Olival Freire

Statement

and

Readings

History of Interpretations of Quantum Mechanics: 1950s through to 1970s

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Abstract

Since the 1950s the landscape concerning interpretations of quantum theory has dramatically changed as complementarity no longer reigns alone and alternative interpretations have begun to appear. Two young American physicists, David Bohm and Hugh Everett, were the main protagonists challenging the received views on the interpretation of Quantum Theory (QT). Bohm criticized the abandonment of determinism and the well defined properties in the quantum domain. He built a model for electrons taking them as bodies with a position and momentum simultaneously well defined and was able to reproduce results obtained by QT in the non-relativistic domain. His interpretation received both the technical name of "hidden variables" and the more philosophically inclined "causal interpretation". Everett built his interpretation, later entitled "many worlds", dispensing with the second kind of evolution of quantum states that von Neumann had taught would govern measurements. For Everett, measurement was ruled by the same mathematical machinery of Schrödinger's equation. In particular, he disliked the complementarity assumption that quantum physics requires the use of classical concepts while limiting their use in the quantum domain as certain pairs of these concepts are complementary but mutually exclusive. Since then the number of alternative interpretations of QT has grown and by 1970 it became commonplace to refer to different interpretations even as far as the orthodox interpretation was concerned. Thus, in addition to the Copenhagen interpretation, a name coined in the battles of the 1950s, there appeared the Princeton interpretation; the latter represented by von Neumann and Wigner and the former by Bohr, Heisenberg, and Pauli. However, while they have become an industry for physicists and philosophers, populating many technical journals and books, they are conspicuously absent from physics teaching and most of the research on physics teaching. The very existence of several interpretations of QT seems to be an inconvenient truth for the business of teaching of physics. The problem about this issue is that most of these alternative interpretations lead to the same experimental predictions, at least in the non relativistic domain. Philosophers, logicians, and historians, however, are familiar with this kind of issue. Indeed, the plethora of quantum interpretations is one of the best examples of the so-called Duhem-Quine thesis: the underdetermination of theories by the empirical data.

Chapter 22
Continuity and Change: Charting David Bohm's Evolving Ideas on Quantum Mechanics

Olival Freire Jr.

22.1 Introduction[1]

"It is too bad, very sad indeed, that he did not live to see how his reputation has shot up recently. His interpretation of quantum mechanics is becoming respected not only by philosophers of science but also by 'straight' physicists." The words of the American physicist Melba Phillips, a long-standing friend of David Bohm (1917–1992), demonstrate yet another case of posthumous recognition in science.[2] In fact since the 1990s Bohm's first proposal for an interpretation of quantum mechanics (Bohm, 1952a), now labeled "Bohmian mechanics", has enjoyed a larger audience than his original proposal got in the early 1950s. A sign of the late prestige accorded to Bohm and to the field he mostly worked in is the volume in honor of the centenary edition of *Physical Review,* the most influential American physics journal. It includes commentaries and reprints from the most important papers ever published in this periodical. In the chapter on "Quantum Mechanics", edited by Sheldon Goldstein and Joel Lebowitz, all the papers including Bohm's 1952 paper on the causal interpretation concern foundations of quantum mechanics and a photo of Bohm opens the chapter (Freire, 2005). However, Bohm's current prestige was not totally unexpected. An inspection of the Festschrift honoring his 70th birthday reveals that in life Bohm received tributes from scientist such as Ilya Prigogine, Maurice Wilkins, and Richard Feynman, all Nobel Prizes at the time of this book appeared, Anthony Leggett, who would go on to win the 2003 Physics Nobel Prize, John Bell, Roger Penrose, David Pines, Bernard d'Espagnat, Jean-Pierre Vigier, in addition to a number of Bohm's collaborators (Hiley and Peat, 1987), and the ultimate accolade was to be elected Fellow of the Royal Society in 1990.

David Bohm was a thinker whose influence went well beyond that of the field of "straight" physics. Neurophysiology, biology, and psychology are some of the

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[2] Melba Phillips to David Peat, 17 Oct 1994, David Bohm Papers, A22, Birkbeck College, London.

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fields where traces of Bohm's influence can be found. His sphere of influence grew from the 1980s on and he became a cultural icon as a consequence of his contact with eastern thinkers, such as Jiddu Krishnamurti and the Dalai Lama, and his search for a dialogue among science and religion and mysticism. All this influence is claimed to be based on David Bohm's work on the foundations of quantum mechanics. However, Bohm's thoughts on this subject changed meaningfully over the course of the four decades he worked on this and it has been hard to identify which part or stage of his thinking is being considered when his ideas are invoked by his readers. An early example of this was Fritjof Capra and his best seller *The Tao of Physics* (Capra, 1991), where Bohm's ideas on order in quantum theory were presented while Bohm's previous ideas on a causal interpretation of the same theory were ignored. Bohm did not help his readers to make sense of the evolution of his thoughts and in the most widely influential of his books, *Wholeness and the implicate order* (Bohm, 1980), he conflated different stages of his interpretation of quantum mechanics. Even in a paper showing the connections between two of his most important approaches to quantum mechanics, when "asked to explain how [his] ideas of hidden variables tie up with those on the implicate order" he emphasized the continuity more than his change of emphasis (Bohm, 1987).

This paper thus intends to chart the evolution of Bohm's ideas on the interpretation of quantum mechanics dealing with both the elements of continuity and change. Continuity in his thoughts is mainly related to his reflections on realism in physics and attempts to depict the kind of world quantum physics is intended to describe. From the search for a "quantum worldview," a chapter of his 1951 *Quantum Theory* textbook, to the presentation of *The Undivided Universe* as "an ontological interpretation of quantum theory," Bohm kept ontology as the philosophical goal of his investigations. The main changes were related to the role of causality, differences in scientific styles, and the creation of new concepts. Bohm indeed abandoned the quest for a causal interpretation of quantum mechanics moving to give both deterministic and probabilistic laws the same philosophical status. Bohm also moved from the construction of physical models able to reproduce quantum mechanical predictions to attempts to mathematize a few foundational concepts such as order and ultimately to build new physical theories with quantum theory as their limits. It is beyond the scope of this paper to discuss the historical contexts which led him from one stage to another in detail. Instead, I will only review the growing relevant literature. This paper is organized as follows: Section 22.2 is devoted to his early reflections on quantum theory as expressed in his 1951 *Quantum Theory* textbook, but it also deals with Bohm's causal interpretation, including its reception among physicists and its developments. Section 22.3 covers a period beginning in the late 1950s when he abandoned his causal interpretation to the early 1980s, when research to mathematize the insight of implicate and explicate orders matured. Section 22.4 deals with Bohm's thoughts at a later stage, when parts of the causal interpretation were revived, wearing different philosophical clothes, and overlapped with research on the mathematization of the idea of order, eventually leading to the concept of "active information." The fifth and final section is devoted to the legacy of Bohm's ideas, which includes both the research program called "Bohmian mechanics" and the continuing quest for the mathematization of order by Basil Hiley, a longstanding collaborator of Bohm.

22.2 Shifting to a Causal Quantum Mechanics

From the philosophical point of view, Bohm's (1951) *Quantum Theory* is remarkable for its attempt to combine Niels Bohr's complementarity with Bohm's own kind of realism. The former denied quantum theory the ambition of describing a world independent of measurements, while the latter included an ontological description of the quantum world, referred to by Bohm as "an attempt to build a physical picture of the quantum nature of matter." Commitment to an ontology for the quantum phenomena was to be a lasting philosophical feature of Bohm's approach to quantum mechanics. The book is also noteworthy for his conceptual clarity and a few innovations such as the reformulation of the EPR thought experiment using spin instead of position and momentum, which later became the standard formulation for theory and experiments about Bell's theorem due to its mathematical simplicity. Bohm also included a treatment of the measurement process using random phases.

No sooner was the book completed, Bohm was already dissatisfied with it. In a process yet to be well charted by historians, Bohm moved to a causal interpretation of quantum mechanics. Unlike Planck, Lorentz, Einstein, or the early critics to quantum mechanics, he did not express just a hope of going back to a causal description for atomic phenomena. In fact, he built a model for his approach assuming that an object like an electron is a particle with a well defined path, which means it has a simultaneously well defined position and momentum. In this model it suffers the physical influence both from potentials such as electromagnetic potential and a new potential resulting from the mathematical manipulations of Schrödinger equation, which Bohm labeled "quantum potential." These ideas were encapsulated in his 1952 paper titled "A suggested interpretation of the quantum theory in terms of 'hidden' variables." This model was very close to the pilot wave that Louis de Broglie had suggested in 1927 though did not pursue. Bohm was unaware of this but quickly learnt of Pauli's early criticisms to such a model. Bohm further developed his approach, the second part of the paper being a consequence of this. Thus, even a harsh critic like Pauli conceded that the approach was logically consistent while he did not accept it for epistemological reasons (Freire, 2005).

Bohm's 1952 paper had philosophical implications as a consequence of its own physical assumptions. According to Bohm (1952a: p. 166), his interpretation "provides a broader conceptual framework than the usual interpretation, because it makes possible a precise and continuous description of all processes, even at the atomic level." More explicitly, he stated that

> This alternative interpretation permits us to conceive of each individual system as being in a precisely definable state, whose changes with time are determined by definite laws, analogous to (but not identical with) the classical equations of motion. Quantum-mechanical probabilities are regarded (like their counterparts in classical statistical mechanics) as only a practical necessity and not as a manifestation of an inherent lack of complete determination in the properties of matter at the quantum level.

Bohm was so fully aware of the philosophical implications of his proposal that he concluded (pp. 188–9) by associating and criticizing the usual interpretation of quantum mechanics, that of complementarity, as following from the nineteenth century positivism and empiricism preached by Ernst Mach. Such philosophical implications concerned the adoption of a realist point of view toward physical theories and the recovery of determinism as a mode of description of physical phenomena, both discarded by the complementarity view. Later in his career, Bohm (1987: p. 33) emphasized that recovering determinism was not his main motivation and that his major dissatisfaction was that “the theory could not go beyond the phenomena or appearances.” The building of an ontology to overcome appearances became a permanent goal in Bohm’s research. Later, the priority he gave to determinism was relaxed but in the 1950s the debate triggered by Bohm’s proposal did indeed privilege the recovery of determinism. Bohm and his collaborators had supported the emphasis on determinism by choosing “causal interpretation” as the label for their approach. Bohm did not use this term in the title of his initial 1952 papers but he used it in his subsequent paper, while reacting to the first criticisms (Bohm, 1952b). Since then both critics and supporters have emphasized the philosophically minded *causal interpretation* over the philosophically neutral while technically accurate *hidden variable interpretation*. To illustrate how attached to the philosophical priority for causality Bohm and collaborators were we can make reference to the work he and Jean-Pierre Vigier did in 1954 slightly changing Bohm’s original model. In this work, they embedded the electron in a fluid undergoing “very irregular and effectively random fluctuation” in its motion (Bohm and Vigier, 1954). While these fluctuations could be explained by either a deterministic or a stochastic description, Bohm and Vigier framed them into the causal interpretation approach, titling their paper “Model of the causal interpretation of quantum theory in terms of a fluid with irregular fluctuations.”

Bohm’s proposal stirred up a debate and gathered adherents, yet it got a poor reception among physicists (Freire, 2005). In the late 1950s, however, Bohm’s research split from that of his collaborators like Vigier and de Broglie. While the latter persevered in their research into the causal interpretation, Bohm gave it up. A number of factors may have played a role in his decision, including discouragement by the limited response to these ideas and “because [he] did not see clearly, at the time, how to proceed further,” (Bohm, 1987: p. 40). Another influential factor, not acknowledged by Bohm himself, was his ideological rupture with Marxism in 1956–1957, which may have led him to play down the role he attributed to determinism in science and society (Freire, 2009). As a matter of fact, from 1960 on Bohm gradually began to search for a new approach to the interpretation of quantum mechanics.

22.3 Implicate and Explicate Order

The new approach took 10 years to mature. Indeed, only around 1970 the first papers suggesting “a new mode of description in physics” (Bohm et al., 1970) and taking “quantum theory as an indication of a new order in physics” (Bohm, 1971, 1973) appeared. Bohm drew heavily on analogies and images to convey the content of his new ideas on order, the most well known being the image of a drop of ink falling into a rotating cylinder full of glycerin. When the cylinder rotates in one direction the ink disappears in the glycerin, which Bohm referred to as the implicate order. When it rotates in the opposite direction, the drop reappears, namely the explicate order. Bohm would associate the explicate order with classical or macroscopic phenomena and implicate order with quantum phenomena. As for Bohm the usual interpretation of quantum mechanics was not the final word in quantum physics, he went on to associate the implicate order to a physical theory yet to be worked out that has standard quantum mechanics as a limiting case (Freire, 1999).

Implicate and explicate order would have remained just as philosophical or scientific insights if it had not been the mathematical elaboration it later received. To accomplish this Bohm did not work alone. He counted on the collaboration of Basil Hiley, his assistant at the Birkbeck College since the early 1960s. Their strategy was to analyze the algebraic structures behind quantum mechanics mathematical formalism and subsequently look for more general algebras which could be reduced to the quantum algebras as special cases. This strategy was informed by the fact that they did not want to take any kind of space-time geometry from the beginning of their reasoning. Instead they tried to develop algebraic structures from which space-time could emerge. Here the algebraic primary structure would be the implicate order and the emerging space-time geometry would be the explicate order. With the benefit of hindsight, we can identify Hiley's unique contribution in this sense. A number of different factors also contributed to the development of this mathematical approach, such as new and mathematically talented students including Fabio Frescura, interactions with the mathematician Roger Penrose at Birkbeck Collehe, and inspiration from the Brazilian physicist Mario Schönberg's early works on algebras and geometry. Highly sophisticated from the mathematical point of view, such an approach has however suffered from little contact with experimental results, which could help to inform the mathematical choices to be done.

Before going on to the next stage of Bohm's ideas on quantum mechanics, let us summarize the influences which had led to the ideas of implicate and explicate order. As recalled by Bohm, there was his search for new ideas, his enduring reflection about what was common to his previous approach and standard quantum mechanics (a task that was eased by John Bell's work pointing to non-locality as the irreducible quantum feature), the insight from a TV program in which he saw the demonstration with ink and glycerin, and the fruitful interaction with mathematicians and mathematical physicists. The question remains of how much Bohm was influenced in the early 1960s by his dialogues with the writer Jiddu Krishnamurti. Bohm once acknowledged some influence from Krishnamurti's psychological ideas on the non separability between observer and observed, which reinforced his ideas on the analogous problems in quantum measurement (Bohm, 1982). Later, however, he did not mention such influence again in his research (Bohm, 1987). Basil Hiley thinks that these dialogues were not influential in Bohm's physics, rather they played a role in Bohm's thoughts about society, thoughts, and creativity.[3] A reflection on the relationship between observer and observed had been an essential feature of Bohm's

[3]Basil Hiley Oral History, interviewed by O. Freire, 11 January 2008, American Institute of Physics.

early reflections on the foundations of quantum mechanics, see for instance how he treated measurement both in his 1951 book and 1952 causal interpretation. Thus, it seems that the influence of these dialogues on his physics, if any, was superseded by his enduring reflection on measurement in quantum physics (Freire, 1999).

22.4 Returning to the Quantum Potential

In the late 1970s a new stage in Bohm's quest for a new approach to quantum mechanics began; albeit strongly overlapping the previous one. To a certain extent it meant a return to Bohm's 1952 ideas. This return, almost 30 years later, is vividly described by Basil Hiley[4]:

> We had a couple of research students working for us, Chris Dewdney and Chris Philippidis. They came to me one day with Bohm's '52 paper in their hand. And, they said, "Why don't you and David Bohm talk about this stuff?" And I then started saying, "Oh, because it's all wrong." And then they started asking me some questions about it and I had to admit that I had not read the paper properly. Actually I had not read the paper at all apart from the introduction! And when I took it and, so, you know, I was now faced with embarrassment that our research students [Laugh] were putting me in, in a difficult position, and so I went back home and I spent the weekend working through it. As I read it, I thought, "What on earth is wrong with this? It seems perfectly all right. Whether that's the way nature behaves is another matter." But as far as the logic, the mathematics, and the arguments were concerned, it was sound. I went back again to see the two Chrises again, I said, "Okay, let's now work out what the trajectories are, work out what the quantum potential looks like in various situations."

The students and the surprised Hiley went on to calculate the trajectories allowed by Bohm's quantum potential using the recently arrived desktop computer resources to plot these trajectories creating images of quantum phenomena (Philippidis et al., 1979). Thus, motivated by students and collaborators, Bohm returned to his 1952 approach, but now he had a new problem: how to interpret such an approach and its deterministic trajectories shaped by the nonlocal physical interactions resulting from the quantum potential. Here there is a crucial point to consider while charting Bohm's thoughts on quantum mechanics. While he and his colleagues kept the mathematics and the model used in the 1952 paper they changed many of their philosophical and conceptual assumptions. The quantum potential was no longer considered a new physical potential. Instead it was interpreted as an indication of a new order, in particular a kind of "active information." Emphasis was no longer put on the causality embedded in such an approach. According to Bohm and Hiley (1993) in their synthesis book *The Undivided Universe*, after considering terms such as "causal" and "hidden variable" interpretations "too restrictive" and stating that "nor is this sort of theory necessarily causal," they concluded that "the question of determinism is therefore a secondary one, while the primary question is whether we can have an adequate conception of the reality of a quantum system, be this causal

[4] Basil Hiley Oral History.

or be it stochastic or be it of any other nature." Their main philosophical stance was thus to look for an ontological view of quantum phenomena, while the main scientific challenge remained how to tie such a requirement with the mathematical work related to the idea of an "implicate order." This challenge has survived Bohm and is a task to which Hiley remains focused, as we will see below.

22.5 Bohm's Legacy

Bohm's main legacy for the understanding of quantum physics is his enduring insistence that the foundations of this theory deserves further investigation and that it should be conducted with open minds to see the problems from different perspectives. In addition, his causal interpretation highlighted the non-locality present both in his interpretation and in standard quantum mechanics. The very existence of such an interpretation was the main inspiration for the work that led John Bell to his seminal theorem. Lancelot Whyte once compared Bohm to Kepler (Freire, 2005). As for Bohm's legacy, it is a high accolade for a contemporary physicist to be compared to the great German mathematician and astronomer.

Yet, the meaning of Bohm's quantum potential and implicate order remains controversial. It remains a research program in progress. In fact, subsequent researchers follow one of three lines of research. The first line continues to work on Bohm's original 1952 proposal not only trying to extend the first physical models but also keeping Bohm's early philosophical commitments with determinism and realism. This is, for instance, the path chosen by Peter Holland (1993).

The second line concerns Bohmian mechanics, as coined by Dürr et al. (1992, 1996). They construed Bohm's proposal in a very clean and elegant way. While in his original paper Bohm worked out analogies between Schrödinger equation and classical Hamilton-Jacobi equations, which led to an emphasis on the role of the non-classical potential that Bohm christened quantum potential, Dürr and colleagues adopted just two premises: the state which describes quantum systems evolves according to Schrödinger equation and particles move, that is, they have a speed in the configuration space. With this approach, without quantum potentials, they derived the same results one gets both with standard quantum mechanics and with Bohm's original approach for nonrelativistic phenomena. This approach has been useful for discussing quantum chaos, and for this reason it has received wide acceptance well beyond physicists interested just in foundations of quantum mechanics. One should note that when these physicists define what they understand to be a *Bohmian theory* priority for determinism disappears and they consider that "a Bohmian theory should be based upon a clear ontology", meaning by ontology "what the theory is fundamentally about." While for non-relativistic physics they have adopted a particle ontology, they admitted that they "have no idea what the appropriate ontology for relativistic physics actually is." This way commitment to a quantum ontology comes before an engagement with a causal pattern for physical theories, a position analogous to that has been adopted by David Bohm and Basil Hiley since the 1960s.

The third line of Bohm's scientific legacy is represented by Basil Hiley, who continues to work on research that he and Bohm had been carrying out before Bohm's death. This research tries to connect the insights of implicate order and active information with the quest for algebraic structures able to underpin space-time geometry and standard quantum mechanics. This program has inherited from the causal interpretation the major challenge of obtaining a fully relativistic treatment in order to match the level attained by standard quantum mechanics with Dirac equation. Bohm had once promised that "the day that we defeat the Dirac equation, we are going to have a special victory party, with a case of champagne".[5] Recently Hiley announced that he has "now found a complete description of the Dirac theory in the Bohm tradition, Bohm momentum, Bohm energy and even a quantum potential which reduces to the Pauli QP in the non-relativistic limit".[6] Only time will tell if the case of champagne should be opened.

References

Bohm, D. (1951). *Quantum theory*. New York, NY: Prentice Hall.

Bohm, D. (1952a). A suggested interpretation of the quantum theory in terms of "hidden" variables – I and II. *Physical Review*, 85(2): 166–179.

Bohm, D. (1952b). Reply to a criticism of a causal re-interpretation of the quantum theory. *Physical Review*, 87(2): 389–390.

Bohm, D. (1971). Quantum theory as an indication of a new order in physics. Part A. The development of new order as shown through the history of physics. *Foundations of Physics*, 1(4): 111–139.

Bohm, D. (1973). Quantum theory as an indication of a new order in physics. Part B. Implicate and explicate order in physical law. *Foundations of Physics*, 3(2): 139–168.

Bohm, D. (1980). *Wholeness and the implicate order*. London: Routledge.

Bohm, D. (1982). Interview. *New Scientist*, 11 November, 96(1331): 361–365.

Bohm, D. (1987). Hidden variables and the implicate order. In: Hiley, B., Peat, F. D., (eds.), *Quantum implications: essays in honour of David Bohm*. London: Routledge, pp. 33–45.

