von Neumann, Wigner, Einstein or Bohm (although the latter was forced to leave in 1951).

Barrett and Byrne (2012) mention that he had presumably already studied the German original. Both of Everett's theses (*Wave Mechanics Without Probability* (1955-56) - the 'long thesis' from now on - and *On the Foundations of Quantum Mechanics* (1956-57) - the 'short thesis'[4] from now on) unequivocally refer to von Neumann's book. Everett's dissatisfaction with the 'Standard' view of QM stems mainly from his understanding of the two dynamics present in the formalism. On the one hand, the unitary and deterministic time-evolution of the quantum state, represented by the Schrödinger-equation (Process 2); in the case of a continuous change of a system according to a wave-equation with U as the linear operator: $\frac{\partial\psi}{\partial t} = U\psi$. On the other hand the discontinuous and non-linear 'collapse' dynamics (Process 1) occurring whenever a measurement is being performed on a physical system S; induced through observing a physical quantity with eigenstates ϕ_1, ϕ_2, ..., according to the Born-rule $|(\psi, \phi_j)|^2$ (cf. [10]; cf. [8]). To put it simply: in Everett's understanding, since the measurement apparatuses M consist themselves of systems described by the theory as evolving according to Process 2, the compound of $M + S$ should do so too (cf. [8]). The so-called 'collapse of the wave-function' was originally proposed by von Neumann and Dirac in 1932 and represents an additional dynamics to the continuous and linear Schrödinger-dynamics. Based on Process 2 and the associated superposition of states, Process 1 simplified states that whenever a measurement is performed, the observed-system 'collapses', 'jumps' or 'reduces' to one of its possible eigenstates. It's exactly this - for Everett - a-causal and ad hoc nature of Process 1 that propelled the formulation of his thesis (cf. ibid.).

However, "[t]he Bohr interpretation is to me even more unsatisfactory, and on quite different grounds" [8] (Everett to Petersen, May 1957). His discontent with 'Copenhagen' (qua Bohr) mainly stems from the way it handles the concept of *measurement*. For Everett, one is not only faced with well-known laws for the classical macro-realm and inherently unknowable laws for the quantum micro-realm, but, more importantly, has to deal with a "strange duality of adhering to a 'reality' concept for macroscopic physics and denying the same for the microcosm" (ibid.). For him, this displayed a "philosophic monstrosity" (Everett to DeWitt, May 1957, in [11]) he wanted to overcome by omitting Process 1 and exclusively viewing Process 2 as a complete description of physical reality; entailing the inclusion of the observer as a physical system by itself into the quantum-mechanical description of reality, rather than having the observer be the reason for the collapse ("the solipsist position" [8]) and therefore treating it in a privileged way. What will become more important later on (especially section 4 and 4.1): for Everett, the 'Copenhagen-observer' is not necessarily excluded from the quantum-mechanical description, but rather induces the collapse of the wave function. Wheeler's insistence on the revision of the long thesis modified this approach, albeit still being at least reminiscent of Everett's own writings. A portrayal of how this alteration relates to complementarity and thus shaped the debate between Wheeler/Everett and

[4]The short thesis is most commonly referred to in its published name *'Relative State' Formulation of Quantum Mechanics*.

‘Copenhagen’ is worth a debate on its own but exceeds the scope of this paper. For a more thorough discussion see for example [8] or [5].[5]

In order to avoid misunderstandings and overlaps, we should distinguish between two different approaches: on the one side we find von Neumann-Dirac as the ‘Standard’ view of QM (the more common one in the US and therefore the one Everett was more familiar with) and on the other hand the ‘Copenhagen’ interpretation (for Everett represented by Bohr, Heisenberg etc.). While Everett himself states that he has studied von Neumann and Bohm’s books, we can quite safely say that he wasn’t acquainted with “Bohr’s epistemological writings” [2]. This entails, briefly, that Everett’s approach was a) fundamentally based on his understanding of von Neumann-Dirac (‘Standard’ QM) rather than Bohr, or, equivalently, ‘Copenhagen’ and b) that he nevertheless was not reluctant to criticise ‘Copenhagen’, regardless of his unfamiliarity with its fundamentals and inherent ambiguities (cf. Everett 1956 in [8]; cf. [5]).