Bohm, D., Hiley, B. (1993). *The undivided universe – an ontological interpretation of quantum theory*. London: Routledge.

Bohm, D., Hiley, B., Stuart, A. E. G. (1970). On a new mode of description in physics. *International Journal of Theoretical Physics*, 3(3): 171–183.

Bohm, D., Vigier, J. -P. (1954). Model of the causal interpretation of quantum theory in terms of a fluid with irregular fluctuations. *Physical Review*, 96(1): 208–216.

Capra, F. (1991). *The tao of physics – an exploration of the parallels between modern physics and eastern mysticisms*, 3rd expanded edition. Boston, MA: Shambhala.

Dürr, D., Goldstein, S., Zanghi, N. (1992). Quantum chaos, classical randomness, and bohmian mechanics. *Journal of Statistical Physics*, 68: 259–270.

Dürr, D., Goldstein, S., Zanghi, N. (1996). Bohmian mechanics at the foundation of quantum mechanics. In: Cushing, J. T., et al. (eds.), *Bohmian mechanics and quantum theory: an appraisal*. Dordrecht: Kluwer, pp. 21–44.

Freire, O., Jr. (1999). *David Bohm e a controvérsia dos quanta*. Campinas: Centro de Lógica, Epistemologia e História da Ciência [CLE].

[5] David Bohm to Melba Phillips [w/d – early 1950s], David Bohm Papers, C-46.

[6] Personal communication to the author, 8 March 2009.

Freire, O., Jr. (2005). Science and exile: David Bohm, the cold war, and a new interpretation of quantum mechanics. *Historical Studies in the Physical and Biological Sciences*, 36(1): 1–34.
Freire, O., Jr. (2009). Causality in physics and in the history of physics: a comparison between Bohm's and Forman's papers. *Quantum Mechanics and Weimar Culture: Revisiting the Forman Thesis, with selected papers by Paul Forman edited by Alexei Kojevnikov, Cathryn Carson, and Helmuth Trischler*. Forthcoming.
Hiley, B., Peat, F. D., (eds.) (1987). *Quantum implications: essays in honour of David Bohm*. London: Routledge.
Holland, P. R. (1993). *Quantum theory of motion – an account of the de Broglie – Bohm causal interpretation of quantum mechanics*. Cambridge: Cambridge University Press.
Philippidis, C., Dewdney, C., Hiley, B. J. (1979). Quantum interference and the quantum potential. *Nuovo Cimento*, 52(1): 15–28.

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The origin of the Everettian heresy ☆

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ABSTRACT

In 1956, Hugh Everett, then a PhD student at Princeton, proposed his "relative state" formulation of quantum mechanics. John Wheeler, who was Everett's advisor, recognized the originality and importance of such a proposal, but he denied that its non-conventional approach to measurement questioned the orthodox view. Indeed, Wheeler made serious efforts to obtain the blessing of Niels Bohr for Everett's ideas. These efforts gave rise to a lively debate with the Copenhagen group, the existence and content of which have been only recently disclosed by the discovery of unpublished documents. The analysis of such documents opens a window on the conceptual background of Everett's proposal, and illuminates at the same time some crucial aspects of the Copenhagen view of the measurement problem. Also, it provides an original insight into the interplay between philosophical and social factors which underlay the postwar controversies on the interpretation of quantum mechanics.

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1. Introduction

The "relative state" formulation of quantum mechanics, put forward by Hugh Everett III in his doctoral dissertation,[1] has become popular as one of the most heterodox interpretations of quantum mechanics. This is due, in the first place, to its non-conventional treatment of the measuring process. Remarkably, however, John A. Wheeler, who was Everett's advisor at Princeton University and a dedicated Bohrian, thought that Everett's proposal was not meant to *question* the orthodox approach to the measurement problem.[2] Indeed, Wheeler made serious efforts to obtain Bohr's blessing for Everett's ideas. In 1956, when he left Princeton to spend one semester in Leiden, he sent a draft of Everett's dissertation to Bohr and went personally to Copenhagen in order to discuss it with him and his collaborators. The debate went on in the following months, culminating in a visit paid by Everett to Bohr in 1959, 2 years after the publication of the dissertation. Notwithstanding Wheeler's reiterated efforts, however, the Copenhagen group remained not only unsympathetic to Everett's ideas, but also reluctant to attach any relevance to them.

The existence of this early debate on Everett's ideas has remained unknown until recently,[3] and its content has not been exhaustively analysed so far. More generally, in spite of the increasing attention that the relative state formulation is receiving from physicists and philosophers,[4] the context of its birth and that

☆ We are grateful to the following institutions for the permission to consult their archives and for authorizing quotations: Center for History of Physics—American Institute of Physics (Hugh Everett III Papers, *EP*; Bryce S. DeWitt oral history, *BDW*), College Park, MD; Niels Bohr Archive (Léon Rosenfeld Papers, *RP*), Copenhagen; American Philosophical Society (John A. Wheeler Papers, *WP*; Archives for the History of Quantum Physics, Bohr Scientific Correspondence, *BSC*), Philadelphia; Princeton University Library (Seeley G. Mudd Manuscript Library, Graduate Alumni Records, *GAR*; Department of Rare Books and Special Collections, Eugene Wigner Papers, *WigP*), Princeton. (Henceforth the document location will be indicated using the corresponding abbreviation.) We consulted some of the papers in possession of Mark Everett (abbreviated as *ME*) due to the kindness of Peter Byrne, who provided us with copies of them after reading the first draft of this paper in the summer of 2007. We also thank Eugene Shikhovtsev for sending us copies of some letters. Preliminary versions of this paper were presented at the Fifth International Symposium Principia (Florianopolis, Brazil), the 2007 Annual Meeting of the History of Science Society (Washington), and the seminar of the REHSEIS group at the Université Paris Diderot. We thank the participants, and particularly David Kaiser, for criticisms and suggestions. We also thank Peter Byrne, Joan Bromberg, Christoph Lehner, and Olivier Darrigol for helpful comments.
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[1] Everett (1957a).

[2] John A. Wheeler to Alexander Stern, 25 May 1956, *WP* (Series 5—Relativity notebook 4, p. 92).

[3] See Freire (2004, 2005) and Byrne (2007). See also F. Freitas. *Os estados relativos de Hugh Everett III: Uma análise histórica e conceitual.* Master dissertation, Universidade Federal da Bahia, 2007.

[4] See Barrett (1999), Butterfield (2002), and references therein. See also Ben-Dov (1990) and C. A. Lehner. *Quantum mechanics and reality: An interpretation of Everett's theory*. PhD dissertation, Stanford University, 1997.

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of its early reception have not been thoroughly investigated.[5] The purpose of the present paper is to fill this lacuna. We will analyse Everett's first manuscripts, as well as the criticisms raised in Copenhagen and the way Everett replied to them. This analysis is not meant to solve the problems that beset Everett's programme, nor to provide grounds for one particular interpretation of his ideas over the others. Nevertheless, it can contribute to the clarification of some controversial passages in his published papers,[6] and help to appraise the overall coherence of his project.

There is, however, another reason for which the reconstruction of the early debate on Everett's dissertation is valuable, namely that such a reconstruction sheds light on the role that Bohr played in the controversies over the foundations of quantum theory in the 1950s. Two issues are involved here.

The first is Bohr's approach to the measurement problem. This is a rather controversial (and poorly documented) topic,[7] on which the documentary material that we have uncovered provides interesting insights. We will examine in particular some letters in which Bohr's collaborators spell out their view of the problem and contrast it with the approaches inspired by von Neumann's theory of measurement. These letters, together with the replies of Everett and Wheeler, document the misunderstandings that hindered the comprehension of Bohr's ideas and made their epistemological and methodological implications so difficult to grasp for those who did not belong to the inner circle of his collaborators. It is quite revealing that even someone like Wheeler, who had worked with Bohr and considered himself an orthodox Bohrian, seemed not to be aware of the chasm that separated the epistemological presuppositions of Bohr's and Everett's programmes.

This brings us to another important issue involved in our analysis, namely the historiographical problem of elucidating the rise and fall of what Jammer has called the "monocracy of the Copenhagen school".[8] The story of Everett's dissertation can be regarded as a paradigmatic example of how strong the influence of Bohr was, even in the American context of the 1950s. However, as we will see, the very factors which ensured the supremacy of the so-called Copenhagen interpretation harboured the premises of its eventual decline. As a fine-grained analysis will reveal, such premises were already apparent in the Everett episode.

Section 2 outlines briefly the historical context in which Everett's proposal was conceived, focusing in particular on the attitude of the physics community towards Bohr's ideas in the 1950s. Section 3 describes the genesis of Everett's dissertation, whose content is discussed in Sections 4 and 5, in the light of several unpublished manuscripts and letters. Special attention will be paid to the conceptual background of Everett's ideas and to their relationship to other research programmes that were developed in the same period. Section 6 provides a historical reconstruction of the various stages of the debate that opposed Wheeler and Everett to the Copenhagen group. The conceptual and philosophical content of the debate is analysed in Section 7. In Section 8, after relating the epilogue of the thesis affair, we focus on the early reception of Everett's ideas. In order to elucidate the psychological, social and cultural factors which influenced the discussion in the 1950s, it will also prove enlightening to take into account the subsequent evolution of Wheeler's and Everett's ideas and careers. Section 9 summarises our conclusions.

2. Historical background: the twilight of the "Copenhagen monocracy"

In this section we outline the context in which the relative state formulation appeared. We focus in particular on Niels Bohr and the so-called "Copenhagen school", whose important (and complex) role within such a context needs to be spelled out before addressing the Everett affair itself.

2.1. *General attitude towards the foundational issues in the US*

In the US, which after the Second World War became the central stage of research in physics in the West, the discussions about the interpretation of quantum mechanics had never been very popular.[9] A common academic policy was to gather theoreticians and experimentalists together in order to favour experiments and concrete applications, rather than abstract speculations.[10] This practical attitude was further increased by the impressive development of physics between the 1930s and the 1950s, driven on the one hand by the need to apply the new quantum theory to a wide range of atomic and subatomic phenomena, and on the other hand by the pursuit of military goals. As pointed out by Kaiser, "the pedagogical requirements entailed by the sudden exponential growth in graduate student numbers during the cold war reinforced a particular instrumentalist approach to physics." In this context, "epistemological musings or the striving for ultimate theoretical foundations—never a strong interest among American physicists even before the war—fell beyond the pale for the postwar generation and their advisors."[11] A few textbooks, like for example David Bohm's *Quantum theory* (1951), discussed some issues of interpretation. However, as a rule, the textbooks in use in the 1950s (in America as well as elsewhere) did not reflect much concern at all about the interpretation of the theory.[12]

A consequence of this attitude was that little attention was paid to Bohr's complementarity, which, according to Heilbron (2001), was perceived as an eminently philosophical approach, an especially obscure one indeed.[13] Kragh has observed that "the uncertainty principle was eagerly taken up by several American physicists [...], but they showed almost no interest in Bohrian complementarity."[14] According to him: "Most textbook authors, even if sympathetic to Bohr's ideas, found it difficult to include and justify a section on complementarity. Among 43 textbooks on quantum mechanics published between 1928 and 1937, 40 included a treatment of the uncertainty principle; only eight of them mentioned the complementarity principle."

Bohr's epistemological reflections were circulated in papers presented at conferences and published in scientific journals and anthologies. Such publications were unlikely to have any direct influence on the background of young physicists, which depended

[5] Cassinello (1994) contains some historical remarks concerning the origin of Everett's thesis. Shikhovtsev (2003) provides more complete information. Both papers, however, overlook the discussions which took place with the Copenhagen group. A succinct account of such discussions can be found in Freitas (2007, *op. cit.*) and Byrne (2007).

[6] See Barrett (1999, Chapter 3).

[7] See Teller (1981), Murdoch (1987).

[8] Jammer (1974, p. 250).

[9] Referring to the attitude of American physicists towards the early debate on the foundations of quantum mechanics, Cartwright (1987) has observed that "Americans in general had little anxiety about the metaphysical implications of the quantum theory; and their attitude was entirely rational given the operationalist-pragmatist-style philosophy that a good many of them shared." According to Kragh (1999, p. 211), the "interest in foundational problems among the Americans [...] went in different directions and was on a less grand scale than in Denmark and Germany". See also Sopka (1980, pp. 3.67–3.69), Assmus (1992).

[10] See Schweber (1986).

[11] Kaiser (2002, pp. 154–156).

[12] Mehra & Rechenberg (2001, p. 1194).

[13] Chevalley (1997, pp. 598–600; 1999).

[14] Kragh (1999, p. 211).

mainly on textbooks.[15] In a referee's report of 1957, Léon Rosenfeld, who was one of Bohr's closest collaborators since the 1930s, complained about this state of affairs: "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 measurements to define the use of classical concepts in the quantal description."[16] In a letter to Bohr of the same year, Rosenfeld remarked: "There is great interest in the topic among chemists and biologists, but there is no book that one can refer them to and that could protect them from the confusion created by Bohm, Landé, and other dilettantes." And he concluded: "I will now do my bit here in Manchester by giving a lecture for chemists and biologists; but nothing can replace the book that *you* must write."[17] As is well known, Bohr did not comply.

Even the circumstances that counterbalanced the scarce propensity of American physicists towards foundational issues ran against the general endorsement of Bohr's views. For example, a number of distinguished scholars who had taken part in the early debate on the significance of quantum mechanics, such as von Neumann, Wigner and Einstein, moved subsequently to the US. But none of them were particularly well disposed towards complementarity. Furthermore, in the 1950s, the circumscribed but increasing interest in cosmology and general relativity boosted a highly speculative field of research, in which American theorists were faced with the fundamental problem of reconciling quantum mechanics with gravitation. However, the approach based on complementarity was generally considered to be unsuited to deal with such a problem.[18]

2.2. Bohr and the quantum orthodoxy

The existence of an "orthodox view" of quantum mechanics was generally taken for granted since the 1930s. However, the meaning of such a label was far from being univocally determined.[19] Several factors contributed to keeping its definition vague, and by the same token to reinforcing the impression that an orthodox view did indeed exist. The very term "Copenhagen interpretation", introduced in the late 1950s to denote the orthodox view,[20] was in the first place intended to underpin the myth of a monolithic "Copenhagen school" acting as the guardian of the quantum orthodoxy. Such a myth was to some extent constructed retrospectively to serve the purposes of the parties involved in the controversies of the 1950s, a period marked by hidden variables and Marxist materialism.[21]

Faye (2002) has argued that the label "Copenhagen interpretation" was used by people opposing Bohr's idea of complementarity" to identify what they saw as the common features behind the Bohr–Heisenberg interpretation as it emerged in the late 1920s." It was generally assumed that these "common features" were conveyed by the "standard" *formulation* of quantum mechanics, whose most popular and mathematically sound version was provided by John von Neumann's *Mathematische Grundlagen der Quantenmechanik*. This book, published in 1932, had several reprints and translations (the English version appeared in 1955). It provided an axiomatic theory in which some aspects of the presentation given by Dirac in his *The Principles of Quantum Mechanics* (1930) received a more rigorous formulation. Thus, for example, the so-called postulate of projection formalized Dirac's idea that, when a system is measured, it "jumps" into an eigenstate of the measured observable.[22]

Von Neumann's formalism can be interpreted in different ways and it is not a priori incompatible with Bohr's view. Yet, von Neumann's presentation may appear "misleading in several respects" when regarded from a Bohrian standpoint. Thus, for example, in the abovementioned report, Rosenfeld observed that "v. Neumann's book 'Foundations of Quantum Mechanics' [...], though excellent in other respects, ha[d] contributed by its unhappy presentation of the question of measurement in quantum theory to create unnecessary confusion and raise spurious problems."[23] Indeed, as Kragh puts it, "the 'measurement problem' was not the same for Bohr and von Neumann."[24] The reason why von Neumann's formulation was nonetheless routinely associated with the "Copenhagen interpretation" is that what people meant by such a term had in most cases little to do with Bohr's complementarity.[25] This is not too surprising, since even within the "Copenhagen scholars", there existed divergent interpretations of Bohr's approach.[26] We are therefore faced with two questions. First, why was the existence of a standard view of quantum mechanics taken for granted? And second, why was such a view so often associated with Bohr?

As for the first question, it must be observed that, in spite of the existence of important differences, both the intellectual backgrounds and the scientific views of people like Bohr, Pauli, Heisenberg, Born, and Jordan, who had been working together on the collective construction of quantum mechanics,[27] had several

[15] See Kuhn (1970).

[16] In 1957, Rosenfeld was requested to give an opinion about the possible translation of Louis de Broglie's *La théorie de la mesure en mécanique ondulatoire* into English. The quotation is from the (negative) referee's report he wrote on that occasion (Léon Rosenfeld. Report on: Louis de Broglie, *La théorie de la mesure en mécanique ondulatoire* (Paris: Gauthier-Villars), 1957, *RP*).

[17] Léon Rosenfeld to Niels Bohr, 14 Jan 1957, *BSC* (reel 31).

[18] This is quite apparent from the 1957 papers of Everett and Wheeler (see Section 4.2). This point was explicitly discussed by DeWitt in a lecture of 1967 (DeWitt, 1968).

[19] See Scheibe (1973, p. 9), Beller (1999b, pp. 187–188), Camilleri (2008).

[20] The term was probably introduced by Heisenberg in his contribution to the volume celebrating Bohr's 70th birthday (Pauli, 1955). The usage of such a label was criticised by Rosenfeld, because it implicitly allowed the existence of *other* interpretations (Freire, 2005, p. 28). Howard (2004) suggested that Heisenberg had in fact personal reasons—namely, the wish to break his isolation after WWII—for assimilating his own position to that of Bohr, whose ideas on complementarity he actually never endorsed.

[21] See Chevalley (1999), Howard (2004), Camilleri (2008).

[22] Dirac (1958, p. 36). For a discussion see Barrett (1999, pp. 22–37).

[23] Rosenfeld's *Report* contains further considerations about the treatment of measurement in the textbooks of quantum mechanics: "The nearest to a really good treatment is found in Landau and Lifschitz's outstanding treatise: but it is too short and not explicit enough to be a real help to the student. The only books which are purposely devoted to an exposition of the principles are v. Neumann's aforementioned treatise and a little book by Heisenberg: the first is (as stated above) misleading in several respects, the second is too sketchy and on the subject of measurements it even contains serious errors (however surprising this may appear, the author being one of the founders of the theory). As to Bohr's authoritative article, it is in fact only accessible to fully trained specialists and too difficult to serve as an introduction into this question." (Rosenfeld, 1957, *op. cit.*)

[24] "Bohr tended to see it as a problem of generalizing the classical framework in order to avoid contradictions between two mutually incompatible classical concepts, both necessary in the description of experiments. His solution was complementarity." In contrast, "to von Neumann, [...] the problem of measurement meant the mathematical problem of proving that the formalism gave the same predictions for different locations of the 'cut' between observer and object." Kragh (1999, p. 214). In the 1960s this difference in the approach to measurement gave rise to what has been called the "Princeton school". This term refers in particular to Eugene Wigner's view of measurement (see Home & Whitaker, 1992; Freire, 2007).

[25] "The Copenhagen interpretation [...] is a mixed bag, consisting of the errors and misunderstandings and superficialities of many people. [...] Hence, putting your hand into this bag you may come up with almost anything you want". (Paul Feyerabend, letter to Imre Lakatos, 28 Jan 1968, in Lakatos & Feyerabend, 1999, p. 127). Feyerabend is here defending Bohr's original view against Popper's criticisms, and arguing that Popper mispresented Bohr, just as "almost all physicists" did.

[26] See Howard (2004), Camilleri (2008), Jacobsen (2007).

[27] See for instance Rozental (1967), Heilbron (2001).

points in common. All of them endorsed both indeterminism and the assumption of the corpuscular and discrete nature of atomic phenomena. They also firmly believed in the completeness of quantum theory and were prepared to dispense with the isomorphism between the symbolic structures of physics and the pictorial representation of microscopic objects. To them, the main issue raised by quantum mechanics was not one of interpretation, but rather one of epistemology[28]: how must our view of physical knowledge be amended in order to accommodate the implications of the discovery of the quantum of action? In this sense, they were attached to the revolutionary character of quantum mechanics,[29] and were unsympathetic to any attempt to restore such classical *ideals* like causality and visualizability in microphysics.

As for the second question, 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.[30] 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."[31] 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.[32] Besides setting the agenda for the development and comprehension of quantum mechanics, Bohr and the Institute of Theoretical Physics of Copenhagen, which he had founded in 1921, provided guidance for a whole generation of physicists, including Heisenberg, Pauli, Dirac, Landau, Weisskopf, Wheeler and many others.[33] As emphasised by Beller (1999b), all those who visited the Institute were deeply impressed by the experience. However, "while in matters of complementarity philosophy not directly relevant to research, physicists were willing to repeat 'Bohr's Sunday word of worship', in physics proper they maintained a fruitful balance between humble reverence and free creativity"[34]—a balance similar to that which characterized Wheeler's attitude in the Everett affair.

During the 1920s and 1930s, the ideas which were to be identified with the "orthodox view" of quantum mechanics became quite popular. The positivist flavour of the approach developed by Heisenberg, Jordan, Born and Pauli was not only in tune with the cultural climate of continental Europe between the two wars,[35] but was also well suited to cope with the change of paradigm that atomic phenomena seemed to demand. Bohr's arguments were generally taken as a warrant that such an approach was free from inconsistency and could be accommodated in a coherent conceptual framework, although the acknowledgement of Bohr's authority implied neither the conscious adhesion to, nor the clear understanding of, his philosophy.[36]

Did this state of affairs give rise to "a somewhat dictatorial imposition of what was called 'the Copenhagen dogma' or 'orthodox view' upon the younger generation of physicists"?[37] To be sure, the defence of the orthodox ideas by a group of physicists whose outstanding prestige was unanimously acknowledged was not always carried out according to the polite rules of an open and rational discussion.[38] However, it is likely that both the existence of an "orthodox view" and the unsharpness of its definition met the needs of the majority of the physics community, which was not concerned with the foundations of quantum mechanics in so far as the theory could be efficiently used to perform calculations and experiments. Not only did vagueness act as a protective belt which prevented the users of the theory from being faced too crudely with the alleged flaws in its foundations, but it also made possible the identification between the orthodox view and Bohr's, thereby allowing them to rely on Bohr's undisputed authority when adopting such an uncritical attitude.[39] As regards the dissenters, the possibility of contrasting their original proposals with a dominant view could offer both psychological and rhetorical advantages. Generally, by the label "orthodox" (or the equivalent "official", "usual", etc.), the dissenters meant the instrumentalist attitude that rejected any attempt to provide a coherent pictorial model of the world allegedly underlying the quantum phenomena. This was of course a dramatic simplification of Bohr's stance. But identifying it with the "orthodox view" allowed the dissenters to avoid coming to grips with the more sophisticated (and, to many, obscure) aspects of Bohr's doctrine.

2.3. *The revival of dissidence and the measurement problem*

Notwithstanding some disagreements about the philosophical interpretation of complementarity,[40] between the 1930s and the

[28] See Heilbron (2001).

[29] In a conversation with Everett, which occurred in the 1970s, Charles Misner, who had been Everett's roommate at Princeton and a student of Wheeler's, recalled that, as an undergraduate, he was "taught by people who had learned quantum mechanics in the 1930's." He remarked that "to them, quantum mechanics was really a big philosophical change, and they were shocked by the whole ideas," whereas he and Everett "[...] 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 [...]." (Hugh Everett interviewed by Charles Misner, May 1977, p. 9, *EP*.)

[30] See e.g. Bohr (1949).

[31] Beller (1999b, pp. 254–257).

[32] Wheeler (1985, p. 226).

[33] See Rozental (1967), French & Kennedy (1985).

[34] Beller (1999b, p. 257).

[35] See e.g. Jammer (1966, Section 4.2), Forman (1971), Brush (1980).

[36] See Heilbron (2001).

[37] Jammer (1974, p. 250).

[38] This observation does not apply solely to the old guard of the Copenhagen school. "Some of the most vitriolic comments directed at people who questioned the Copenhagen Doctrine were given by Rosenfeld. He's written some papers that have taken the young people who were wanting to probe a little more deeply to task". (Bryce S. DeWitt & Cecile M. DeWitt-Morette interviewed by Kenneth W. Ford, 28 Feb 1995, p. 18, *BDW*.) Rosenfeld's attitude is apparent from his letters, some of which are quoted in the remainder of this paper. In 1972, he wrote for example to Frederik Belinfante: "Not only [...] is it futile to speak of two Copenhagen schools; but it is even wrong to speak of one Copenhagen school; there has never been any such thing and I hope there will never be. The only distinction is between physicists who understand quantum mechanics and those who do not." Léon Rosenfeld to Frederik J. Belinfante, 22 Jun 1972, RP. Feyerabend argued that the vagueness of the principles defining the Copenhagen interpretation allowed its defendants "to take care of objections by *development* rather than by *reformulation*", a procedure which—he added—"serves to create the impression that the correct answer has been there all the time and that it was overlooked by the critic." Hence, according to Feyerabend, the attitude of Bohr and his followers "has very often been one of people who have the task to clear up the misunderstandings of opponents rather than to admit their own mistakes." (Feyerabend, 1964, p. 193, quoted in Home & Whitaker, 1992, pp. 258–259.) Beller (1999a, p. 191) has described the dialectical strategy of the Copenhagen scholars as "the rhetoric of finality and inevitability", arguing that they "advocated their philosophy of physics not as a possible interpretation but as the only feasible one." This attitude was often pointed out by those who, like Einstein, were dissatisfied with the Bohr–Heisenberg "religion" (Albert Einstein to Erwin Schrödinger, 31 May 1928, *apud* Murdoch, 1987, p. 101; see also Heilbron, 2001, pp. 222–223). Thus for example, in a paper that appeared in *Physics Today* in 1954, Henry Margenau (1954, p. 9) observed that Bohr's complementarity "relieved its advocates of the need to bridge a chasm in understanding by declaring that chasm to be unbridgeable and perennial; it legislated a difficulty into a norm."

[39] In one of his Dublin seminars (1949–1955), Schrödinger remarked: "Philosophical considerations about quantum mechanics have gone out of fashion. There is a widespread belief that they have become gratuitous, that everything is all right in this respect for we have been given the marvellously soothing word of *complementarity* [...]." (*Apud* Bitbol, 1996a, pp. 212–213.)

[40] See Camilleri (2008, p. 10).

end of the 1940s the "monocracy of the Copenhagen school in the philosophy of quantum mechanics" remained "almost unchallenged."[41] Einstein, who was one of the earliest and most influential critics, did not renew his attacks after the discussions on the EPR paper in the mid-1930s. Schrödinger dismissed his "wave interpretation" of 1926, and his analysis focused on the epistemic interpretation of the state vector which he regarded as the "official" one. Even de Broglie repudiated his pilot-wave theory and joined the orthodox camp.[42] In the early 1950s, however, the situation began to change. "The appearance in 1949 of the often quoted Einstein volume edited by Schilpp (1949) which contained Bohr's debate with Einstein, Einstein's self-written 'obituary' and his candid 'reply to criticisms' and which was widely read by philosophizing physicists contributed considerably to the creation of a more critical atmosphere toward the complementarity philosophy."[43] In the same period, in his Dublin seminars, Schrödinger presented his critical reflections on the orthodox view, which were subsequently developed in a series of papers that appeared in the 1950s. In these papers Schrödinger sharpened his criticisms and sketched a sophisticated philosophical framework (differing substantially from that of 1926) for his wave interpretation.[44]

In contradistinction to the previous decades, a number of physicists belonging to the new generation, who—to paraphrase John Bell—had not sat at the feet of Bohr, were sympathetic to such criticisms.[45] The social and cultural context of fundamental research had undergone deep changes following the WWII. On one hand, in the West, the intellectual environment resulting from the Americanization of research was not very favourable to the understanding of Bohr's ideas, although for the reasons highlighted in Section 2.1, this did not immediately produce a hostile attitude.[46] On the other hand, in the Soviet Union, such ideas, which had been previously tolerated, were accused of promoting idealist trends in science and were almost banished.[47] The repercussions of the Soviet polemics were enhanced by the context of the Cold War. Marxist physicists in the West were stimulated to take sides with the critics of Bohr's views. Some of them endorsed either the "stochastic" or the "statistical" interpretations, which seemed to fit the materialist framework better than complementarity.[48] However, the main challenge to the orthodox view came from David Bohm, a brilliant young physicist and American Marxist. In 1952 he proposed a hidden variable theory in which particles had well-defined (though not entirely determinable) trajectories. Such a theory challenged a famous no-go theorem stated by von Neumann (which was supposed to rule out hidden variables) and called into question the need to resort to complementarity when dealing with atomic phenomena. Bohm's theory was generally regarded with scepticism. Yet it gathered some important supporters, including Jean-Pierre Vigier, Mario Bunge, and Hans Freistadt. De Broglie himself, stimulated by Bohm's work, resumed his pilot-wave programme with renewed enthusiasm.[49]

In 1957, some of these alternative views on quantum mechanics were debated at an international conference held in Bristol. Besides Bohm, Rosenfeld and other distinguished physicists, a number of philosophers—such as Adolf Grünbaum, Norwood Hanson, and Paul Feyerabend—attended the meeting and took part in the discussions.[50] Though such discussions were probably not given much importance in Copenhagen,[51] the fact that three of the founding fathers of quantum mechanics, all of which Nobel Prize winners, had resumed their earlier criticisms could not go unnoticed.[52] As pointed out by Camilleri, "in the context of the emergence of a new threat from Bohm, de Broglie and Vigier, as well as Soviet physicists such as Blokhintsev and Alexandrov, the different schools of thought [which had been involved in the previous decades in the dispute on the true meaning of complementarity] closed ranks in identifying themselves with Bohr—the canonical author—whose writings were taken as a direct expression of the 'authentic' Copenhagen interpretation."[53] Indeed, Pauli, Heisenberg, Born and Rosenfeld all wrote papers to rebut the objections of Schrödinger and other dissenters. Bohm's work, in particular, was virulently criticised.[54]

The controversies in the first half of the 1950s revolved mainly around the possibility of providing a "causal interpretation" of quantum mechanics—possibly "completing" it with "hidden parameters". In the second half of that decade, however, the problematic aspects of measurement in quantum physics started to receive increasing attention. An important part of Heisenberg's contribution to the volume celebrating Bohr's 70th birthday, in which the author presented the Copenhagen interpretation and replied to recent criticisms, was dedicated to spelling out what Heisenberg considered to be the orthodox approach to measurement. Heisenberg quoted in particular an assertion by Lajos Janossy to the effect that, since the "reduction of wave-packets" cannot be deduced from Schrödinger's equation, there must be "an inconsistency in the 'orthodox' interpretation."[55]

The doubts raised by the "reduction of wave-packets" were certainly not new (they went back to the Fifth Solvay conference of 1927 and had been discussed for example at an international conference held in Warsaw in 1938, which both von Neumann and Bohr attended). In the 1930s and 1940s, there had been some sporadic contributions intended to clarify the puzzling aspects of von Neumann's postulate of projection. These contributions

[41] Jammer (1974, p. 250).

[42] *Ibid*, pp. 113–114.

[43] *Ibid*, p. 250. Einstein's late objections against the "orthodox view" are discussed in Howard (1985). See also Paty (1995).

[44] See Bitbol (1996a).

[45] Bell (2004, p. 271).

[46] As late as in 1970, DeWitt (1970, p. 159), in introducing what he called the "'conventional' or 'Copenhagen' interpretation", observed: "If a poll were conducted among physicists, the majority would profess membership in the conventionalist camp, just as most Americans would claim to believe in the Bill of Rights, whether they had ever read it or not."

[47] See Graham (1988).

[48] See Jammer (1974, Chapters 9 and 10). There were, however, important exceptions, like for example Rosenfeld and the Soviet physicist Vladimir Fock. About Marxism and quantum mechanics, see Freire (1997).

[49] For an elementary account of Bohm's theory, see Barrett (1999). The role played by Bohm's Marxist ideas in his search for a new interpretation of quantum mechanics is discussed in Forstner (2008). For an analysis of the reception of Bohm's proposal, see Freire (2005). A survey of the "causal interpretations" proposed in the early 1950's can be found in Scheibe (1973, p. 2). See also Jammer (1974, pp. 287–288).

[50] Körner (1957). Karl Popper, who was not able to attend, sent a written report.

[51] Rosenfeld advised Bohr not "to waste his time in reading [the proceedings of the conference]", but rather suggested that Petersen might look through them and tell him "about the worse nonsense" he would find there. (Léon Rosenfeld to Niels Bohr, 21 Oct 1957, *BSC*, reel 31.)

[52] "This comedy of errors [the attempt to develop a "theory of measurement" based on the "causal interpretation" of quantum mechanics] would have passed unnoticed, as the minor incident in the course of scientific progress which it actually is, if it had not found powerful support in the person of L. de Broglie, who is now backing it with all his authority." (Rosenfeld, 1957, *op. cit.*)

[53] Camilleri (2008).

[54] See e.g. André (1953), Born (1953), Pauli (1955). As regards the criticisms addressed to Bohm, see Freire (2005).

[55] Heisenberg (1955, p. 23). Such statements are not unusual in the literature of the 1950s. Schrödinger, for example, repeatedly criticised the collapse of the wave function (Bitbol, 1996a, p. 111): see for instance Schrödinger (1953, pp. 18–20). See also Margenau (1958), in which the objections of de Broglie are discussed (pp. 31–32). (Margenau's own criticisms went back to the 1930s.)

included a couple of works which attributed a crucial role to mental faculties such as volition and consciousness in the measuring process.[56] Far from committing himself to such approaches, Bohr put much emphasis in his writings on the fact that the physical account of measurement by no means required a *conscious* observer.[57] While it is likely that such emphasis reflected the worry that his view could be confused with what the Soviets regarded as "idealistic vagaries",[58] there is no doubt that it also expressed a deep conviction of his. The role played by the observer in the epistemological framework of complementarity was not to be understood in terms of idealistic doctrines, but rather in connection to a *pragmatic* analysis of the conditions under which one can acquire objective knowledge.[59] However, for many scholars, denying the subjectivist character of Bohr's approach amounted to dismissing at once his pragmatic analysis. Along these lines, Bohr's *functional* distinction between object-system and measuring instrument was presented as a crude *physical* assumption according to which macroscopic systems behave classically. In other words, according to this reading, Bohr's approach just split the physical world into a quantum microcosm and a classical macrocosm.[60]

In the second half of the 1950s there was a rise of studies on the measurement problem,[61] from which emerged in particular the "thermodynamic approach" developed by Günther Ludwig.[62] By treating macroscopic measuring apparatus as thermodynamic systems, such a programme purported to explain, within the framework of ordinary quantum mechanics, the fact that measurements have *definite* outcomes. After Bohr's death, those of his disciples who were committed to materialism, like Rosenfeld, saw in such a programme the possibility of providing a rigorous physical foundation for Bohr's approach, thereby dispelling the misunderstandings surrounding the alleged subjectivism of the Copenhagen view. Thus, when Wigner (1963) took up the banners of the approach which attributed a role to the observer's mind, claiming that it fitted the orthodox view of Heisenberg and von Neumann, Rosenfeld reacted by strongly supporting the theory of measurement that Adriana Daneri, Angelo Loinger and Giovanni Maria Prosperi (1962) had proposed in the framework of the thermodynamic approach.[63]

3. The genesis of Everett's thesis

3.1. Everett at Princeton

Everett enrolled himself at Princeton University in 1953, after obtaining a bachelor's degree in chemical engineering at the Catholic University of America in Washington, where he had shown exceptional mathematical ability.[64] In his first year Everett took the course of Quantum Mechanics with Robert Dicke.[65] In May 1955 he passed the general exams and undertook his doctoral research on the "Correlation Interpretation of Quantum Mechanics" under the supervision of Wheeler.

Wheeler was a prominent figure at Princeton. He had given important contributions to nuclear physics and had served in the Manhattan project. When he met Everett, at some moment between 1954 and 1955, he was just beginning to get involved in the research in cosmology. Wheeler had been acquainted with Bohr since the mid-1930s, when he had spent some time at the Institute of Theoretical Physics of Copenhagen with a Rockfeller post-doctoral fellowship.[66] In 1939, Bohr visited Princeton bringing the news of the first observations of nuclear fission, and they started a collaboration that led to the theory of fission based on the liquid drop model. They remained friends until Bohr's death.[67] In an address delivered at Princeton University in 1955, Wheeler described Bohr's complementarity as "the most revolutionary philosophical conception of our day."[68] Therefore his decision of discussing Everett's ideas with Bohr in person shows to what extent he must have been impressed by them. Indeed, Wheeler's letters prove that he held Everett in high esteem.[69]

With regard to the origin of Everett's ideas on quantum mechanics, our main source is an interview recorded at a party in 1977 (*op. cit.*). The interview is in fact an informal discussion with Charles Misner, who had done his PhD in cosmology under Wheeler in the same years as Everett. According to Everett's and Misner's recollection, the choice of the topic of Everett's thesis was influenced by the discussions which they both had with Bohr's assistant Aage Petersen, who was then visiting Princeton.[70]

[56] The first was a little book by Fritz London & Edmond Bauer (1939), and the second was a paper by Carl Friedrich von Weizsäcker (who was a close collaborator and former student of Heisenberg). See Jammer (1974, pp. 482–489).

[57] Thus, for example, in a paper of 1958, Bohr stressed that the description of atomic phenomena has "a perfectly objective character, in the sense that no explicit reference is made to any individual observer." (Bohr, 1963, p. 3.) It is worth noting that, in 1957, Fock, who had been a prominent and tenacious advocate of complementarity in the Soviet Union, visited Copenhagen and had a few conversations on the philosophical significance of quantum mechanics with Bohr. According to the Soviet commentators, Bohr's efforts to avoid any "subjectivist" ambiguity in his late writings were an outgrowth of such conversations. (Graham, 1988, pp. 311–313.)

[58] See Graham (1988). Heisenberg's epistemic interpretation of the wave function was often considered to imply a "subjectivist" view (see Stapp, 1994; Howard, 2004). Since Heisenberg was considered to be a member of the "Copenhagen school", the charge of subjectivism was sometimes extended to Bohr (see Howard, 2004; Howard discusses in particular the use of this rhetorical strategy in Popper's writings).

[59] These aspects are discussed in Section 7.

[60] See e.g. Bell (2004, pp. 188–189). A good example is provided by the celebrated course of theoretical physics of the Soviets Lev Landau and Evgenij Lifshitz (whose first edition in English, supervised by John Bell, appeared in 1958). Their account of measurement, which was traditionally considered to be quite close to Bohr's (Bell said that it was perhaps "the nearest to Bohr that we have"; *Ibid*, p. 217), postulated—in Bell's words—that macroscopic systems "spontaneously" jump into a definite macroscopic configuration which, in the case of a "classical" apparatus, corresponds to an eigenstate of the "reading" (i.e. a so-called "pointer state").

[61] See Margenau (1963) and references therein.

[62] See Jammer (1974, pp. 488–490).

[63] See Rosenfeld (1965) and the discussion of Section 7.3. For a detailed analysis of the dispute between Rosenfeld and Wigner, which went on till the early 1970s, see Freire (2007).

[64] A detailed biography of Everett is provided by Shikhovtsev (2003); see also Byrne (2007). The information about Everett's curriculum is taken from the Princeton alumni file, *GAR*.

[65] From Dicke's textbook (Dicke & Wittke, 1960) we can conjecture that the course paid little attention to interpretive issues.

[66] See Wheeler (1985, p. 125).

[67] In 1957, Bohr earned the Atoms for Peace Award. In reply to Wheeler's congratulations, Bohr wrote to him: "In these weeks I have with gratitude dwelt with many memories and not least with our cooperation through the years and your faithful friendship." (Niels Bohr to John A. Wheeler, 12 Apr 1957, *BSC*, reel 33). Bohr received the Award at a ceremony which was attended by President Eisenhower and for which Wheeler delivered an address.

[68] Wheeler (1956, p. 374); quoted in Jammer (1974, p. 74).

[69] Thus, for example, referring to the necessity to dispel the misunderstandings which could arise from Everett's work, Wheeler wrote to him: "This appallingly difficult job I feel you (among very few in this world) have the ability in thinking and writing to accomplish". And, alluding to Bohr, he added: "The combination of qualities, to accept corrections in a humble spirit, but to insist on the soundness of certain fundamental principles, is one that is rare but indispensable; and you have it. But it won't do much good unless you go and fight with the greatest fighter." (John A. Wheeler to Hugh Everett, 22 May 1956 [2nd letter], *ME*.)

[70] Everett interview, *op. cit.*, p. 9. Petersen was educated at the University of Copenhagen and became Bohr's assistant in 1952. According to Everett, he spent 1

In the interview,[71] Everett remarks that Petersen was the only one who "took seriously" the issues relating to the foundations of quantum mechanics, and in his letters to Petersen he repeatedly expresses the desire of renewing their "always enjoyable arguments."[72]

In one of his papers, Everett quotes an address delivered by Einstein (who had been working at the Institute for Advanced Studies of Princeton since 1933) in the spring of 1954.[73] On that occasion, according to Everett, Einstein had colourfully expressed his discomfort with the idea that simple acts of observation can bring about drastic changes in the universe.[74] This is a good example of the kind of atmosphere that Everett could respire at Princeton, even though the emphasis put by Misner on Einstein's seminar in the interview suggests that such occasions were in fact rare.[75] Princeton hosted some of the most distinguished experts of the foundations of quantum mechanics: John von Neumann, whose textbook was the main reference of Everett's work (see Section 4.1), was at the Institute for Advanced Studies; and Eugene Wigner was Everett's professor of Methods of Mathematical Physics at Princeton University.[76] Also, it was at Princeton that, a few years earlier, David Bohm had worked out his hidden variable theory. Everett did not meet Bohm personally, since Bohm had to leave Princeton in 1951, as a consequence of McCarthyism.[77] However, Everett's manuscripts show that he was acquainted with Bohm's work on hidden variables. Moreover, Bohm's textbook of quantum mechanics (which presented the standard formulation, but also discussed some issues of interpretation such as the measurement problem and the EPR paradox) seems to have been one of Everett's main sources for the study of the Copenhagen views on measurement (see Section 4.2).