3 Everett’s Approach to QM

Everett depicts his discontent with the ‘Standard’ formulation with two idealised thought experiments (nowadays known as a ‘Wigner’s friend’ type)[6] to argue against the consistency of the formulation. Even though the ‘experiments’ describe an “*extremely hypothetical*” [8] situation and therefore being well-nigh impossible to actually being performed, Everett’s target is not an actual experimental set-up, but rather a critique of the conceptual problem of *measurement*. The ‘gedankenexperiments’ ultimately end in different alternatives one might give to the consideration of a universe that is inhabited by more than one observer-system (solipsism reproach). Accordingly, Everett’s long-thesis proposal is that

> “[t]he general validity of pure wave mechanics, *without any statistical assertions*, is assumed for *all* physical systems, including observers and measuring apparata. Observation processes are to be described completely by the state function of the composite system which includes the observer and his object-system, and which at all times obeys the wave equation (Process 2)” (1955-56 in [8]).

In his project one is able to consider “the state functions themselves as the fundamental entities” (ibid.) and therefore also the state vector of the entire universe. In simpler, exaggerated terms: Everett proposes a quantum-mechanical description that treats the mathematical formalism as deterministic and causal and our subjective experience as

[5]Everett’s strategy was strikingly similar to assertions made by Schrödinger (cf. [12],cf. [13]). Nevertheless, Schrödinger did not give the state vector an epistemic denotation.

[6]The argument these days known as ‘Wigner’s friend’ only appeared in a paper in 1961. However, it is not entirely clear whether Wigner was inspired by Everett’s paper, or Everett took up the idea in discussions with Wigner - as Everett also learned about the two dynamics (Process 1 and 2) in Wigner’s class (cf. [2]; cf. [5]).

indeterministic and probabilistic.[7] A central assumption for how he is able to infer experience from the symbolism is therefore his understanding of the 'state vectors' themselves: for Everett, they correspond to the physical state of the system (cf. [8]). It's exactly in this sense that he regards von Neumann's depiction of the observation-interaction as "involving a physical process" [5] signifying "an isomorphism (or at least a homomorphism)" (Everett to DeWitt 1957, [11]) between the mathematics and physical experience.[8] However, it is important to understand that an isomorphic relationship is not necessarily a 'one-to-one' correspondence, but, as Everett explains in a footnote, rather represents a homomorphism in our present day understanding: any theory is denoted as a logical construct, "consisting of symbols and rules for their manipulation, *some* of whose elements are associated with elements of the perceived world" (ibid.). In this regard, it is easy to see that Everett's understanding of physical knowledge is both not rooted in and distinctively different from 'Copenhagen' (Bohr).[9]

4 The Root(s) of Misunderstanding

In this section I try to show that the possible core of the futile endeavour to reconcile the un-reconcilable cannot be simply pinpointed as the misjudgement of a single person. To prevent possible misunderstandings regarding my proposal: it's not about recriminations or allegations against Wheeler, but rather about the following two intertwined assumptions. Without an involvement of the 'Copenhagen group' and a more thorough understanding of the ideas of his pupil, Everett could have been part of the debate in the late 1950's and his ideas the core of a possible alternative development and therefore course of history.

That Wheeler did not (or did not want to) understand Everett's own approach to a far-reaching extent is apparent when considering his correspondence with the 'Copenhagen group' about his student's thesis.[10]

For Everett, it's not the experimental conditions that set the framework for the interpretation of a mathematical formalism, but the theory and its symbols themselves. This axiomatic handling of a physical theory "may very well have been totally instrumental" (Everett and Misner 1977, in [15]) in the genesis of Everett's thesis and mirrors Wheeler's attitude towards theoretical physics, as Misner expressed during the

[7]Everett's concern about probability can also be detected with the titles of his manuscripts or theses dedicated towards QM and written between 1955-56: three out of four papers explicitly state the term in the title. All of them extensively treat its connotation.