It is reasonable to think that, in this context, a critical attitude towards the orthodox view of quantum mechanics might emerge occasionally in discussions and seminars, and that non-conventional ideas circulated more freely in Princeton than elsewhere. The very fact that Wheeler accepted the supervision of a PhD research like Everett's shows that he had an open-minded attitude with regard to such issues.[78] Indeed, 15 years earlier Wheeler had been the supervisor of Richard Feynman, who, in his PhD thesis had set the basis of the path-integral formulation of quantum mechanics.[79] Even though Everett denied having received any external input for undertaking his work,[80] in the interview he and Misner allude to the influence that Wheeler's characteristic approach to theoretical physics might have exerted on the development of the relative state formulation. Misner says: "He [Wheeler] was preaching this idea that you ought to just look at the equations and if there were the fundamentals of physics [...] you followed their conclusions and gave them a serious hearing. He was doing that on these solutions of Einstein's equations like Wormholes and Geons". And Everett replies: "I've got to admit that that is right, and might very well have been totally instrumental in what happened."[81]

The analysis of Everett's early writings does not indicate that his search for an original approach to quantum mechanics was inspired by issues of cosmology. Yet, there is little doubt that Wheeler's interest in Everett's ideas was enhanced by his recent involvement in that area of research. This is mostly apparent from the final version of the dissertation, in the drafting of which Wheeler took an important part. Therefore, if Everett's ideas received some attention when they were first put forward, this might be partly due to the circumstance that, at the time, Princeton was in the small minority of places in the US at which physicists were interested in general relativity and cosmology. (As we will see, Bryce DeWitt, who was a friend of Wheeler's and the head of the cosmology group of the University of North Carolina at Chapel Hill, was to play a crucial role in the diffusion of Everett's ideas.)

3.2. The steps towards the dissertation

Everett's dissertation *On the Foundations of Quantum Mechanics* (Everett, 1957a) was submitted in March 1957. Except for the abstract[82] and a few minor stylistic alterations, the dissertation is identical to the paper published in July 1957 in the *Reviews of Modern Physics*, with the title *"Relative State" Formulation of Quantum Mechanics* (1957b). It is a rather small manuscript (36 pages), which was written in the winter of 1956–1957. In an introductory note, Everett mentions "an earlier less condensed draft of the present work, dated January 1956", which he says "was circulated to several physicists".[83] There is good evidence that the longer draft "circulated to several physicists", whose title was *Wave Mechanics Without Probability*,[84] was very similar, if not identical, to a paper of 137 pages published many years later

(*footnote continued*)
year in Princeton (Hugh Everett to Max Jammer, 19 Sep 1973, ME). This occurred probably in 1954–1955, because Petersen accompanied Bohr when Bohr visited Princeton in the autumn of 1954 (see Section 6). (Felicity Pors, priv. comm., 16 Oct 2007.)

[71] *Ibid*, p. 10.

[72] Hugh Everett to Aage Petersen, 31 May 1957, *WP* (Series I—Box Di—Fermi Award #1—Folder Everett). See also Hugh Everett to Aage Petersen [draft], summer of 1956, *ME*.

[73] Everett (1973, p. 116). Wheeler (1979b, p. 184) recalled: "We persuaded him [Einstein] to give a seminar to a restricted group. In it the quantum was a central topic."

[74] According to Everett's recollection, Einstein said that he "could not believe that a mouse could bring about drastic changes in the universe simply by looking at it". However, the quotation might have been reported to Everett by others, since in his 1977 interview (*op. cit.*, p. 4) he did not remember having attended the seminar.

[75] Everett interview, *op. cit.*, p. 4. Wheeler (1979b) reported a few occasions when he and Einstein discussed issues of fundamental physics. In May 1953, for example, Einstein invited Wheeler and his students to his home for tea and answered questions about his view of quantum mechanics.

[76] Von Neumann and Wigner were not directly involved in the public debate on the interpretation of quantum mechanics in the 1950's. However, von Neumann's persistent concern with the epistemological issues raised by quantum mechanics is borne out by the efforts he devoted to the revision of the English translation of his book (Freire, 2005, p. 27). See also Rédei & Stöltzner (2001), and, with regard to von Neumann's opinion on Bohm's proposal, Stöltzner (1999). As for Wigner, his dissatisfaction with Bohr's complementarity predated his involvement in the debates of the 1960's (Freire, 2007; Camilleri, 2008). Interestingly, in the notes taken by Wheeler in Copenhagen in 1956 (John A. Wheeler, *Notes taken in Copenhagen*, 3 May 1956, *ME*), Aage Petersen refers to von Neumann's theory of measurement as "von N[eumann]+Wig[ner]" "stuff".

[77] See Olwell (1999), Freire (2005).

[78] In the interview with Everett (*op. cit.*, p. 5), Misner says: "You probably already had these quantum mechanical ideas and just needed someone to talk to about them and he [Wheeler] was obviously the kind of person who..."

[79] Feynman might have read some version of Everett's dissertation (or might have been informed about it by Wheeler), since at the beginning of 1957 he already knew the general lines of Everett's work (see Section 6).

[80] As we will see in Sections 4 and 5, two important sources of inspiration for Everett's work were the hidden variable theories on the one hand, and Schrödinger's "wave interpretation" on the other. Schrödinger was sent a preprint of Everett's paper in 1957, but, in so far as we know, he did not reply.

[81] Everett interview, *op. cit.*, pp. 9–10.

[82] See Barrett (1999, p. 65).

[83] Everett (1957a, p. 1).

[84] See Alexander Stern to John A. Wheeler, 20 May 1956, *ME*; Wheeler, *Notes*, 1956, *op. cit*; Hip J. Groenewold to Hugh Everett & John A. Wheeler, 11 Apr 1957, *ME*.

(in 1973) as *The Theory of the Universal Wave Function*.[85] Henceforth we will refer to this paper as the "long thesis".[86]

The documentary material that will be discussed in the following sections indicates that the manuscript that was read in Copenhagen (*Wave Mechanics Without Probability*) was the *second* version of the thesis.[87] This does not necessarily imply that a *first* structured version, differing substantially from the long thesis, actually existed.[88] Nevertheless, the hypothesis that the bulk of the long thesis had already been worked out early in 1955 is supported by the analysis of both the original manuscript and a few unpublished papers.[89] Besides a small paper entitled *Objective vs Subjective probability* (Everett, 1955a)[90], which outlines the "Wigner's friend"-type argument that forms the core of Everett's critique of the standard formulation in the long thesis,[91] the archives contain two manuscripts which were probably written in the summer of 1955 (see Section 6). One of them, *Quantitative Measure of Correlation* (Everett, 1955b), summarises the mathematical results of the second chapter of the long thesis (on correlation theory). The other (Everett, 1955c) is a short paper (9 pages) whose title *Probability in Wave Mechanics* suggests a close relationship with the second version of the thesis. Indeed, this paper expounds all the relevant results concerning the interpretation of quantum mechanics that one finds in the long thesis.[92] Even though the presentation is made in a non-technical language devoid of formulas, it seems unlikely that Everett had reached all his conclusions without relying on a formal analysis. Therefore, by the summer of 1955, Everett had probably already outlined both the mathematical and the conceptual framework of his approach.[93] In the light of this reconstruction, one can understand why Everett, who we know had continued to work "madly" on the draft to be sent to Copenhagen until Wheeler's departure to Europe in April 1956,[94] in later recollections always stated that the long thesis had been written in 1955.[95]

Here is a tentative chronology of the thesis versions and of the related papers:

(1a) *Objective vs Subjective probability*, short manuscript (first half of 1955).
(1b) *Quantitative Measure of Correlation*, short manuscript (summer 1955).
(1c) *Probability in Wave Mechanics*, short manuscript (summer 1955).
(2) *Wave Mechanics Without Probability*, second version of the dissertation (the long thesis) (winter 1955–1956), published as *The Theory of the Universal Wave Function* (1973).
(3) On the Foundations of Quantum Mechanics, final dissertation (winter 1956–1957), published as "Relative State" Formulation of Quantum Mechanics (July 1957).

4. The reasons for Everett's discontent

4.1. Standard formulation

Everett's proposal stems from his dissatisfaction with von Neumann's formulation of quantum mechanics—"the more common (at least in this country) form of quantum theory", as he says in a letter to Petersen.[96] Both of Everett's published papers contain explicit references to von Neumann's *Mathematical Foundations*, whose English translation appeared in 1955, exactly when Everett's ideas were taking shape.[97] A central assumption in Everett's understanding of the standard formulation is that the state vector mirrors the *physical* state of a system (i.e. its putative *objective properties*[98]). Based on this hypothesis, von Neumann's account of observation can be regarded as involving a *physical* process in which the state of the observed system undergoes in general an acausal transition (from a superposition of eigenstates of the measured observable to *the* specific eigenstate corresponding to the observed value).[99] Such a process, whose outcomes can

[85] The paper was published in a collective volume edited by DeWitt & Graham (1973). There is a letter from Everett to Jean-Marc Lévy-Leblond (15 Nov 1977, *EP*) which seems to support the hypothesis that the title of the original manuscript was indeed changed in the process of publication.

[86] A copy of the long thesis was sent to Copenhagen in April 1956, and a second one seems to have followed a few weeks later (Everett to Petersen [draft], 1956, *op. cit.*). We were unable to locate either. However, a draft of the long thesis is deposited in the *EP* archive (Everett, 1956). It contains some handwritten corrections which were incorporated in the paper published in 1973. The *EP* manuscript lacks the cover (hence we can only guess its title). However, a cover with the title *Wave Mechanics Without Probability*, which might have belonged to the *EP* manuscript, was unearthed by Peter Byrne among the papers in possession of Everett's son. If the *EP* manuscript is the one that Everett sent to DeWitt in 1971 (after removing the cover, in which there appeared a title that Wheeler found inappropriate; John A. Wheeler to Niels Bohr, 24 Apr 1956, *BSC*, reel 34; also in *WP*, Series I, Box Boh-Bu, Folder Bohr, N. #2), this would explain why the title of the version published in DeWitt & Graham (1973) differed from the original.

[87] See John A. Wheeler to Hugh Everett, 22 May 1956 [1st letter], *WP* (Box Di-Fermi #2, Folder Everett); Wheeler to Bohr, 24 Apr 1956, *op. cit.*; John A. Wheeler to Allen Shenstone, 28 May 1956, *WP* (Box Di-Fermi #2, Folder Everett); Groenewold to Everett & Wheeeler, *op. cit.*; Aage Petersen to Hugh Everett, 28 May 1956, *ME.*

[88] The archives contain no document that may correspond to such a first version. However, the recent discovery of some folders containing Everett's personal papers (Byrne, 2007) may hopefully provide further insight into the very first steps of Everett's doctoral research.

[89] For example, as pointed out by DeWitt (DeWitt interview, *op. cit.*, p. 6), the first draft of the last chapter of the long thesis was probably written prior to Einstein's death (April 1955), since Einstein is referred to as if he were still alive (Everett, 1973, p. 112). Admittedly, the long thesis contains references to three books published in 1955, one of which (von Neumann's *Mathematical foundations*) is also extensively quoted. Yet in the original manuscript of the long thesis deposited in the *EP* archive (Everett, 1956), the quotations from von Neumann's book appear to have been added later. Moreover, the reference to a paper that appeared in an issue of the *Supplemento al Nuovo Cimento* printed on 22 November 1955 lacks the volume and page number (they were added in the version published in 1973), which suggests that Everett read the pre-print.

[90] Everett's unpublished manuscripts are included in the final list of references.

[91] The opening sentence of this manuscript ("Since the root of the controversy over the interpretation of the formalism of quantum mechanics lies in the interpretation of the probabilities given by the formalism, we must devote some time to discussing these interpretations"; Everett, 1955a, p. 1) suggests that it was— or was intended to be —part of a larger work. Indeed, the structure of the paper resembles that of the introduction of the long thesis, although the projection postulate is not given the same central place. Moreover, in this early manuscript, Everett's own proposal is not mentioned.

[92] In particular, the "emergence" of objects from correlations is discussed by means of an example which is reproduced almost literally on p. 86 of the long thesis.

[93] Interestingly, in Everett (1956), the chapter on Observation, which forms the core of Everett's proposal, appears to have been imported from an earlier (and arguably shorter) manuscript (witness the old numbering of pages which appears in the upper margin).

[94] Nancy Gore Everett, Diary, entry of 28 Mar 1956, *ME* (Peter Byrne, priv. comm.). Nancy was Everett's wife.

[95] Everett interview, *op. cit.*, p. 6; Hugh Everett to Jean-Marc Lévy-Leblond, 15 Nov 1977, *EP*; Everett to Raub, 1980, *op. cit.*; Hugh Everett to Bill Harvey, 20 Jun 1977, *EP*, Series I-8. According to the recollection of Everett's wife, who typed the manuscript (Everett interview, *op. cit.*, p. 6), the thesis was written in the winter of 1954–1955 (Nancy Gore Everett, *Calendar of events*, *EP*, Box 1, Folder 1). (But this information could simply be inaccurate: the manuscript that Nancy Everett had in mind might actually be the *second* version, which was written in the winter of 1955–1956.)

[96] Everett to Petersen, 1957, *op. cit.*

[97] Everett probably had a working knowledge of German, and might have read von Neumann's book in the original.

[98] Everett (1973, p. 63).

[99] This reading of von Neumann has been thoroughly criticised by Becker (2004). The way to understand the postulate of projection changes depending on one's interpretation of the state vector. The interpretation that Everett seems to take as the "conventional" one is not inconsistent with that which seems to underlie some statements made by "orthodox" scholars (for example Dirac's assertion that "the theory describes the state of the world at any given moment by

be statistically predicted using the Born rule, is considered to be responsible for the probabilistic features of quantum phenomena.[100] Unlike other critics of the postulate of projection,[101] therefore, Everett does not regard the collapse of the wave function as a formal trick, which the epistemic construal of the state vector requires in view of the intrinsic indeterminism of the quantum phenomena. Rather, he believes that in the standard formulation, the collapse of the wave function is what *prescribes* the probabilities of the various possible outcomes.[102] According to him, therefore, the postulate of projection instantiates a particular interpretation of quantum indeterminism, namely that of "objective chance". Although there are no grounds for endorsing or rejecting such an interpretation a priori, Everett contends that the odd implications of the projection postulate compel us to look for an alternative in which the probabilistic features of quantum mechanics can be understood in terms of "subjective chance".[103]

What Everett finds disturbing in the projection postulate is, first of all, the artificial way in which it splits the dynamics of the theory. It appears to be "a 'magic' process in which something quite drastic [occurs] (collapse of the wave function), while in all other times systems [are] assumed to obey perfectly natural continuous laws'".[104] The *ad hoc* nature of the projection postulate is borne out by the fact that, being designed to account for *idealized* observations, it is unsuited to deal with realistic models of the measurement interaction.[105] More generally, if one tries to understand measurements as just a physical interaction occurring between measuring apparatus and systems, the theory "leaves entirely unknown" *which* interactions are to be regarded as measurements.[106] Everett illustrates the consequences of this situation by means of a Wigner's-friend-type argument (see Section 3.2), from which he infers that a consistent application of the projection postulate within the standard theory implies the commitment to solipsism, i.e. to the hypothesis that there is only *one* observer in the universe who is responsible for the "collapse" of the state of observed systems.[107] Everett sees basically two ways to avoid this conclusion. Either one denies that measurement interactions fall into the domain of applicability of microphysics, or one postulates that the quantum description is simply incomplete, and must be supplemented with hidden parameters that can also characterise measurements. Both these solutions are at variance with the idea that the state vectors provide a complete model of the world, an idea to which Everett is strongly committed.

4.2. Dualistic approach

The first way to avoid the alleged paradoxes of the standard formulation is to assume that "not every physical system possesses a state function, i.e. that even in principle quantum mechanics cannot describe the process of measurement itself." Everett considers this option "somewhat repugnant, since it leads to an artificial dichotomy of the universe into ordinary phenomena, and measurements."[108] In the long thesis he gives a further reason for rejecting this view, namely that it "does violence to the so-called principle of psycho-parallelism" stated by von Neumann.[109]

In the introduction of the long thesis, Everett makes a distinction between this view and Bohr's. After outlining the former approach together with other possible solutions, he says: "We have omitted one of the foremost interpretations of quantum theory, namely the position of Niels Bohr."[110] He discusses the latter in the conclusion, but then one gets the impression that, in Everett's eyes, the Copenhagen interpretation (which is the label he uses to denote what he takes to be Bohr's approach[111]) is closely related to the dualistic view presented earlier.[112] The criticisms he addresses to the Copenhagen interpretation in the long thesis[113] are summarised and developed in a letter to Petersen of May 1957, in which he says that, while his paper of 1957 addresses "mostly" von Neumann's formulation, he finds Bohr's approach "even more unsatisfactory", although "on quite different grounds."[114] The main objections appearing in the letter of 1957 are similar to those raised in the long thesis of 1955–1956. (Incidentally, this shows that Everett had not changed his mind-notwithstanding the fact that, for reasons on which we will return, his criticisms do not appear in the final version of the dissertation.) What Everett finds "irritating" in the Copenhagen interpretation is on the one hand 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 measurement processes)", and, on the other hand, the "strange duality of adhering to a 'reality' concept for macroscopic physics and denying the same for the microcosm."[115]

(*footnote continued*)
a wave function." Institut Solvay, 1928, p. 256.) See Bitbol (2000, pp. 72–83) for a discussion.

[100] In a letter of 1973 to Max Jammer (*op. cit.*), Everett identifies the "probability interpretation of quantum mechanics" with the assertion that "somehow the measuring process [is] 'magic' and subject to a separate axiom governing the collapse of the wave function."

[101] See e.g. Bohm (1952), Margenau (1958), Schrödinger (1953, 1958).

[102] Everett (1957b, p. 142).

[103] Everett (1955a).

[104] Everett to Jammer, 1973, *op. cit.*

[105] Everett (1973, pp. 100–103).

[106] Everett (1955a, p. 4).

[107] The argument, which came subsequently to be known as the "Wigner's friend" paradox, appeared in a paper of Wigner's dated 1961. Given the resemblance between Wigner's and Everett's formulation, one may wonder whether Wigner picked up the argument from Everett's thesis, which he might have read. (However, of course, the converse might also be true, i.e. Everett might have been inspired by discussions with Wigner.) In a paper of 1958 (pp. 168–169), Schrödinger alludes to the same argument: "But jokes apart, I shall not waste the time by tritely ridiculing the attitude that the state-vector (or wave function) undergoes an abrupt change, when 'I' choose to inspect a registering tape. (Another person does not inspect it, hence for him no change occurs.) The orthodox school wards off such insulting smiles by calling us to order: would we *at last* take notice of the fact that according to them the wave function does not indicate the state of the physical object but its relation to the subject; this relation depends on the knowledge the subject has acquired, which may differ for different subjects, and so must the wave function." This ironical presentation of the problem suggests that, had Schrödinger read the pre-print of Everett's paper that he was sent by Wheeler, he would have found Everett's arguments quite naïve. Nevertheless, Schrödinger was opposed to the epistemic interpretation of the state vector and he believed, like Everett, that "the Kopenhagen epistemology [...] leads to the physics of solipsism." (*Ibid.*)

[108] Everett (1955a, p. 3).

[109] Everett (1973, p. 7). The principle was stated by von Neumann (1955, p. 418) in the following terms: "[...] it must be possible so to describe the extra-physical process of the subjective perception as if it were in reality in the physical world—i.e. to assign its parts equivalent physical processes in the objective environment, in ordinary space."

[110] Everett (1973, p. 8).

[111] Everett found the term "Copenhagen interpretation" in the above mentioned book edited by Pauli (1955), which is cited in the long thesis.

[112] The introduction and the conclusion of the long thesis were arguably written at different times. The first and third "interpretations" outlined in the conclusion are explicitly put into correspondence with the first and fourth "alternatives" appearing in the introduction (solipsism and hidden variables respectively). Everett avoids emphasising the correspondence between the second interpretation (Copenhagen) and the second alternative (dualistic view), but it is quite clear that he sees a link between them.

[113] Everett (1973, p. 111).

[114] Everett to Petersen, 1957, *op. cit.*

[115] *Ibid.* It is instructive to recall the discussion about the "relationship between Quantum and Classical concepts" which Everett found in Bohm's textbook. In his presentation of the orthodox view, Bohm said that "in order to obtain a means of interpreting the wave function, we must [...] *at the outset*

In the letter to Petersen, Everett develops his critique, pointing out other alleged deficiencies of the Copenhagen approach:

> You talk of the massiveness of macrosystems allowing one to neglect further quantum effects (in discussions of breaking the measuring chain), but never give any justification for this flatly asserted dogma. Is it an independent postulate? It most certainly does *not* follow from wave mechanics [...]. In fact, by the very formulation of your viewpoint you are totally incapable of any justification and *must* make it an independent postulate—that macrosystems are relatively immune to quantum effects.
>
> You vigorously state that when apparatus can be used as measuring apparatus then one cannot simultaneously give consideration to quantum effects—but proceed blithly to apply [the uncertainty relations] to such devices, tacitly admitting quantum effects.

Furthermore, Everett claims that while the Copenhagen interpretation takes "the fundamental irreversibility of the measuring process" to be what "allows the destruction of phase relations and make possible the probability interpretation of quantum mechanics", "there is nowhere to be found any consistent explanation of this 'irreversibility'." And he concludes: "Another independent postulate?"

In the light of these criticisms one may find surprising Everett's assertion, stated elsewhere, that the Copenhagen interpretation is "undoubtedly safe from contradiction."[116] Indeed, Everett is prepared to concede that the Copenhagen interpretation avoids inconsistency, but he believes that this is achieved at the cost of endorsing a strongly dualistic approach. Such an approach is at odds with the task of providing a coherent and all-inclusive *model* of the world, which is, for Everett, the very goal of physics. Hence, the Copenhagen interpretation is to him "hopelessly incomplete."[117]

The final version of the dissertation, in which Everett criticises what he calls the "external observation formulation", contains a remark which can be interpreted as a further objection to the Copenhagen interpretation. As we will see, the label "external observation formulation" denotes a dualistic approach in which the state reduction is brought about by an "external" observer that cannot in principle be described by the formalism. Such a view is clearly reminiscent of the one that Everett associated with the Copenhagen interpretation (and this association is indeed made explicit by Wheeler[118]). The question of whether the *pragmatic* aspects of Bohr's view, and in particular his *functional* distinction between measuring apparatus and object system, can really be expressed in the dualistic terms of the external observation formulation is postponed to Section 7. Certainly Wheeler and Everett thought that they could, and interpreted Bohr's remarks on the necessity to frame the quantum predictions in a well-defined experimental context as implying that von Neumann's measurement chain needed to be "cut" into two parts, one of which could not be described by quantum mechanics. This view, they argued, led to critical problems "in the case of a closed universe", since then "there is no place to stand outside the system to observe it. There is nothing outside it to produce transitions from one state to another."[119] The external observation formulation appears thus unsuited to providing a description of the *whole* universe; and this, in turn, precludes any possibility of a synthesis with general relativity.[120]

4.3. Hidden variables

From the manuscript *Objective vs Subjective Probability*, it is clear that at first Everett regarded hidden variable theories as a promising approach to overcome the paradoxes of the standard formulation. In later writings, he still acknowledges their "great theoretical importance" and undisputable appeal, but he emphasises that they are unnecessarily "cumbersome and artificial" as compared to his own proposal.[121]

Bell has pointed out some structural analogies between Everett's and the hidden variable approaches.[122] Indeed the conceptions of physical theories which underlie the two approaches are closely related to each other, and so are the strategies adopted to fit the quantum indeterminism into them. Like the hidden variable theorists, Everett held that theories must supply an exhaustive model of the world, including observers and measurement interactions,[123] although, unlike them, he believed that the state vectors alone can provide such a model. Everett claimed that the indeterministic features of quantum phenomena only appear within *subjective* experience. According to him, this point of view was similar to that adopted by the advocates of hidden variables, for whom "the probabilities occurring in quantum mechanics are *not* objective" since "they correspond to our ignorance of some hidden parameters."[124] However, Everett's proposal did not stem from an aprioristic commitment to determinism.

> From my point of view there is no preference for deterministic or indeterministic theories. It is quite conceivable that an adequate *stochastic* interpretation could be developed [...] where the fundamental processes of nature are pictured as stochastic processes *whether or not* they are undergoing observation. I only object to mixed systems where the character changes with mystical acts of observation.[125]

In the long thesis, following Schrödinger (1952), Everett nonetheless criticised the stochastic interpretations because of their "desire to have a theory founded upon particles", while it seems "much easier to understand particle aspects from a wave picture [...] than it is to understand wave aspects [...] from a particle picture."[126]

More generally, Everett seemed to agree with Schrödinger that "the demand for a non-subjective description is inevitable, of course without prejudice whether it be deterministic or otherwise."[127] If Everett is so concerned with probability (think of the titles of his earliest manuscripts) this is because, for him, probabilities arise within the conventional formulation as a

(*footnote continued*)
postulate a classical level in terms of which the definite results of a measurement can be realized." He also asserted that "classical concepts cannot be regarded as limiting forms of quantum concepts", and that "without an appeal to a classical level, quantum theory would have no meaning". (Bohm, 1951, pp. 624–626.)

116 Everett (1973, p. 111).

117 Hugh Everett to Bryce S. DeWitt, 31 May 1957, courtesy of Eugene Shikhovtsev.

118 Wheeler (1957, p. 151).

119 Everett (1957b, p. 142).

120 Interestingly, such an objection is not mentioned in Everett's letter to Petersen, though the letter was written *after* the paper. This suggests that this objection reflected in fact a concern of Wheeler's.

121 Everett (1973, p. 113); Everett to DeWitt, 1957, *op. cit.*

122 Bell (2004, pp. 93–99) made a comparison between Everett's approach and de Broglie's pilot wave theory. See also Barrett (1999, Chapter 5). This point is discussed by DeWitt in a letter sent to Wheeler and Everett in 1957. (Bryce S. DeWitt to John A. Wheeler & Hugh Everett, 7 May 1957, *WP*, Series I—Box Di—Fermi Award #1—Folder Everett).

123 See e.g. Körner (1957, p. 61).

124 Everett (1955a, p. 4).

125 Everett to DeWitt, 1957, *op. cit.*

126 Everett (1973, p. 114).

127 Schrödinger (1958, p. 162).

consequence of state reduction, and state reduction requires in turn the intervention of an external observer, thereby undermining the very possibility of an objective description.

5. Everett's project

5.1. A unitary model of the world

Everett outlines his conception of theories in an appendix of the long thesis. The relationship between such a conception and his formulation of quantum mechanics is discussed in a letter to DeWitt, some passages of which are quoted in a note added in proof to the paper published in 1957.

> To me, any physical theory is a logical construct (model), consisting of symbols and rules for their manipulation, *some* of whose elements are associated with elements of the perceived world.[128]

The "perceived world" or "world of experience" is to be understood as "the sense perceptions of the individual, or the 'real world'—depending upon one's choice in epistemology." As to *his* choice, Everett is quite reticent. His theory deals ultimately with the content of the observers' memories. However, he proposes to identify the "subjective knowledge (i.e. perceptions)" of the observers with "some objective properties (states)" of theirs.[129]

Remarkably, all throughout Everett's writings, the terms "real" and "reality" (as well as "actual", "branching process", "branches") appear systematically in quotes. Indeed, Everett emphasises that the meaning of terms such as "reality" ought to be understood on the basis of their usage in scientific *practice*.[130]

> When one is using a theory, one naturally pretends that the constructs of the theory are "real" or "exist". If the theory is highly successful (i.e. correctly predicts the sense perceptions of the user of the theory) then the confidence in the theory is built up and its constructs tend to be identified with "elements of the real physical world". This is however a purely psychological matter. No mental construct (and this goes for everyday, prescientific conceptions about the nature of things, objects, etc. as well as elements of formal theories) should ever be regarded as more "real" than any others. We simply have more *confidence* in some than others.[131]

In the long thesis, the point is illustrated by the following example:

> The constructs of classical physics are just as much fictions of our minds as those of any other theory we simply have a great deal more confidence in them. It must be deemed a mistake, therefore, to attribute any more "reality" here than elsewhere.[132]

Everett's attitude shows some analogy with Schrödinger's "methodological realism" or "quasi-realism", in which any naïve metaphysical commitment is explicitly rejected.[133] Although Everett holds that the primary purpose of theoretical physics is to build useful models, he does not bother about their ontological status, since, he says, models "serve for a time and are replaced as they are outworn."[134] This attitude is also apparent in Everett's critique of Bohr's doctrine of classical concepts (which we shall discuss in detail in Section 7). Far from attributing any special status to classical concepts, Everett urged their replacement by quantum ones. This position was not based on ontological considerations. Rather, Everett thought that since *all* concepts serve to deal with a *reality-in-quotes*, there is no reason to stick to a particular set of concepts: our concepts can evolve just as our models of "reality" do.[135]

The "conceptual model of the universe" that Everett proposes "postulates only the existence of the universal wave function which obeys a linear wave equation."[136] In such a theory, "one can regard the state functions themselves as the fundamental entities, and one can even consider the state function of the whole universe."[137] In one of the manuscripts of 1955, Everett put it as follows: "The physical 'reality' is assumed to be the wave function of the whole universe itself."[138] In the long thesis, comparing his programme to the existing interpretations of quantum mechanics, Everett explicitly refers to a paper in which Schrödinger contrasts the continuous description provided by the wave function with the "quantum jumps" of the "current probability view".[139] Indeed, in Schrödinger's writings of this period, one can easily find passages which are amazingly in tune with Everett's views:

> [...] at the present stage and as long as the state vector plays the role it does it must be taken to represent 'the real world in space and time', it ought not to be sublimed into a probability function for the purpose of making forecasts [...] changing abruptly when somebody (who?) cares to inspect a photograph or a registering tape.[140]

What Everett has in mind when he talks of "model" is an "objective description" of "reality". Such a description must leave no room for mental entities and processes which exceed the boundaries of quantum physics.[141] In accordance with von Neumann's principle of psycho physical parallelism, which Everett interprets as implying that an observer (including their perceptions) is completely characterised by her/his physical state, the observers and their mental states must be described by a state vector. The universal wave function includes therefore an exhaustive model of all existing observers and of their interactions with the observed systems. This is perhaps why Everett contends that, unlike the conventional formulation, his theory "sets the framework for its interpretation".[142] In the methodological appendix of the long thesis, Everett says that each theory must contain an "interpretive part", i.e. "rules which put some of the elements of the formal part into correspondence with the perceived world."[143] Thus one might possibly argue that the universal wave function "sets the framework for its interpretation" because it is isomorphic to the "world" perceived by all observers (inasmuch as it mirrors the properties of the observers' brains which correspond to their "subjective perceptions").[144] From Everett's standpoint, the same cannot be said of the conventional formulation, in which "pure wave mechanics" must be supplemented with the postulate of projection if one wants to put the symbolism (which, in general, describes a system by

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[128] Everett to DeWitt, 1957, *op. cit*.
[129] Everett (1973, p. 63).
[130] See for example Everett (1973, p. 116).
[131] Everett to DeWitt, 1957, *op. cit*.
[132] Everett (1973, p. 134).
[133] See Bitbol (1998, pp. 182–184).
[134] Everett (1973, p. 111).
[135] For a comparison with the debate that Schrödinger had with Bohr on this issue, see Murdoch (1987, p. 101), Bitbol (1996a, pp. 22–23).
[136] Everett (1973, p. 117).
[137] *Ibid*, p. 9.
[138] Everett (1955c, p. 9).
[139] The paper cited by Everett (1973, p. 115) is Schrödinger (1952).
[140] Schrödinger (1958, p. 169).
[141] This is explicitly stated in a letter of 1980 (Everett to Raub, 1980, *op. cit.*)
[142] Everett (1957b, p. 142). See also Wheeler (1957, p. 152).
[143] Everett (1973, p. 133).
[144] To be sure, this point of view is quite problematic. Its meaning and implications are analysed in the following subsections.

means of a *superposition* of "absolute" states) into correspondence with the "perceived world" (in which the system is described by a *single* element of the superposition).[145]

Everett is committed to an ideal of unity, simplicity and completeness.[146] The structural features of his theory reflect this commitment. Firstly, there is no dualism in the dynamics: the projection postulate is relinquished and the universal wave function evolves according to a continuous and deterministic process. Secondly, this simplification is purportedly achieved without introducing supplementary "artificial" variables (see Section 4.3).

5.2. Objective description and correlations

While Everett's motives, goals and assumptions are similar to those of other critics of the conventional formulation of quantum mechanics, his strategy to make a descriptive interpretation of the theoretical symbolism viable is completely original. The cornerstone of this strategy is what Everett names "the fundamental principle of the relativity of states". Suppose that the universal wave function is expanded as a linear combination of the vectors of some basis. According to the principle of the relativity of states, if, in a given component of this expansion, a system is represented by the eigenvector of an observable A corresponding to the eigenvalue a_i, then the system can be said to *have* the property "$A = a_i$" (i.e. to *be* in the corresponding state), but this assertion is true only *relative to* the properties that the other systems "have" in the same component of the expansion (i.e. to their state in that component).

On one hand, in virtue of the principle of the relativity of states, the state vectors need no longer to undergo an abrupt, acausal change in order to provide a consistent *description* of the properties which measurements are supposed to reveal.

> From the viewpoint of the theory, all elements of a superposition (all "branches") are "actual", none any more "real" than another. It is completely unnecessary to suppose that after an observation somehow one element of the final superposition is selected to be awarded with a mysterious quality called "reality" and the others condemned to oblivion. We can be more charitable and allow the others to coexist—they won't cause any trouble anyway because all the separate elements of the superposition ("branches") individually obey the wave equation with complete indifference to the presence or absence ("actuality" or not) of the other elements.[147]

On the other hand, properties are now intrinsically "relative":

> All statements about a subsystem [...] become *relative* statements, i.e. statements about the subsystem relative to a prescribed state for the remainder.[148]

In this way Everett thinks that he has managed to construe the quantum theory as an "objective description", although of course the description is objective not in the sense that it captures *the* "actual value" of each observable, but because it provides a symbolic structure which connects *any* possible value of a given observable to a particular state of the whole universe (which includes a specific state of every conceivable observer).[149] What quantum mechanics describes are the *correlations* occurring in nature.[150]

Everett argues that, in this framework, even *objects* should be understood in terms of correlations, no matter whether their size is atomic or macroscopic:

> [If we] consider a large number of interacting particles [...], throughout the course of time the position amplitude of any single particle spreads out farther and farther, approaching uniformity over the whole universe, while at the same time, due to the interactions, strong correlations will be built up, so that we might say that the particles have coalesced to form a solid object. That is, even though the position amplitude of any single particle would be "smeared out" over a vast region, if we consider a "cross section" of the total wave function for which one particle has a definite position, then we immediately find all the rest of the particles nearby, forming our solid object.[151]

As an example, Everett analyses the formation of a hydrogen atom in a box containing a proton and an electron. He concludes that:

> What we mean by the statement, "a hydrogen atom has formed in the box", is just that this correlation has taken place—a correlation which insures that the *relative* configuration for the electron, for a definite proton position, conforms to the customary ground state configuration.[152]

(This example is also discussed in the manuscript *Probability in Wave Mechanics* of 1955, in which one finds the same emphasis on correlations, though the notion of "relative state" is not yet explicitly stated there.) More generally, Everett claims that "all [physical] laws are correlation laws".[153] These passages help us to understand how Everett can claim that his "universal wave function model" is complete, notwithstanding the fact that it contains no information about *which* branch represents "actuality". Indeed, from Everett's point of view, such a question is not one that can or must be answered by physics, for the simple reason that it cannot be formulated in terms of correlations. In 1957, Everett wrote to Norbert Wiener:

> You also raise the question of what it means to say that a fact or a group of facts is actually realized. Now I realize that this question poses a serious difficulty for the conventional formulation of quantum mechanics, and was the main motives for my reformulation. The difficulty is removed in the new formulation, however, since it is quite unnecessary in this theory ever to say anything like "Case A is actually realized."[154]

Thus Everett can consistently hold that his model provides a complete description of "reality". There remains a crucial

[145] This reasoning assumes that, in the conventional formulation, there is a straightforward link between state vectors and physical states. As we have seen, this assumption was part of Everett's reading of von Neumann's formulation.

[146] "We have a strong desire to construct a single all-embracing theory which would be applicable to the entire universe." (*Ibid*, p. 135.)

[147] Everett to DeWitt, 1957, *op. cit.*

[148] Everett (1973, p. 118).

[149] In this case too, it is interesting to compare Everett's position to Schrödinger's. Commenting on our "yearning for a complete description of the material world in space and time", Schrödinger (1958, p. 169) remarked: "[...] It ought to be possible, so we believe, to form in our mind of the physical object an idea (Vorstellung) that contains in some way everything that *could be* observed in some way or other by any observer, and not only the record of what *has been* observed simultaneously in a particular case."

[150] Everett's mathematical work on correlations was probably undertaken independently of his reflection on quantum mechanics. Indeed, the chapter of the long thesis dedicated to correlation theory contains a lot of mathematical details that are not essential to the remainder. The chapter on correlation theory was not reproduced in the final dissertation. However, it gave rise to a paper (Everett, 1955b), which remained unpublished (albeit Wheeler considered it "practically ready" for submission; John A. Wheeler to Hugh Everett, 21 Sep 1955, *EP* (Box 1, Folder 9)).

[151] Everett (1955c, p. 6).

[152] Everett (1973, p. 86).

[153] See Everett (1973, pp. 118; 137).

[154] Hugh Everett to Norbert Wiener, 31 May 1957, *ME*.

problem, however, to be solved "investigat[ing] the internal correlations in the universal wave function"[155], namely, how to put this description into correspondence with the correlations that *we* observe. As we will now see, for Everett even this problem can be settled without singling out a unique "actual" branch.

5.3. Subjective experience and probabilities

How does Everett's theory account for the "perceptions" of a typical observer engaged in experimental activity?

> For this purpose it is necessary to formulate abstract models for observers that can be treated within the theory itself as physical systems, to consider isolated systems containing such model observers in interaction with other subsystems, to deduce changes that occur in an observer as a consequence of interaction with surrounding subsystems, and to interpret the changes in the familiar language of experience.[156]

More specifically, it must be shown that the memory contents of a typical observer described by Everett's theory are consistent with the qualitative and quantitative features that are commonly ascribed to the results of the observations carried out in atomic physics: the "appearance of the collapse" (i.e. the invariance of the result when a measurement is immediately repeated, and the consistency of the results recorded by different observers who measure the same observable) on the one hand, and the statistical distributions predicted by the Born rule for ensembles of measurements carried out on identical systems on the other.

In accordance with the principles of the relativity of states and psychophysical parallelism, these features of empirical data must in the first place be expressed in terms of correlations between memory states of the observer. For example, the repeatability requirement will be expressed by the following proposition (R): Consider an observer O who, after measuring some observable, has immediately repeated the measurement. If r_1 and r_2 are the values recorded by O's memory as the results of the two observations, then $r_1 = r_2$. We note that the correlation between subsequent measurement outcomes has been reduced to "some present properties" of the observer's memory which can be identified "with features of the past experience". The idea behind this move is that

> 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.[157]

Secondly, one must be able to deduce, from the model provided by the universal wave function at a given instant, that (R) has probability 1 of being true. Everett assumes that this second condition is fulfilled if the set of the branches in which the state of O's memory contradicts (R) has vanishing measure in the Hilbert space. As for the measure to be used, Everett proposes, on the basis of a plausibility argument that he finds compelling, a function which is analogous to the probability function appearing in the Born rule. This choice enables Everett to claim that, in the case in which O has performed the same measurement upon an infinite collection of identical systems, the statistical results predicted by the conventional theory are recovered (since they correspond to the statistical distribution recorded by all memory sequences "except for a set [...] of measure zero"). Assuming that "the actions of the [observer] at a given instant can be regarded as a function of the memory contents only", this is supposed to demonstrate why we use standard quantum mechanics to predict experimental results.[158]

We have so far considered the empirical domain of atomic physics. By the same type of argument, Everett also claims that "the classical appearance of the macroscopic world to us can be explained in the wave theory." In quantum mechanics, the general state of a system of macroscopic objects does not ascribe any nearly definite positions and momenta to the individual bodies. Yet, such a state can "at any instant be analyzed into a *superposition* of states each of which *does* represent the bodies with fairly well defined positions and momenta." Hence if one considers the result of an observation performed upon a system of macroscopic bodies in a general state, the observer

> will not see the objects as 'smeared out' over large regions of space [...] but will himself simply become correlated with the system—after the observation the composite system of objects+observer will be in a superposition of states, each element of which describes an observer who has perceived that the objects have nearly definite positions and momenta, and for whom the relative system state is a quasi-classical state [...], and furthermore to whom the system will appear to behave according to classical mechanics if his observation is continued.[159]

Based on the foregoing arguments, Everett maintains that his theory can account for both classical determinism and quantum indeterminism in terms of "subjective experience". In particular, he believes that he has shown "how pure wave mechanics, without any initial probability assertions, can lead to these notions on a subjective level, as appearances to observers."[160] Hence, he claims that, whereas in the conventional formulation the "probabilistic features are postulated in advance instead of being derived from the theory itself", in the relative state formulation

> the statistical assertions of the usual interpretation do not have the status of independent hypothesis, but are deducible (in the present sense) from the pure wave mechanics that starts completely free of statistical postulates.[161]

In the last two decades, several commentators (e.g. Barrett, 1999; Kent, 1990) have pointed out that Everett's argument is wanting. There is perhaps no need of a statistical postulate in order to "interpret" each branch of the universal wave function *individually*, i.e. to state which occurrences in the "perceived world" that particular branch describes. Yet, the theory provides us with infinite branches, and *this* is the formal structure from which we have to extract empirical information. *Here* we need what Everett calls the "interpretive part" of the theory. As a matter of fact, Everett *does use* an interpretive rule in his deduction, which is similar to that of classical statistical mechanics, although logically weaker. Unlike the measure of the set of trajectories in the phase space of statistical mechanics, the measure of the set of branches is not straightforwardly interpreted as a statistical weight for empirical statements. Nevertheless, such an interpretation is indeed assumed in the limiting case: true statements are those which hold for all but a set of branches of measure zero. Everett himself asserts that "the situation here is fully analogous to that of classical statistical mechanics" and develops the analogy in detail. The very constraints from which Everett derives the mathematical function to be used as a measure in the Hilbert space reflect in his

155 Everett (1973, p. 118).
156 Everett (1957b, p. 142).
157 *Ibid*, p. 144.
158 *Ibid*, pp. 148; 144.
159 Everett (1973, pp. 89–90).
160 *Ibid*, p. 78; see also p. 142.
161 Everett (1957b, p. 149).

eyes "the only choice which makes possible any reasonable statistical deductions at all", just as "the choice of Lebesgue measure on the phase space can be justified by the fact that it is the only choice for which the 'conservation of probability' holds."[162] In his assessment of 1957, Wheeler makes a quick allusion to Laplace's universe. From a pencilled note in the margin of a letter, we learn that the analogy he saw between Everett's and Laplace's theories was in fact quite deep and general. "In Laplace description," he says, "we don't know what's going to happen tomorrow morning, but we have a scheme within which it fits." And he adds: "How to do the same in qm description of nature."[163]

It is unlikely that Everett would have endorsed a *postulate* stating the interpretive rule his argument seems to rest on. One often gets the impression that he believed that the rule simply followed from an adequate interpretation of branches. But the few passages that are explicitly intended to clarify the controversial aspects of such an interpretation, either in published papers or in private correspondence, can hardly be said to shed any light on the issue.[164] In the last decades, the attempts to provide a consistent interpretation of branches have given rise to a growing family of disparate approaches, ranging from many-worlds and many-minds to consistent histories and relational interpretations. For almost all these approaches it is important to define the ontological status of branches—a problem that Everett systematically avoids, talking at most of a *language* difficulty in connection to the "splitting" of the observer state when a measurement is performed.[165] In the light of Everett's pragmatic conception of reality, the question of whether his pictorial language is to be understood literally or metaphorically may appear immaterial. Yet, among the 1955 manuscripts, there is a paper (Everett, 1955c) in which Everett seems to take rather seriously the "splitting" process and its possible effects as seen "from within". In that paper he says for example that, after a measurement, "the observer himself has split into a number of observers, each of which sees a definite result of the measurement."[166] Or that the price to be paid in order to have a complete theory "is the abandonment of the concept of the uniqueness of the observer, with its somewhat disconcerting philosophical implications."[167] He also draws a detailed analogy with the case of a splitting amoeba. On this passage, Wheeler, who read the manuscript, annotated: "This analogy seems to me quite capable of misleading readers in what is a very subtle point. Suggest omission." And elsewhere: "Split? Better words needed." While acknowledging the value of the paper, Wheeler wrote to Everett that it had to be reformulated in order to avoid "mystical misinterpretations by too many unskilled readers."[168] From these remarks, it would seem that Wheeler considered the references to branches and splitting as a matter of form, rather than one of substance. Certainly, however, he was aware that Everett's pictorial phrasing might not only be confusing, but might also conceal some real shortcoming. In replying to the claim of the Copenhagen group that there was no relationship at all between "Everett's system" and "physics as we do it", Wheeler said:

> No, because Everett traces out a correspondence between the 'correlations' in his model universe on the one hand, and the on the other hand what we observe when we go about making measurements. [...] Has the nature of the correspondence been made clear [...]? Far from it.[169]

6. Striving for Copenhagen's imprimatur

At the beginning of the fall term of 1955, Everett submitted *Quantitative Measure of Correlation* and another paper (probably *Probability in Wave Mechanics*) to Wheeler. In his response, after approving the former, Wheeler observed: "As for the 2nd one, I am frankly bashful about showing it to Bohr in its present form, valuable and important as I consider it to be."[170] Remarkably, the reference to Bohr comes without any introductory comment. Since it must have been quite unusual for a Princeton student to have his drafts read by Bohr in person, this suggests that the possibility of sending the paper to Copenhagen had already been discussed. When exactly we do not know. In October 1954, Bohr had visited Princeton, and we know that he met Everett.[171] But it is unlikely that any serious discussion between them took place on that occasion. The project to get Bohr involved in the assessment of Everett's thesis could have originated from Wheeler. The aforementioned note shows that Wheeler was impressed by Everett's qualities and ideas since the beginning (see Section 3.2). Furthermore, as we shall see, although Wheeler endorsed Bohr's doctrine, he was puzzled by some aspects of it, and probably saw Everett's proposal as an opportunity to sound Bohr out about the necessity to "generalize" the orthodox view.