[8]It should be called into attention that, depending on one's reading of von Neumann's projection postulate, the understanding of the state vector changes. Everett's interpretation of the postulate was meticulously faulted by e.g. [14].

[9]Despite my focus on a basic difference and its consequences, one could also point towards similarities or connections between Bohr's and Everett's approach; e.g. their attached importance to the concept of correlations or relativity (cf. [5], cf. [8]).

[10]I will not commit to Wheeler's terminological critique of e.g. 'splitting' or 'branching'. As in the exemplary case of a splitting amoeba, it is not entirely clear how exactly Everett meant the example to be understood.

recorded conversation with Everett: namely the "idea that you ought to just look at the equations and obey the fundamentals of physics while you follow their conclusions and give them a serious hearing" (ibid.). It's exactly this basic understanding of the *nature of physical knowledge* that I read as both a lack of engagement with epistemological essentials of the respective discussion partners, as well as the pivotal misunderstanding; without its surmounting, or at least recognition, there were no means of reconciliation. I want to claim that this fundamental difference can be regarded as the main reason for Wheeler's endeavour being lead astray and Everett being deprived of the opportunity of a remarkable career in physics.

In contrast to Wheeler (or Everett, for that matter), for Bohr, mathematical symbols in physical theories are, in principle, only endowed with denotation insofar as they correlate to experimental set-ups; for him the entire mathematics is firstly only but a "tool for deriving predictions, of definite or statistical character [and secondly] not susceptible to pictorial interpretation" [16]. Or, equivalently, as Stern's example about schizophrenia intends to show: "physical theories establish correlations between facts of our experience, the 'definition' of which does *not* involve the mathematical constructs of those very theories" (Stern to Wheeler 1956, in [5]). Notwithstanding, as I indicated in section 1, it should also not be overlooked that 'Copenhagen's' argument had various layers, that were, at least during the 1950's, not explicitly discerned - e.g. the importance of language or nature of physical knowledge; one could even speak of a mere "collection of generic statements" [5]. Replying to indistinct statements from Copenhagen (concerning Everett's view on Process 2) Wheeler wrote: "if it is a theological statement to postulate the "universal wave function" it is also a theological statement to refuse to entertain the postulate" (Wheeler to Stern 1956, in [17]; cf. [2]). One can read this exemplary utterance as an indication for Wheeler merely understanding *that* Bohr refuses Everett's basic assumption, but not *why*. Regarding the generic, even ad hoc criticism of 'Copenhagen' it could very likely be that both the correspondence and Wheeler's visit in Copenhagen in 1956 failed to reach basilar epistemological considerations. I think it is remarkable that up to the 1950's, the influence of Bohr was so great that a doctoral dissertation of an American University had to obtain the 'blessing' of 'Copenhagen' before being assessed at Princeton. Even more puzzling is the fact that Jammer, while researching for his book [18], described Everett's deliberations as "one of the best kept secrets in this century". Implying not only that Everett's thesis did "virtually [...] not have a first life in the 1950s" [2], but also that the correspondence with 'Copenhagen' had been unknown until the 1970's.

Wheeler's lack of understanding regarding Bohr's doctrine therefore is not a clear-cut affair. For reasons of limited space, I cannot go into too much detail about the miscommunications but would like to point to [18], [5] and [8] for an extensive account of this multi-layered matter.