In 1956, Wheeler was invited by the university of Leiden to hold the Lorentz Chair for one semester. Before leaving in April, he received from Everett a bound copy of *Wave Mechanics Without Probability*, which he mailed to Copenhagen soon after his arrival in Leiden.[172] In the letter accompanying the manuscript, Wheeler appears quite cautious. "The title itself," he says, "[...] like so many ideas in it, need further analysis and rephrasing."[173] A few days later, Wheeler went to Copenhagen in order to discuss the

[162] *Ibid*, pp. 147–149. For example, the additivity requirement, which plays a crucial role in the deduction, is so chosen as "to have a requirement analogous to the 'conservation of probability'." (*Ibid.*) In his letter to Max Jammer (*op. cit.*), Everett insisted that his "deduction of the probability interpretation" was "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." And he continued: "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."

[163] Stern to Wheeler, 1956, *op. cit.*

[164] See Barrett (1999, pp. 86–90).

[165] Everett (1973, p. 68).

[166] Everett (1955c, p. 5).

[167] *Ibid*, p. 8. In a note of 1956, Everett wrote: "Statistical ensemble of observers is, within the context of the theory, a *real*, in distinction to a *virtual*, ensemble!" (*Notes on Stern's letter*, 1956, *ME*.)

[168] This remark is contained in a note that Wheeler sent to Everett in September 1955 (Wheeler to Everett, 1955, *op. cit.*). That Wheeler was indeed referring to *Probability in Wave Mechanics* is actually only a conjecture, though a plausible one.

[169] Wheeler to Stern, 1956, *op. cit.*

[170] Wheeler to Everett, 1955, *op. cit.*

[171] There is a photograph, which appeared in a local journal, portraying Bohr holding a discussion with a group of students, and Everett is among them (Shikhovtsev, 2003; the photograph is deposited in the Emilio Segrè Visual Archives of the American Institute of Physics).

[172] Wheeler to Everett, 1956 [I], *op. cit.*

[173] John A. Wheeler to Niels Bohr, 24 Apr 1956, *BSC* (reel 34). This letter contains a passage (in which Wheeler refers to the "second draft of the thesis of Everett") that seems to confirm that Bohr already knew about Everett, and that the *first* version of the thesis had already been mentioned to him. That Petersen was acquainted with Everett's former writings is suggested by a passage of a letter, in which, besides other things, he says: "I also had the opportunity to read the *new* draft of your thesis." (Aage Petersen to Hugh Everett, 28 May 1956, *ME*; our emphasis).

draft with Bohr and Petersen. Shortly after returning to Leiden, he wrote to Everett:

> We had three long and strong discussions about it. [...] 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.

Wheeler also added that Bohr had promised to write to him about Everett's work and that "he was arranging [...] for Stern to give [...] a seminar report on [Everett's] thesis, so it could be thoroughly reviewed before he wrote."[174] The same day, Wheeler forwarded to Everett the notes that he had taken during the discussion with Petersen (when he was with Bohr, he said, he wrote "almost never"[175]), together with a second letter in which he outlined his plan of action. In this letter, besides insisting on the necessity of removing any possible source of misunderstanding (though this was going to take "a lot of heavy *arguments* with a practical tough minded man like Bohr"), Wheeler tried to make clear what he considered to be the main issue at stake:

> I don't think, because I don't make out Bohr's case well, that it isn't strong or convincing: that the words you use in talking about things in your formalism have nothing to do with words+concepts of everyday physics; that one will give rise to a complete misunderstanding of what is going on to use the same words.[176]

After some time, Wheeler, who had not received any news from Bohr, wrote to Stern. Stern answered that he had indeed given a seminar on Everett's paper, and added that "Prof. Bohr was kind enough to make a few introductory remarks and open the discussion." The outcome of this discussion was a merciless criticism of Everett's "erudite, but inconclusive and indefinite paper."

> In my opinion, there are some notions of Everett's that seem to lack meaningful content, as, for example, his universal wave function. Moreover he employs the concept of observer to mean different things at different times [...].
>
> 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 subjective appearances of observers.
>
> But, to my mind, the basic shortcoming in his method of approach [...] is his lack of an adequate understanding of the measuring process.
>
> His claim that process I [the Schrödinger equation] and process II [the collapse of the wave function] 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.[177]

Wheeler's reply is a long and detailed defence of Everett's proposal, which aims to dispel the impression that Everett's purpose was to criticise the orthodox approach. In the preamble of his letter, Wheeler reassured Stern about his own intentions:

> I do not in any way question the self consistency and correctness of the present quantum mechanical formalism [...]. 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.

About Everett, Wheeler observed 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.[178]

(Of this alleged conversion there is no trace in Everett's writings; see Section 4.2.) Although Wheeler believed that "the concept of 'universal wave function'" was indeed "an illuminating and satisfactory way to present the content of quantum theory", he was prepared to "recognise that there are many places in Everett's presentation that are open to heavy objection, and still more that are subject to misinterpretation." He added that "to make the whole discussion consistent at every point" he would "make sure that Everett [had] the benefit of a number of weeks in Copenhagen." The importance that Wheeler attached to this plan is also apparent from his previous letters to Everett:

> I told Bohr I'd arrange to pay [...] half your minimum rate steamship fare New York to Copenhagen; I think there's an appreciable chance Bohr would take care of the other half, according to what he said. He would welcome very much a several weeks' visit from you to thrash this out. 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!).

To this request, Wheeler added the following remark: "So in a way your thesis is all done; in another way, the hardest part of the work is just beginning". And he concluded: "How soon can you come?"[179] This letter was dictated by phone or telex in order to reach Everett as soon as possible, and, as previously mentioned, it was followed by another sent the same day. In the second letter, Wheeler reiterated his plea and argued that Everett's qualities would not have done much good unless he went and fought "with the greatest fighter" (in which case, he pledged to go to Copenhagen during part of Everett's time there "if that might help"). Wheeler also said that in his annual letter of assessment to the National Science Foundation Fellowship Board (which sponsored Everett's studies), he had urged the need for Everett to go to Copenhagen "with this sentence: 'I feel Everett's very original work is destined to become widely known, and it ought to have the bugs ironed out of it before it is published rather than after!'"[180] In the same period, Wheeler wrote to Bohr, arguing that Everett should "discuss the issue with [him] directly and arrive at a set of words to describe his formalism that would make sense and be free from misunderstandings for this purpose."[181]

[174] Wheeler to Everett, 1956 [I], *op. cit.* Alexander Stern was an American researcher then at the Institute of Theoretical Physics of Copenhagen.

[175] Wheeler to Everett, 1956 [II], *op. cit.*

[176] *Ibid.*

[177] Stern to Wheeler, 1956, *op. cit.*

[178] Wheeler to Stern, 1956, *op. cit.*

[179] Wheeler to Everett, 1956 [I], *op. cit.*

[180] Wheeler to Everett, 1956 [II], *op. cit.*

[181] John A. Wheeler to Niels Bohr, 24 May 1956, *BSC* (reel 33).

Wheeler's strategy is outlined in a letter to Allen Shenstone, the chairman of the Physics Department of Princeton University:

> After a first review in Copenhagen of Everett's Thesis in its present only partly satisfactory second draft, I have urged him to come and struggle it out in person with Bohr for a few weeks. I would like to see the thesis reach a form where it will be accepted for publication in the Danish Academy. I think his very original ideas are going to receive wide discussion. [...] Since the strongest present opposition to some parts of it comes from Bohr, I feel that acceptance in the Danish Academy would be the best public proof of having passed the necessary tests. Because of my feeling of the importance of this mutual agreement before publication, I am contributing $260 towards Everett's travel out of my very small Elementary Particle Research Fund.[182]

The project of having Everett's work published by the Royal Danish Academy of Sciences had already been mentioned to Everett in the two letters of May 22nd:

> I also feel that the Danish Academy and under Bohr's auspices is the best possible plan for you to publish your work: a full length presentation, going to a wide audience.[183]

When Everett got the news from Wheeler, he phoned him. Following their conversation, Wheeler cabled to Bohr:

> Everett now Princeton phone asking confer with you hopes fly almost immediately but must return in midjune you cable him if convenient my great hope thesis suitable Danish academy publication after revision have answered Stern regards.[184]

Shortly afterwards, Everett received a cable from Copenhagen, in which some reservations about this plan were expressed.[185] The cable was followed by a letter which Petersen wrote after consulting with Wheeler. In the letter, Petersen assured Everett that Bohr would have very much liked to discuss his ideas with him, but he added that a period of 2 or 3 months was in their opinion necessary to "come to the bottom of the problems."[186] Since he had in the meantime returned Everett's dissertation to Wheeler (with a note enclosed explaining that Bohr had been too busy to send comments "on the question discussed in the thesis", but hoped to write to him in more detail "about the status of observers in the complementary mode of description"[187]), Petersen requested Everett to send a new copy. He also suggested that "as a background of [his] criticism", Everett should give "a thorough treatment of the attitude behind the complementary mode of description" and state as clearly as possible "the points where he [thought] that this approach [was] insufficient."[188] In the middle of June, Everett was expected to start a job at the Weapon Systems Evaluation Group of the Pentagon in Arlington, which was incompatible with the conditions laid out by Bohr for the visit to Copenhagen. Even though Everett had not excluded the possibility of being allowed to leave in the fall, the project was eventually abandoned.[189]

Wheeler came back from Europe at the end of September 1956. By that time, Everett had passed his final examination and left Princeton for Arlington.[190] However, it took 6 more months for the thesis to be finally submitted (it was defended in April 1957 and graded "very good"[191]). Bohr and his collaborators (including Rosenfeld and Hip Groenewold[192], who had not attended Stern's seminar, but had read the manuscript) were "warmly thanked" in a note "for the useful objections."[193] An obvious question is why the thesis, whose second version had been achieved in the first months of 1956, was submitted only 1 year later. We know from a letter of Wheeler's that, for administrative reasons related to military service, Everett wished to remain registered at Princeton University at least until 1956.[194] In the course of 1956, as we have seen, he moved to the Pentagon, where he was no longer in danger of being drafted, but probably had little time to devote to the thesis.[195] Besides these practical reasons, however, it is likely that the revision of the second version in the light of the objections raised in Copenhagen took a good deal of time. In his autobiography, Wheeler remembers that he worked with Everett "long hours at night in [his] office to revise the draft."[196] In an interview with Kenneth Ford, DeWitt reported Wheeler's recollection more colourfully, saying that Wheeler told him many years later that "he sat down beside Everett and told him precisely what to write."[197] Elsewhere, DeWitt expressed the belief that "Wheeler felt that the Uhrwerk [the long thesis] might offend his hero Bohr."[198] Wheeler explains in his autobiography that "his real intent was to make [Everett's] thesis more digestible to his other committee members."[199] Bohr and the debate with the Copenhagen group are not mentioned.[200] Yet, there is little doubt that the revision also aimed at making Everett's ideas "more digestible", or at least more comprehensible, *to Bohr*.

The external observation formulation, with which Everett contrasts his approach in the final version of his thesis, is associated, if only obliquely, with Bohr's view—which was not the case for the "conventional formulation" that Everett criticised in the long thesis. At the same time, the emphasis is no longer on the alleged shortcomings of the orthodox view, but on the limitations which seem to restrict its domain of applicability. In his assessment, Wheeler is careful to stress that Bohr's view provides a *consistent* interpretation of the conventional theory. He points out that the "'external observation' formulation of

182 John A. Wheeler to Allen G. Shenstone, 28 May 1956, *WP* (Series I—Box Di—Fermi Award #1—Folder Everett).

183 Wheeler to Everett, 1956 [II], *op. cit.* In the other letter of the same day, he says "I would like to feel happier than I do with the final product; then I would like to see it published in the Danish Academy in full—that's the perfect place for it." (Wheeler to Everett, 1956 [I], *op. cit.*)

184 John A. Wheeler to Niels Bohr, Cable, 26 May 1956, *BSC* (reel 33).

185 Aage Petersen to Hugh Everett, 28 May 1956, *ME*.

186 Petersen to Everett, 1956, *op. cit*.

187 Aage Petersen to John A. Wheeler, 26 May 1956, *BSC* (reel 33).

188 Petersen to Everett, 1956, *op. cit.* To this suggestion, Everett replied: "[...] while I am doing it you might do the same for my work." (Everett to Petersen [draft], 1956, *op. cit.*) Everett agreed to send a new copy of the thesis and remarked: "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)."

189 Petersen to Everett, 1956, *op. cit.*; Everett to Petersen [draft], 1956, *op. cit*.

190 Wheeler to Shenstone, 1956, *op. cit.*; Petersen to Everett, 1956, *op. cit.* Everett to Petersen [draft], 1956, *op. cit.*, Nancy Everett's calendar of events, *op. cit.*

191 *GAR*.

192 Groenewold had been at the University of Groningen since 1951 (he became professor in 1955). He had made his doctorate at the university of Utrecht under the supervision of Rosenfeld, with a dissertation entitled *On the Principles of Elementary Quantum Mechanics*.

193 Everett (1957a, p. 1).

194 Wheeler to Everett, 1956, *op. cit.* In the interview with Misner (*op. cit.*, p. 6), Everett himself alludes to the risk of being enlisted in the army upon finishing his studies, and this circumstance is confirmed by DeWitt (Bryce S. DeWitt to Eugene Shikhovtsev, [w/d], courtesy of Eugene Shikhovtsev).

195 Petersen to Everett, 1956, *op. cit.* Everett to Petersen [draft], 1956, *op. cit.*, Nancy Everett's calendar of events, *op. cit.*; DeWitt to Shikhovtsev, [w/d], *op. cit*.

196 Wheeler (2000, p. 268).

197 DeWitt interview, *op. cit.*, p. 6.

198 DeWitt to Shikhovtsev, [w/d], *op. cit.*

199 Wheeler (2000, p. 268).

200 Nor are the discussions with Bohr mentioned in Wheeler's interviews deposited in the archives of the American Institute of Physics.

quantum mechanics has the great merit that it is dualistic"[201]—which is a remarkably gentle way of saying that it "splits the world in two."[202] We know that Everett regarded this "artificial dichotomy" as "a philosophic monstrosity" (see Section 5.2),[203] but Wheeler himself, in his autobiography, refers to it as "a difficulty that still deeply troubles me and many others."[204] Wheeler's cautiousness is jokingly pointed out by DeWitt in a letter of 1967:

> [...] I can only say 'Good Old John!'. It always amused me to read your Assessment of Everett's theory [...] how highly you praised Bohr, when the whole purpose of the theory was to undermine the stand he had for so long taken!"[205]

In 1956, writing to Stern, Wheeler had been unequivocal: "Everett's thesis is not meant to *question* the present approach to the measurement problem, but to accept it and *generalize* it."[206] Indeed, in the introduction and in the conclusion of the paper of 1957, the relative state formulation is not presented as an *alternative* to the orthodox approach, but rather as a *new theory* which generalizes it.

> The aim is not to deny or contradict the conventional formulation of quantum theory, which has demonstrated its usefulness in an overwhelming variety of problems, but rather to supply a new more general and complete formulation, from which the conventional interpretation can be *deduced*.[207]

Everett's dissertation was published in the *Reviews of Modern Physics*, within a collection of papers "prepared in connection with the Conference on the Role of Gravitation in Physics" held at Chapel Hill in January of 1957. Everett did not attend the conference. Yet, his ideas were mentioned in the discussions,[208] and his paper was submitted by Wheeler to DeWitt, who was the editor in charge for the section of the July issue of the *Reviews* containing conference papers.[209] The paper was published together with a "companion piece" written by Wheeler, since, notwithstanding the thorough revision, Wheeler was not yet completely satisfied and feared the possible misunderstandings.[210] In his assessment, Wheeler discussed some aspects of Bohr's epistemological analysis explicitly, showing how they could be reformulated in the framework of Everett's theory. These were certainly not the optimum publishing conditions for Everett's work to receive the wider recognition that Wheeler had originally hoped for. Pre-prints were nonetheless sent to many distinguished physicists, including Schrödinger, van Hove, Oppenheimer, Dyson, Yang, Wiener, Wightman, Wigner, and Margenau, besides of course Bohr and his collaborators.[211]

The responses of DeWitt, Wiener and Margenau were quite favourable.[212] Groenewold sent a long letter, in which he said that although he found the new draft much improved compared to that he had borrowed 1 year earlier in Copenhagen, "with regard to the fundamental physical and epistemological aspects" he "still profoundly disagree[d]."[213] Once more, Bohr answered that, although he had no time to write down his comments, he would have asked Petersen to report their discussions. His only remark was that the argumentation contained "some confusion as regards the observational problem."[214] Perhaps he had in mind this "confusion" when, 2 months later, he wrote to Wheeler that he was preparing a new collection of his papers on the epistemological problems in quantum physics (*Atomic Physics and Human Knowledge* was to appear the following year) and that he hoped that, "in spite of all present divergences", this would "help to appreciate the clarification of our position in this field of experience", which, according to his conviction, had been obtained.[215]

Petersen's letter followed indeed, as Bohr had promised. It rejected Everett's approach as a whole, defending the Copenhagen approach to measurement and pointing out Everett's alleged misunderstandings. In his answer, besides spelling out his criticisms of Bohr's approach (see Section 4.2), Everett mentioned the possibility of being "sent to Europe in the fall on business", in which case he "could probably take a few weeks off and come to Copenhagen."[216] But something hindered this second attempt. The meeting between Everett and Bohr that Wheeler had longed for eventually occurred 2 years later, in March 1959. During the 6 weeks he spent in Copenhagen, Everett met Bohr, but, according to the recollections of his wife, no real discussion on Everett's ideas took place.[217] In Everett's interview, the comments on his visit to Copenhagen are lost in background noise, and we are left with only a few fragments ("that was a hell...doomed from the beginning"), which are however quite telling.[218] A much more explicit account is contained in a letter written by Rosenfeld (who had moved to Copenhagen in 1958) many years later:[219]

> With regard to Everett neither I nor even Niels Bohr could have any patience with him, when he visited us in Copenhagen more than 12 years ago in order to sell the hopelessly wrong ideas he had been encouraged, most unwisely, by Wheeler to develop. He was undescribably stupid and could not understand the simplest things in quantum mechanics.

7. The issues at stake in the debate

The fact that Wheeler was persuaded that Everett's ideas might obtain Bohr's approval is puzzling. It shows that we should not confine an analysis of the discussions about Everett's proposal to overt disagreements. We must address in the first place the

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201 Wheeler (1957, p. 151).

202 Wheeler (2000, p. 269).

203 Everett (1955a, p. 3); Everett to DeWitt, 1957, *op. cit.*

204 Wheeler (2000, p. 269).

205 Bryce S. DeWitt to John A. Wheeler, 20 Apr 1967. *WP* (Series I—Box Co-De Folder DeWitt). DeWitt refers to Wheeler (1957) (see below).

206 Wheeler to Stern, 1956, *op. cit.*

207 Everett (1957b, p. 141).

208 For example, Feynman, who attended the conference, made some critical remarks on "the concept of a 'universal wave function'." (This fact was brought to our attention by H. Dieter Zeh, who saw the report of the proceedings of the conference deposited in the Wright Air Development Center, Ohio.)

209 DeWitt to Shikhovtsev, [w/d], *op. cit.*

210 Wheeler (2000, p. 268).

211 John A. Wheeler, *Note*, 10 Mar 1957, *WP* (Series I—Box Di—Fermi Award #1—Folder Everett).

212 DeWitt to Wheeler, 1957, *op. cit.*; Wiener to Wheeler & Everett, 1957, *op. cit.*; Henry Margenau to John A. Wheeler & Hugh Everett, 8 Apr 1957, *WP* (Series I—Box Di—Fermi Award #1—Folder Everett).

213 Groenewold to Everett & Wheeler, 1957, *op. cit.*

214 Bohr to Wheeler, 12 April 1957, *op. cit.*

215 Niels Bohr to John A. Wheeler, 6 Aug 1957, *BSC* (reel 33).

216 Everett to Petersen, 1957, *op. cit.*

217 Nancy Everett recalled that "during our visit [...] Niels Bohr was in his 80's and not prone to serious discussion of any new (strange) upstart theory." Nancy Gore Everett to Frank Tipler, 10 Oct 1983, *EP* (Box 1, Folder 9). (Bohr was actually 73.) Wheeler gave a similar account in a letter to Max Jammer (19 Mar 1972, *WP*, Series I—Box I—Jason—Folder Jammer).

218 Everett interview, *op. cit.*, p. 8.

219 Rosenfeld to Belinfante, 22 Jun 1972, *op. cit*. In a letter of 1971, Rosenfeld congratulated John Bell for having succeeded in giving "an air of respectability" to "Everett's damned nonsense". (Léon Rosenfeld to John S. Bell, 30 Nov 1971. *RP*.) (Rosenfeld referred to a talk given by Bell at an international conference held at the Pennsylvania State University, in which Bell had presented Everett's theory as a "refurbishing of the idea of preestablished harmony".) Rosenfeld's words should of course be placed in the context of the 1970s (see Section 8). We are thankful to Anja Jacobsen for having brought the correspondence of Rosenfeld with Belinfante and Bell to our attention.

misunderstandings surrounding the Copenhagen view, as well as its inherent ambiguities.

7.1. Symbolism

To the Copenhagen group, Everett's formulation of quantum mechanics appeared as a "symbolic limbo" having no thread of connectivity with concrete experimental practice.[220] Everett's interpretation of the wave function seemed to them quite confusing and unjustified, since it endowed the *predictive* symbols of the conventional theory with a *descriptive* connotation which they were not meant to have.[221] In his letter of 1956, Stern wrote:

> Then there is the concept of state in quantum theory. An elementary system does not come with a "ready-made" state. It does not possess a state in the sense of classical physics.[222]

A similar remark was made by Petersen in his discussion with Wheeler:

> Ψ does not pertain to a phys[ical] system in the same way as a dynamical variable. [...]Ψ fu[nction] for elec[tron] doesn't have sense until we get something like a prob[ability] dist[ribution] of spots.
>
> So, Q.M. formalism no well defined appli[cation] without exp[erimental] arrangement.[223]

Indeed, as Groenewold pointed out in 1957, one could figure out an accurate theory of atomic phenomena involving no wave functions at all:

> All physical observable quantities may ultimately be expressed in statistical relations between results of various measurements. These relations may be expressed [...] without wave functions (or more general statistical operators).[224]

In his reply to Stern, Wheeler addressed such objections:

> 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?
>
> (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*.

This was a "*new physical theory*", stemming from "Everett's free volition." Again and again Wheeler stresses the same point:

> The greatest possible confusion will result if the mathematical quantities in Everett's theory, such as the wave function, are thought of as having the purpose that the wave function fulfills in the customary formulation.

And referring to the link between Everett's model and the phenomena:

> The very meaning of the word "consequences" has to be defined within the framework of the theory itself, not by applying to Everett's concept of wave function epistemological considerations that refer to 'wave function' in the *completely different* of the usual formalism.[225]

Of course, the idea that the state vectors provide a "complete *model* for our world", rather than "expressing the probabilities for the occurrence of individual events observable under well-defined experimental conditions",[226] could hardly appear attractive to Bohr, rooted as it was in a conception of theories that he regarded as a vestige of the classical way of thought. In Bohr's eyes, Everett's attempt to avoid any reflection about the use of concepts in physics, by taking the wave function "as the basic physical entity *without a priori interpretation*", could not produce "a further clarification of the foundations of quantum mechanics."[227] Scientific knowlegde, for him, was no less concerned with *words* than it was with the mathematical symbolism (see Section 7.4). This point was stressed by Bohr in his discussions with Wheeler, who, as we have seen in Section 6, after his journey to Copenhagen wrote to Everett that the words that went with the formalism had to be drastically revised in order to avoid "complete misinterpretation of what physics is about."[228] Even though Wheeler's phrasing seems to call more for the improvement of Everett's prose than for a reflection on the use of concepts, there is little doubt that what Bohr actually wanted to emphasise was the general fact that "one can no more exclude meaning and understanding from physics than one can substitute servomechanisms for physicists."[229]

7.2. Relativity

Both Everett and Bohr considered it an important lesson to be learnt from quantum mechanics that physical systems could not be endowed with properties "in the absolute". Yet Everett thought that his relative state formulation was the only way to take the fundamental relativity of properties into account without introducing either subjective or dualistic features in physics. As we have seen, this solution did not put into question the assumption that there must be a correspondence between the state vector of a system and its "objective properties". Bohr's complementarity instead dispensed with the idea that measurements reveal (and state vectors describe) properties which are defined independently of the experimental context. In quantum mechanics, the

[220] Stern to Wheeler, 1956, *op. cit.* Stern is referring here to "Heisenberg's recent attempt at a theory of elementary particles", which he compares to Everett's proposal.

[221] "[...] The entire formalism is to be considered as a tool for deriving predictions, of definite or statistical character, as regards information obtainable under experimental conditions described in classical terms and specified by means of parameters entering into the algebraic or differential equations of which the matrices or the wave-functions, respectively, are solutions. These symbols themselves, as is indicated already by the use of imaginary numbers, are not susceptible to pictorial interpretation." (Bohr, 1948, p. 314). Everett outlines Bohr's instrumentalist conception of formalism in the long thesis (1973, p. 110). See Stapp (1972) for a discussion.

[222] Stern to Wheeler, 1956, *op. cit.*

[223] Wheeler, *Notes*, 1956, *op. cit*. When he read this sentence, Everett scrawled in the margin: "Nonsense!".

[224] Groenewold to Everett & Wheeler, 1957, *op. cit*.

[225] Wheeler to Stern, 1956, *op. cit.*

[226] The quotations are from Wheeler to Stern, 1956, *op. cit.*, and Bohr (1948, p. 314) respectively.

[227] Everett (1957b, p. 142), and Aage Petersen to Hugh Everett, 24 Apr 1957, *WP* (Series I—Box Di—Fermi Award #1—Folder Everett).

[228] Wheeler to Everett, 1956 [I], *op. cit*. In his notes (*op. cit.*), Wheeler reports that Petersen, recalling that Everett blamed Bohr for his "conservative" attitude, retorted: "Bohr would say Everett much too class[ical], not in math but in recognize new features. Just as in past formalisms, the whole problem the tough one was to find the right words to express the content of the formalism in acceptable form."

[229] Stern to Wheeler, 1956, *op. cit*. See Section 7.4 for further discussion.

attribution of properties to a system is consistent with the empirical data only in so far as the observations are confined to a given set of "compatible" observables, i.e. to *certain* experimental contexts. Therefore, from a Bohrian point of view, the fact that state vectors do work as a *meta-contextual* predictive tool prevents us from interpreting them as descriptions of the putative properties of a system. Accordingly, the state vector attributed to a system acquires a physical meaning only when it has been related to the eigenvalues of some observable and to the operations through which the observable is measured.[230] As Petersen put it:

> Only a coord[inate] sys[tem] can give a vector a meaning. Have to know Ψ plus *exp[erimental]* apparatus to make predictions.[231]

Bohr himself repeatedly made this point in his lectures and correspondence, emphasising the analogy with the situation encountered in special relativity.[232] As pointed out by DeWitt, however, also Everett's theory could be put into correspondence with Einstein's approach, although of course for different reasons:

> The conventional interpretation of the formalism of quantum mechanics in terms of an "external" observer seems to me similar to Lorentz's original version (and interpretation) of relativity theory, in which the Lorentz-Fitzgerald contraction was introduced *ad hoc*. Everett's removal of the "external" observer may be viewed as analogous to Einstein's denial of the existence of any privileged inertial frame.[233]

Everett's theory can be regarded as an attempt to "objectify" the relational aspects of Bohr's approach. The "relativity to the context" implied by Bohr's pragmatic view of formalism is replaced by the "relativity of states", which results in correlations that can be entirely represented *within* the symbolic model of the universe. As we know, the main motive for this move was to neutralise the alleged subjectivist implications of the projection postulate. In the relative state formulation, after a measurement, there is no outcome that is more "actual" than the other a priori possible outcomes: all outcomes are "actual" relative to some state of the universe, and this is supposed to eliminate the need to resort to a "magic process" that projects the state vector onto the subspace corresponding to *the* specific property allegedly revealed by the measurement. However, no "magic process" is required in Bohr's approach either. For Bohr, state vectors are merely predictive symbols that serve to anticipate the results obtained in a well-defined context: if the context undergoes an "objective" change, as it does *after* a result has been recorded, so does the state vector to be used for predicting the results of further observations. This point was emphasised by Groenewold:

> Now one can introduce the statistical operator, which just represents in a very efficient way all the information which already has been obtained and which may be used to calculate the conditional probability (with respect to this information) of other information which still may be obtained or used. Thus also the statistical operator is conditional and depends on the standpoint from which the system is described. It is relative like the coordinate frame in relativity theory. It seems to me that this conditional character has been overlooked in your papers (as well as in many others).[234]

7.3. Irreversibility

For the Copenhagen group, the main shortcoming of Everett's theory was that it failed to recognize the fundamental role of irreversibility in physics. Stern wrote to Wheeler:

> 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 to 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. Such a connotation would be more in accord with the fact of the irreversibility, the wholeness of the quantum phenomenon as embodied in the experimental arrangement.[235]

Likewise, in his letter of 1957, after pointing out the necessity of cutting off the "measuring chain", Groenewold remarked:

> But it is extremely fundamental that this cutoff is made after the measuring result has been recorded in a permanent way, so that it no longer can be essentially changed if it is observed on its turn (i.e. if the chain is set forth). This recording has to be more or less irreversible and can only take place in a macrophysical (recording) system. This macrophysical character of the later part of the measuring chain is decisive for the measuring process. I do not think that it can be left out of consideration in its description. It does not seem to act an essential part in your considerations.[236]

From Everett's standpoint, such objections were completely misguided.[237]

> [...] one of the fundamental motivations of the paper is the question of *how can it be* that mac[roscopic] measurements are "irreversible", the answer to which is contained in my theory (see remarks chap. V), but *is* a serious lacuna in the other theory.

Indeed, as we have seen in Section 4.2, the way in which the Copenhagen group accounted for the irreversibility of the measurement process was for Everett highly unsatisfactory and mysterious. In Bohr's writings, the fundamental role of

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[230] For Bohr, what is relative (to a given experimental context) is not the property itself, but rather the very possibility of attributing a given property to a system. For a discussion see Murdoch (1987, Chapter 7). It is telling that, in his epistemological writings, Bohr preferred the term "behaviour" to that of "property" (*Ibid*, p. 135). The meta-contextual connotation that the notion of "property" has in ordinary language must have appeared confusing to Bohr when applied to atomic systems.

[231] Wheeler, *Notes*, 1956, *op. cit*.

[232] See e.g. Murdoch (1987, pp. 145–146).

[233] DeWitt to Everett & Wheeler, 1957, *op. cit.*

[234] Hip Groenewold to Hugh Everett & John A. Wheeler, 11 Apr 1957, *WP* (Series I—Box Di—Fermi Award #1—Folder Everett). For a discussion see Teller (1981). In the light of these considerations, and in spite of the differences emphasised by both parties in the debate, one could be tempted to point out some connections between Bohr's and Everett's approaches. On the one hand, by taking into account Everett's emphasis on correlations, one might argue that Bohr's interpretation of the state vector requires no projection postulate at all. On the other hand, Bohr's notion of complementarity might be helpful in interpreting Everett's principle of the relativity of states. According to such a principle, the properties possessed by a system at a given instant depend critically on the basis chosen to expand the universal wave function (see Barrett, 1999). One may assume the existence of some "internal" mechanism which selects a preferred basis. But as long as this is not done, the arbitrary choice of the preferred basis that determines *which* sort of (relative) properties are attributed to a system (for instance, a definite value for position, but not for momentum) looks very much like the Bohrian choice between "complementary" contexts. For a discussion see Bitbol (1998, pp. 286–293).

[235] Stern to Wheeler, 1956, *op. cit*.

[236] Groenewold to Everett & Wheeler, 1957, *op. cit*.

[237] They indicated "rather clearly" that his critics had "had insufficient time to read" his work. This and the following quotations are taken from *Everett's notes on Stern's letter*, 1956, *ME*.

irreversibility in physics was often stressed. But, according to Everett, little was said about the origin of this "magic irreversibility".

The arguments put forward by the Copenhagen group about this and other aspects of measurement involved (and sometimes mixed up) two different levels of reflection. The first and more fundamental level implied a *pragmatic-transcendental* argument to the effect that irreversibility is a *constitutive* feature of measurement, and that it cannot be ensured unless the description of the results is framed within the representation of ordinary "objective" experience. The second level implied a *physical* explanation of irreversibility connecting irreversibility with the "reduction" of the state vector, and the reduction of the state vector with the *macroscopic* nature of the measuring apparatus. Assuming that quantum mechanics should also apply to the macroscopic domain, the former (pragmatic) argument raised a problem of consistency, which the latter (physical) argument was designed to settle.[238]

A detailed analysis of the issue of irreversibility in connection with Everett's work can be found in the correspondence that Rosenfeld had with Belinfante in 1972. It is worth quoting some passages from these letters, with the caveat that they were written many years after Everett's dissertation. The context was then heavily influenced by the controversies of the 1960s, in which Bohr, who died in 1962, took no part. Rosenfeld, who, as he says himself, was doing his best to pull Belinfante out of the pitfall in which he had been precipitated by the reading of Everett,[239] wrote to him:

> ... I do not think you are right to go on and say that one could do without reducing the state vector, which means physically without carrying the measurement to its completion by recording a permanent mark of its result. You should leave such a heresy to Everett.[240]

In his letters, Rosenfeld explained that the reason why there is "no choice whatsoever about the necessity of applying the [state] reduction" is that "the reduction rule is nothing else than a formal way of expressing the idealized result of the registration": without it "the phenomenon is not well defined."[241] He also stressed that the "reduction rule" did not require an *ad hoc* postulate: it could be deduced (in principle) from thermodynamic considerations that applied to *macroscopic* systems. Since the registration is necessary, and since it requires state reduction, which can only be established for macroscopic systems, Rosenfeld concluded that nobody "can avoid committing himself to accepting the necessity of macroscopic measuring instruments."[242] Indeed, as we have seen (Section 2.3), in the early 1960s Rosenfeld supported, against Wigner, the theory of measurement proposed by Daneri, Loinger and Prosperi (1962). In his opinion, such a theory provided a rigorous framework for Bohr's ideas.[243] In the 1950s, however, the Copenhagen group did not oppose Everett's objections with anything like a *theory* of measurement, but merely with a collection of generic statements.

7.4. Words

In Bohr's view, the mathematical symbols employed in physics have a meaning only inasmuch as they refer to well-defined measurements. Therefore, the meaningful use of a theory presupposes that one can define unambiguously the experimental setup, in which the measurements are performed, as well as their possible outcomes.[244] This point was made by Petersen during the discussions of 1956:

> [...] Math can never be used in phys[ics] until have words. [...] What mean by physics is what can both be expressed unambig[uously] in ordinary language. Spots on plate have meaning but not in Everett—he talks of correlations but can never build that up by Ψ fun[ctions].[245]

Stern stressed the same idea in his letter:

> Our formalism must be in terms of possible or idealized experiments whose interpretations thereby involves [sic] 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.

He also illustrated this point by means of an example taken from biology:

> To trace the schizophrenic phenomenon from the basic molecular level to the observational level of its psychological symptomatic manifestations is an aspect of the observation problem. It cannot be traced in the detail of a space-time description.[246]

This example is meant to show that physical theories establish correlations between facts of our experience, the "definition" of which does *not* involve the mathematical constructs of those very theories. Such a remark generalized a typical Copenhagen assertion, which Groenewold summarised as follows:

> Because all observable quantities may ultimately be expressed in statistical relations between measuring results and the latter are represented by essentially macrophysical recordings, the former ones may ultimately be expressed in macrophysical language. That does of course not mean that the formalism, which serves as a tool for calculating these statistical relations could also be expressed in macrophysical language. On the contrary in this field the macrophysical language is liable to loose its original more or less unambiguous meaning.[247]

Besides highlighting the importance of "classical" concepts (i.e. concepts used in ordinary language and classical physics) for describing the experimental context in which atomic phenomena are observed, Bohr also insisted on the need to use such concepts for providing a pictorial description of the phenomena themselves. In both cases Bohr assumed that an account based on classical concepts automatically fulfilled the conditions for an *objective* description. In the former case, as we have seen, such conditions were related to the requirements of communicability and repeatability which are constitutive of experimental practice. In the latter case, they were related to the *ob*jectification of phenomena allegedly required by the very concept of *ob*serva-

[238] See Murdoch (1987, pp. 112–118). See also Section 7.5.

[239] Léon Rosenfeld to Frederik J. Belinfante, 24 Aug 1972, *RP*.

[240] Rosenfeld to Belinfante, 22 Jun 1972, *op. cit*.

[241] Léon Rosenfeld to Frederik J. Belinfante, 24 Jul 1972. *RP*.

[242] *Ibid.*

[243] "Now, the crux of the problem which worries Wigner so much is that the reduction rule appears to be in contradistinction with the time evolution described by Schrödinger's equation. The answer, which was of course well known to Bohr, but has been made formally clear by the Italians [Daneri, Loinger and Prosperi], is that the reduction rule is not an independent axiom, but essentially a thermodynamic effect, and accordingly, only valid to the thermodynamic approximation." Rosenfeld to Belinfante, 24 July 1972, *op. cit*.

[244] See e.g. Stapp (1972).

[245] Wheeler, *Notes*, 1956, *op. cit*.

[246] Stern to Wheeler, 1956, *op. cit*.

[247] Groenewold to Wheeler & Everett, 1957, *op. cit*.

tion.[248] This twofold argument is summarised by Petersen in his letter of 1957:

> There can on [Bohr's] view be no special observational problem in quantum mechanics in accordance with the fact that the very idea of observation belongs to the frame of classical concepts. The aim of [Bohr's] analysis is only to make explicit what the formalism implies about the application of the elementary physical concepts. The requirement that these concepts are indispensable for an unambiguous account of the observations is met without further assumptions [...].[249]

As we have pointed out in the discussion about irreversibility, the Copenhagen scientists did not always clearly distinguish the various levels involved in Bohr's argument—the level of language, that of the conditions for the possibility of physics, and that of the content of physical knowledge. This is even more true for Bohr's critics. Everett's reading of Bohr's argument, for example, was that

> [in the Copenhagen interpretation] the deduction of classical phenomena from quantum theory is impossible simply because no meaningful statements can be made without pre-existing classical apparatus to serve as a reference frame.[250]

Here Bohr's *transcendental* reasoning, according to which the formulation of a physical problem *presupposes* the specification of the corresponding experimental conditions ("apparatus"), and hence requires a suitable conceptual framework, is presented as a *physical* assumption about the *existence* of a macroscopic world ("phenomena") governed by classical mechanics. That Everett understood Bohr's argument as a postulate implying "that macrosystems are relatively immune to quantum effects" is confirmed by the main criticism that he addressed to the Copenhagen interpretation, namely that it "[adhered] to a 'reality' concept [...] on the classical level but [renounced] the same in the quantum domain."[251] Unsurprisingly, Everett regarded such a "postulate" with no sympathy at all ("epistemologically garbage", he annotated on Groenewold's letter). For him, Bohr's conception of formalism, as well as his insistence on the primitive role of classical concepts, imposed arbitrary limits upon the scope of quantum mechanics. Everett contrasted this dogmatic position with the pragmatic view that he advocated with regard to "the constructs of classical physics" (see Section 5.1), and he claimed that, by showing that classical *physics* can be derived from quantum theory, one could in fact replace "classical" *concepts* by "quantum" ones. In his reply to Petersen, after pointing out that he did not think that his viewpoint could be dismissed "as simply a misunderstanding of Bohr's position", Everett formulated it as follows:

> The basing of quantum mechanics upon classical physics was a necessary provisional step, but now [...] the time has come to proceed to something more fundamental. There is a good analogy in mathematics. The complex numbers were first introduced only in terms of the real numbers. However, with sufficient experience and familiarity with their properties, it became possible and indeed more natural to define them first in *their own right* without reference to the reals. I would suggest that the time has come to do the same for quantum theory—to treat it in its own right as a fundamental theory without any dependence on classical physics, and to derive classical physics from it. While it is true that initially the classical concepts were required for its formulation, we now have sufficient familiarity to formulate it without classical physics, as in the case of the complex numbers.