Nevertheless, the missing clarification is even more so applicable to their critics, e.g. Everett. The above mentioned 'reliance on the outset' makes a deduction of classical from quantum physics impossible for him, hence the necessity of an experimental framework is understood as a 'strange duality' implying "that macrosystems are relatively immune to quantum effects" (Everett to Petersen 1957; cf. [19]). The existence of misapprehensions on Everett's side unfortunately is not surprising, recalling the pragmatist attitude in the US. In the tape recording of him and Misner, the latter remarked that they, in contrast to the people who learned about QM ever since its 'natal hour', were not remotely as profoundly shattered by its philosophical implications, for in "every new course in physics you get some new kind of nonsense which seems to make sense a little bit later, so quantum mechanics is no worse than electromagnetic fields, or $F = ma$" (Everett and Misner 1977, cf. [15]). His indifference towards interpretational matters can also be detected in a letter exchange with Jammer in 1973: while the latter asked Everett whether he ever studied philosophy or psychology, Everett replied that "the only formal course [he] had was an introduction to epistemology" [9] at his undergraduate institution, the Catholic University of America (cf. [20]). There simply was no serious effort on Everett's part to "to reach a deeper understanding of the philosophical background of complementarity" [5] in particular, or epistemological considerations in general. However, I read this circumstance as an advantage, namely that he, based on his unconcernedness, most likely wouldn't even have considered involving 'Copenhagen' into the thesis-process. Why would he? His plan was to provide a dissociation from it - already implied by the title *Wave Mechanics Without Probability* - for which he wouldn't need the 'blessing' of Bohr. This stands in stark contrast to what Wheeler had in mind, as will be clarified in the next section.[11]

4.1 Wheeler and Everett - Short and Long Thesis

Regardless of the misunderstandings and ambiguities surrounding the 'Copenhagen' approach, I want to argue that if Wheeler would have taken seriously the concerns of his pupil, he might not have tried to find a common ground for Everett and Bohr. That he only agreed on supervising Everett because of his hope to reconcile QM with General Relativity (GR) is at most secondary;[12] what can be stated quite safely by contrast is his receptiveness towards new and far reaching ideas, e.g. him supervising Feynman's doctoral dissertation that introduced the path-integral formulation of QM (cf. [21]).

Wheeler might have had admirable intentions for Everett, nevertheless, his plans on how to go about handling the thesis of his PhD student were everything but restrained: he planned on publishing the dissertation at the 'Danish Academy of Science' in full length in order to legitimate it in Europe, as he had the feeling that the "original work [of his student] is destined to become widely known, and it ought to have the bugs ironed out of it before it is published rather than after" (Wheeler to Everett 1956, in [8], cf. [2]. That he probably did not entirely see through Everett's approach (the 'bugs'

[11] It can be quite safely assumed that the idea of involving Bohr originated from Wheeler. However, according to Osnaghi et al. (2009), one nevertheless cannot be entirely certain of it.

[12] "Princeton was in the small minority of places in the US at which physicists were interested in general relativity and cosmology" [5]

he wanted 'ironed out') is most evident when contrasting his concerns (universalising 'Copenhagen') with those of his pupil (overcoming its shortcomings).

Not caused, but likely fostered by both the difference in attitude between Bohr and Everett/Wheeler, and the correspondence between Wheeler and the 'Copenhagen group', Everett's thesis was not only shortened by a significant amount, but basically obliterated.

While Everett in his long thesis (1955-56) criticised Bohr qua 'Copenhagen' (section 2), in his short thesis (1957) he referred to Bohr qua "'external observation' formulation" [22] and by association expressed the concern of reconciling QM with GR. One result of the thesis' revision evidently was postulating the inevitability of leaving the observer-system out of the mathematical expression on measurement interactions; thereby substituting Everett's own view of QM "*presuppos[ing]* the classical level and the correctness of classical concepts in describing this level" [8]. This surrogation can be interpreted as the hand writing of Wheeler, as Everett himself didn't bring up such criticism even in correspondence after the paper was written (cf. Everett to Petersen 1957, [19]). Bohr on the other hand, albeit frequently underlining deliberations regarding the necessity of classicality, never made such claims: "it is of course possible to include any intermediate auxiliary agency employed in the measuring process [into the quantum-mechanical description]" [23].

It is puzzling that Wheeler either did not realise Everett's deep dissatisfaction with 'Copenhagen' or simply circumvented it in order to not "offend his hero Bohr" [24]. After his visit to Copenhagen and a letter from Stern (1956), Wheeler 's reply to criticism of the 'Copenhagen group' against Everett's long thesis teems with misunderstandings of his student's ambitions: he asserts 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 an self consistent, despite a few traces that remain in the present thesis, draft of a past dubious attitude" ([17], emphasis added).

It is remarkable that there is no track of such a shift in attitude in Everett's own writings. Regarding the remarks above (section 2 and 3), he even thought that the 'Copenhagen' approach to measurement, based on his understanding, was 'even more unsatisfactory' than the 'Standard' von Neumann account. That Everett tried to undermine instead of generalise 'Copenhagen' is contrasted by Wheelers assurance that the dissertation "is not meant to *question* the present approach to measurement, but to accept and *generalize* it" (ibid.). This could be an indication that, for him, the question was not so much about the nature of physical knowledge itself, but rather a mere semantic difference between his 'hero' and pupil. The missing perception of this deep philosophical chasm between the un-reconcilable ultimately lead the situation into an impasse, which was eventually escaped by revisiting the long thesis in such a way that it would almost completely bury Everett's own approach by shifting the focus from an undermining to a generalisation.

While the long thesis displays Everett's concerns (particularly von Neumann's formulation), the short thesis reflects Wheeler's concerns about the 'external observation formulation' for the purpose of harmonising fundamentally different - sensu epistemological - approaches. Unlike the long version, the short thesis left out claims about the shortcomings of 'Copenhagen' and focused on the realm of its area of validity. Everett's ideas were rendered from a serious critique to a mere objectified modification of the 'Copenhagen' interpretation. Wheeler "sat down with Everett and told him precisely what to omit from the manuscript of 1956" [24]. Additionally, the thesis was published with an attached assessment by Wheeler (cf. [25]). In order to make the thesis more easily digestible to Bohr, the revision of the dissertation not only shortened its length by a considerable amount - from around 96 to 23 pages - but also took away the last possibility for Everett to be part of the debate about measurement in the second half of the 1950's. "In comparison with the 1956 paper [...] it is practically new text" [24].

Without a discussion, and, more importantly, a perception of the essential divergences in the first place, there was no possibility to find a common ground between Wheeler's highly praised student and the so-called 'father-figure' of QM. Summarising, Wheeler's understanding of Everett's ideas combined with his lack of comprehension of Bohr's doctrine led to an undermining of his student's proposal and eventually to the shortened thesis Everett ultimately defended. These misunderstandings could have been prevented with a more thorough examination on either side; notwithstanding my picturing of Wheeler as the intermediary, or, equivalently, stimulating factor.

5 Conclusion and Different History

In sum, I delineate three main reasons for Everett not taking part in the measurement debate in the second half of the 1950's: a) inherent ambiguities within 'Copenhagen' itself, associated with mere 'generic statements' rather than explicit references towards measurement; b) as a consequence thereof, Wheeler's misunderstanding of 'Copenhagen's or, equivalently, Bohr's epistemological foundations and, most importantly, c) Wheeler's lack of understanding of Everett's own approach that eventually lead to an involvement of 'Copenhagen' and ultimately to the undermining of Everett's ideas. It has to be put into the realm of speculation however, whether Wheeler, had he understood Everett's severe dissatisfaction with how 'Copenhagen' dealt with the problem of measurement, would have proposed to supervise Everett in the first place. By the same token, the possibility for Everett to further pursue a career in academia, as he, even before starting his PhD, had immense interest in game theory and military matters has to remain speculation. Nevertheless, what I think is safe to say is that we would be faced with a quite different theory of 'Many-Worlds' and therefore historiographical description, had Everett not "washed [his] hands of the whole affair in 1956" (Everett to Lévy-Leblond 1977, in [8]).

Summarising the essay's central implication: while Everett aimed for an explicit critique of - and therefore suggested a real alternative to - the 'Standard' and 'Copenhagen' interpretation of QM, Wheeler's insistence on revision resulted in a generalising of and hence a formulation of the thesis that suggested a more universally applicable 'Copenhagen'. The reworking of the dissertation ultimately reduced the original approach to a mere alteration, rather than using its existing capacity to propose an apt rival to the 'Copenhagen' or 'von Neumann' formulation - like e.g. Bohm's causal interpretation was. In lieu of Wigner being the responsible character for bringing the "Princeton school" [26] into being in the late 1960's, Everett could have been the one to pose a contradistinction from 'Copenhagen' already in the second half of the 1950's.

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