Everett concluded this passage by observing: "I'm sure that you will recognize this as Bohr's own example turned against him".[252] Indeed, from Wheeler's notes, we know that, during their discussions, Petersen had made the following example:

> Bohr (ac[cording] to A[age] P[eternsen]) need non rel[ativistic] way to live self into rel[ativistic] world—have to sep[arate] between space [and] time—consider watch; *entrance into* Complex n[umbers] only via real n[numbers]; hence entrance into rel via non rel.[253]

Of course, from a Bohrian standpoint, Everett's hope to *derive* from the theory the conceptual framework *presupposed* by physics was an illusion, since one could not even make sense of the theory without relying on a well-defined experimental practice. As Rosenfeld put it in 1959:

> Everett's work [...] suffers from the fundamental misunderstanding which affects all the attempts at 'axiomatizing' any part of physics. The 'axiomatizers' do not realize that every physical theory must necessarily make use of concepts which *cannot*, in principle, be further analysed, since they describe the relationship between the physical system which is the object of study and the means of observation by which we study it: these concepts are those by which we give information about the experimental arrangement, enabling anyone (in principle) to repeat the experiment. It is clear that *in the last resort* we must here appeal to *common experience* as a basis for common understanding. To try (as Everett does) to include the experimental arrangement into theoretical formalism is perfectly hopeless, since this can only shift, but never remove, this essential use of unanalysed concepts which alone makes the theory intelligible and communicable.[254]

With similar arguments in mind, in 1957 Petersen wrote to Everett:

> Of course, I am aware that from the point of view of your model-philosophy most of these remarks are besides the point. However, to my mind this philosophy is not suited for approaching the measuring problem. I would not like to make it a universal principle that ordinary language is indispensable for definition or communication of physical experience, but for the elucidation of the measuring problem [...] the correspondence approach has been quite successful.[255]

During the discussions in Copenhagen, Wheeler came to realise that, if Everett's "model philosophy" intended to do away with Bohr's prescriptions about the use of classical concepts, it had to show (without relying on Bohr's pragmatic-transcendental argument) that the general conditions which make experimental

[248] See Bitbol (1996b, pp. 256–269) for a critical analysis.
[249] Petersen to Everett, 1957, *op. cit*.
[250] Everett (1973, p. 111). Everett regarded this position as "conservative".
[251] Everett to Petersen, 1957, *op. cit.* See Section 4.2.

[252] *Ibid.* See also Wheeler (1957, p. 151.)
[253] Wheeler, *Notes*, 1956, *op. cit*. Bohr often remarked that the use of imaginary numbers in quantum theory prevents one from interpreting the quantum formalism "as an extension of our power of visualization" (Bohr, 1998 [1937] p. 86). Also, he liked to mention the discovery of irrational numbers as an example of how concrete problems (e.g. measuring the diagonal of the square) may lead us to extending the use of ordinary concepts (in the example: rational numbers) (Petersen, 1985, pp. 301–302).
[254] Léon Rosenfeld to Saul Bergmann, 21 Dec 1959, *RP*. The letter answered the request for "an opinion about Everett's point of view on the presentation of the principles of quantum mechanics" formulated by Saul M. Bergmann of the Boston Laboratory for Electronics.
[255] Petersen to Everett, 1957, *op. cit*.

activity possible are indeed fulfilled in the world described by the theory.[256] In the words of one of Everett's epigones, the theory was demanded to explain "why the sentient beings we know [...] have the particular concepts they do for describing their world".[257] According to Wheeler, one could thus show that Everett's theory "does not require for its formulation any reference to classical concepts" and is "conceptually self-contained."[258] Along these lines, in his discussions with Petersen, Wheeler had sketched an argument according to which, since human practices (including communication and experimentation) are an outgrowth of (the complex physical processes underlying) biological selection, they could be expected to be described by some process occurring within Everett's "model universe": "Thinking, experimentation and communication—or psychophysical duplicates thereof—are all taken by Everett as going on *within* the model universe.".[259] He wrote to Everett:

> Aage Petersen [...] had a tendency to insist that small interaction, small $e^2/\hbar c$, was essential for a world in which one could use normal words. On the contrary, I argued that the world came first—it could have small or large $e^2/\hbar c$, but grant only complex systems, and evolution, and you have systems that *must* find a way to communicate with each other to give mutual assistance in the struggle for existence; in the struggle for survival words would necessarily be invented to deal with a large $e^2/\hbar c$. You don't first give a list of words and then ask what systems are compatible with them; instead, the system comes first, and the words second.[260]

Wheeler's argument was developed in his letter to Stern, in which he concluded:

> The kind of physics that occurs does not adjust itself to the available words; the words evolve in accordance with the kind of physics that goes on.[261]

In the assessment of 1957, we find almost the same sentence. Yet, there is an interesting semantic shift, due to the fact that the term "words", which in the letter stands essentially for "concepts", is replaced by "terminology", and the verb "evolve", which in the letter is clearly related to the evolutionary argument that immediately precedes it, becomes "adjust". Formulated that way, the statement no longer alludes so strongly to a physical explanation of the fact that physicists use certain concepts. We can only conjecture that the objections of the Copenhagen group played some role in this reformulation. However, there is no doubt that the idea of providing a *naturalized* account of the conditions that make physics possible was in contrast to Bohr's doctrine. This is testified by a lapidary remark in Wheeler's notes: "Language *second*. Very contrary to Bohr, say A[age] P[etersen]."[262]

7.5. Observers

In the 1950s, the Copenhagen group seems to have regarded the idea of developing a "quantum theory of measurement" (which would apply to measuring devices) as a possible source of confusion. For example, in the abovementioned report of 1957 (see Section 2.2), Rosenfeld argued:

> Bohr's considerations were never intended to give a 'theory of measurement in quantum theory', and to describe them in this way is misleading, since a proper theory of measurement would be the same in classical and quantal physics, the peculiar features of measurements on quantal systems arising not from the measuring process as such, but from the limitations imposed upon the use of classical concepts in quantum theory. By wrongly shifting the emphasis on the measuring process, one obscures the true significance of the argument and runs into difficulties, which have their source not in the actual situation, but merely in the inadequacy of the point of view from which one attempts to describe it. This error of method has its origin in v. Neumann's book 'Foundations of Quantum Mechanics' [...].

In the report, Rosenfeld made some sarcastic remarks on the efforts made by a group of physicists "to develop their own 'theory of measurement' in opposition to what they believed to be the 'orthodox' theory of measurement, as presented by v. Neumann." According to Rosenfeld, these "reformers [...] involved themselves in a double misunderstanding, criticizing a distorted and largely irrelevant rendering of Bohr's argument by v. Neumann, and trying to replace it by a 'theory' of their own, based on quite untenable assumptions."[263]

In the notes he took in Copenhagen, Wheeler reports these words of Petersen:

> Von N[eumann]+Wig[ner] all nonsense; their stuff beside the point; [...]Von N[eumann]+Wig[ner]—mess up by including [the] meas[uring] tool in [the observed] system. [...] Silly to say apparatus has Ψ-function.[264]

Also, Petersen insisted that, when considering the "paradox outlined by Everett", one must keep in mind the "distinction between Bohr way & the 2 postulate way to do q[uantum] mech[anics]". It should be stressed, however, that "Bohr way" did not rule out the possibility of treating observers quantum-mechanically.[265] Nothing prevents one from providing a model of the physical process which is supposed to correspond to a measurement. Yet the symbols appearing in such a model acquire a meaning only when one states the set of measurements that can be performed *upon* the compound system S+O (where S and O are the *physical systems* which represent the "object-system" and the "apparatus", respectively). In other words, *any* formal model presupposes an observer who can perform the experimental operations and interpret the possible outcomes in accordance

[256] In his paper of 1957 (pp. 151–152), Wheeler says: "The results of the measurements can be spelled out in classical language. Is not such 'language' a prerequisite for comparing the measurements made by different observing systems?"

[257] Vaidman (2002).

[258] Wheeler (1957, pp. 151–152).

[259] Wheeler to Stern, 1956, *op. cit.*

[260] Wheeler, *Notes*, 1956, *op. cit*.

[261] Wheeler to Stern, 1956, *op. cit.*

[262] Wheeler, *Notes*, 1956, *op. cit*. See Petersen (1985). For a thorough analysis of the philosophical background of Bohr's doctrine of concepts, see Chevalley (1994). See also Murdoch (1987), Faye (1991).

[263] Rosenfeld, 1957, *op. cit.* Rosenfeld is here alluding to David Bohm and other "young physicists, who, misled partly by v. Neumann's ideas, partly by preconceived philosophical opinions, were unable to understand the real problems underlying the formulation of quantum theory, and [...] undertook to reform quantum theory according to their own liking, and to develop, as they put it, a 'causal interpretation' of this theory." However, since the report was written in 1957, it is likely that Everett's work had some role in exacerbating Rosenfeld's irritation.

[264] Wheeler, *Notes*, 1956, *op. cit*.

[265] See Bohr (1939). In that paper, Bohr asserted: "In the system to which the quantum mechanical formalism is applied, it is of course possible to include any intermediate auxiliary agency employed in the measuring process." (Bohr, 1998 [1939] p. 104.) In one of the abovementioned letters, referring to Wigner's allusions to a special role played by consciousness in the measuring process, Rosenfeld asserted that the opinion according to which the "recording process is not entirely describable by quantum mechanics" was "simply wrong". (Rosenfeld to Belinfante, 24 Jul 1972, *op. cit.*)

with a given conceptual and pragmatic framework. As Groenewold put it:

> …the observer […] not only "observes" the object system, but also describes it with some theory and "interprets" if you like. …I do not see how your automatical observer included in the described combined system also could be used for describing the activities of reading the recorded measuring result and of assigning statistical operators to the object system on the ground of the obtained information.[266]

The "transcendental" role that the observer (or the apparatus) plays within the instrumentalist view of formalism is taken into account by Bohr's *functional* distinction between the apparatus *qua* physical system and the same *qua* measuring instrument.[267] As Petersen pointed out in his discussion with Wheeler, "QM description of measuring tool prevents its *use* as a meas[uring] tool."[268] In a letter to Everett, Petersen developed this point:

> I do not understand what you mean by quantized observers. Obviously, one can treat any interaction quantum-mechanically, including the interaction between an electron and a photographic plate, but when utilized as an "observer" the definition of the "state" (position) of the plate excludes considerations of quantum effects. It seems to me that as far as your treatment of many-body systems is consistent with the proper use of the formalism it has nothing to do with the measuring problem.[269]

Nonetheless, the existence of two "complementary" ways of conceiving the apparatus raised an issue of consistency:

> On one hand the combined object and measuring systems are considered from the microphysical quantum mechanical point of view. So far one could not even speak of measurement. On the other hand the later part of the measuring chain and in particular the recording system is regarded from the macrophysical classical point of view. A satisfactory theory of measurement has to relate these two aspects to each other.[270]

A solution to this consistency problem is sketched by Rosenfeld in his letter of 1959:

> The fact, emphasized by Everett, that it is actually possible to set up a wave-function for the experimental apparatus and Hamiltonian for the interaction between system and apparatus is perfectly trivial, but also terribly treacherous; in fact, it did mislead Everett to the conception that it might be possible to describe apparatus+atomic object as a closed system. This, however, is an illusion: the formalism used to achieve this must of necessity contain parameters such as external fields, masses, etc. which are precisely the representatives of the uneliminable residues of unanalysed concepts.[271]

A similar remark had been made by Petersen in 1957:

> There is no arbitrary distinction between the use of classical concepts and the formalism since the large mass of the apparatus compared with that of the individual atomic object permits that neglect of quantum effects which is demanded for the account of the experimental arrangement.[272]

With some reason, Everett found this and similar physical explanations loosely formulated and unconvincing. And since he thought that the conclusions reached by Bohr on the basis of his reflection on the preconditions of physics must ultimately be justified by some *physical* arguments, this led him to conclude that Bohr's doctrine rested in fact on a "flatly asserted dogma" (see Section 4.2). Indeed, as we have seen, rendering Bohr's analysis superfluous by exhibiting a self-consistent *physical* model of the world (including observers) was one of the main goals of the final version of Everett's dissertation. This reflected a concern that Wheeler had already expressed in 1956, when he wrote to Bohr:

> But I am more concerned with your reaction to the more fundamental question, whether there is any escape from a formalism like Everett's when one wants to deal with a situation where several observers are at work, and wants to include the observers themselves in the system that is to receive mathematical analysis.[273]

From Stern's letter we know that the idea of providing a naturalized account of the "emergence" of the pragmatic framework presupposed by the instrumentalist interpretation of formalism had been cautiously put forward by Wheeler in a letter of the same year:

> In your letter you ask, "Do we need mathematical models, like those of game theory, that will include the observers, in order to put across to the mathematically minded what is meant by these ideas?" (I take it you mean complementarity and other ideas of quantum theory "as distinct from the mere formalism.")[274]

In the 1957 paper, this proposal was contrasted with the external observation formulation. In such a formulation, the idea that the very possibility of linking the symbolic structure to experience *presupposes* a pragmatic framework is replaced by a postulate implying that "the 'measuring chain' has to be cut off" and that some physical system has to be *left out* of the mathematical description whenever an observation takes place.[275] The foregoing analysis should have made clear that Bohr's hostility towards Wheeler's programme was not due to his commitment to such a postulate. Indeed, Petersen wrote to Everett: "I don't think that you can find anything in Bohr's papers which conforms with what you call the external observation interpretation."[276] What made little sense for Bohr was the attempt to restore what Pauli called the "ideal of the detached observer",[277] by postulating an "independent reality" and assuming that physics must describe it. To him, taking this approach was overlooking the analysis of the

[266] Groenewold to Everett & Wheeler, 1957, *op. cit*. The term "super-observer", which Wheeler uses in his paper of 1957 (p. 152), is possibly reminiscent of some analogous remark made during the discussions in Copenhagen.

[267] See Murdoch (1987, Chapter 5).

[268] Wheeler, *Notes*, 1956, *op. cit.*

[269] Petersen to Everett, 1957, *op. cit*.

[270] Groenewold to Everett & Wheeler, 1957, *op. cit*.

[271] Rosenfeld to Bergmann, 1959, *op. cit*.

[272] Petersen to Everett, 1957, *op. cit*.

[273] John A. Wheeler to Niels Bohr, 24 Apr 1956, *BSC* (reel 34).

[274] Stern to Wheeler, 1956, *op. cit*. The letter quoted by Stern is now lost.

[275] The quotation is from the letter of Groenewold to Wheeler and Everett (*op. cit*.). The "external observation" reading of Bohr's approach was arguably based on his frequent remarks emphasising "the necessity of describing entirely on classical lines all ultimate measuring instruments which define the external conditions of the phenomenon, and therefore of keeping them outside the system for the treatment of which the quantum of action is to be taken essentially into account." (Bohr, 1998 [1939], p. 107.)

[276] Petersen to Everett, 1957, *op. cit*.

[277] Wolfgang Pauli to Niels Bohr, 15 Feb 1955 (Pauli, 1994, p. 43). Pauli uses this expression to denote Einstein's view. Hooker (1991, p. 507) has described such a view as one in which the objectivity of the physical description depends on its ability "to put [us] into the models as *objects* in such a way as to take [us] out of the picture as *subjects*."

very conditions which make it possible for an observer *engaged* in the investigation of experience to describe atomic phenomena objectively.[278]

8. Epilogue

Contrary to Wheeler's hopes, after obtaining his PhD, Everett continued to collaborate with the Pentagon and did not return to academic research.[279] In 1962 he was invited to present the relative-state formulation at a conference on the foundations of quantum mechanics held at the Xavier University of Cincinnati, before an audience including Furry, Wigner, Dirac, Aharanov, Rosen, and Podolsky (a short account of the conference appeared in *Physics Today*[280]). But except for this and other sporadic signs of interest, the impact of Everett's work was modest.[281] DeWitt has reported that when Max Jammer interviewed him for his book on the history of quantum mechanics, in 1969, he did not know anything about Everett. "This," he glossed, "was an example of how totally the physics community was ignoring him."[282]

DeWitt had no sympathy for the Copenhagen interpretation, and he was struck by Everett's ideas when, in 1957, he read the draft of the dissertation that Wheeler sent him.[283] On that occasion he wrote a long and detailed commentary, raising objections to which Everett replied in a way that he found convincing.[284] At the end of the 1960s, in the new climate surrounding the studies on the foundations of quantum mechanics,[285] DeWitt, who "felt that Everett had been given a raw deal" resolved "to rectify this situation".[286] DeWitt's interest in Everett's ideas was at least partly due to the role that they could play in the framework of his own research programme on quantum gravity.[287] In 1967, he presented the "Everett–Wheeler interpretation (EWI)" at the Battelle Rencontres,[288] and 3 years later he lectured on it at the International School of Physics "Enrico Fermi", in the framework of a course on the foundations of quantum physics organised by Bernard d'Espagnat. In 1970 *Physics Today* published a paper in which DeWitt contrasted his many-worlds version of the EWI with both the Copenhagen interpretation and the mentalistic approach advocated by Wigner. The paper gave rise to a lively debate, which marked the beginning of the "rediscovery" of Everett's work.

Everett took no part in that debate. In 1971, he consented to the publication of the long version of the thesis in a small book edited by DeWitt and his student Neill Graham "with the proviso that [he] would not have to devote any effort to editing, proof reading, etc."[289] In 1977, Wheeler, who was then at the University of Texas in Austin, invited Everett for a conference. There Everett met DeWitt for the first and last time.[290] Everett's ideas sparked the interest of some of Wheeler's students who attended the conference. David Deutsch, who was among them, has reported that Everett appeared quite sympathetic to the many-worlds interpretation.[291] However, answering a letter of that year in which he was explicitly asked if he advocated such an interpretation, Everett said laconically: "I certainly approve of the way Bryce DeWitt presented my theory, since without his efforts it would never have been presented at all."[292] And referring in another letter to the title of DeWitt's and Graham's book, *The Many-Worlds Interpretation of Quantum Mechanics*, he said: "This of course was not my title as I was pleased to have the paper published in any form anyone chose to do it in!" And he added: "I, in effect, had washed my hands of the whole affair in 1956."[293] Indeed, Everett made little effort to promote and develop his ideas, and showed himself reluctant to go beyond generic comments in private correspondence either.[294]

There are some hints that Wheeler's attitude after the publication of Everett's dissertation was not very supportive.[295] As we have seen, Wheeler's admiration for Bohr did not prevent him from attaching great importance to Everett's unorthodox

[278] Accordingly, Bohr's idea of completeness, like that of objectivity, had little to do with the possibility of providing an all-encompassing model of the universe, including observers. What counted, instead, was the ability to answer all the possible questions that can be concretely framed in an experimental context. As Hooker (1991, p. 507) puts it: "To be Bohr-objective is to achieve simultaneously both an empirically adequate, exhaustive and symbolically unified description of the phenomena we can produce and an accurate portrayal of the conditions under which such phenomena are accessible to us." Hence "*Bohr-objectivity* cannot consist in removing the knowing subject from the representation of reality—precisely to the contrary." From Bohr's point of view, the "restrictions" that the instrumentalist interpretation of formalism allegedly imposed upon the scope of quantum theory did not deprived us of any portion of physical knowledge. On the contrary, they were (in a Kantian sense) *constitutive* of knowledge. For an analysis of the Kantian aspects of Bohr's philosophy see for example Honner (1987), Murdoch (1987), Faye (1991), Kaiser (1992), Chevalley (1994).

[279] See Wheeler to Everett, 1956 [I], *op. cit.*; Byrne (2007).

[280] Werner (1964). At the conference, Everett was invited to outline his approach, which he did, insisting particularly on the "deduction" of the standard probabilistic interpretation. In reply to questions about the status of branches, Everett examined the case in which an observer performs a sequence of measurements on an ensemble of identical systems. In this case, he argued, each "element" of the resulting superposition of states "contains the observer as having recorded a particular definite sequence of results of observation". He concluded that any such element can be identified as "what we think of as an experience", and that "it is tenable to assert that all the elements simultaneously coexist." To the remark of Podolsky: "It looks like we would have a non-denumerable infinity of worlds", Everett answered: "Yes." (Proceedings of the Conference on the Foundations of Quantum Mechanics, Xavier University, Cincinnati, 1962; deposited at the American Institute of Physics.)

[281] Shikhovtsev (2003) mentions in particular an invitation by Wheeler to give a seminar at Princeton in 1959. Everett's paper was cited in the philosophical works of Margenau (1963), Shimony (1963), and Petersen (1968). It was *not* cited in the famous papers on the measurement problem that Wigner wrote in that period (Wigner 1961, 1963). In 1963, referring to Everett in a letter, Wigner observed: "The state vector, as he imagines it, does not convey any information to anyone, and I don't see what its role is in the framework of science as we understand it." (Eugene Wigner to Abner Shimony, 24 May 1963, *WigP* (Box 94, Folder 1). The limited impact of Everett's work is discussed by Freire (2004) based on the statistics of the citations that it received in the decade that followed the publication.

[282] DeWitt interview, *op. cit*, p. 7.

[283] "I read it and I was stunned, I was shocked." (DeWitt interview, *op. cit*., p. 7.)

[284] Everett to DeWitt, 1957; *op. cit.*; DeWitt interview, *op. cit*., p. 7.

[285] See Freire (2004).

[286] DeWitt to Shikhovtsev, [w/d], *op. cit.*; Bryce S. DeWitt to Olival Freire, pers. comm., 29 Jun 2002.

[287] The paper in which DeWitt presented the famous Wheeler–DeWitt equation relies on Everett's approach in order to provide an interpretive framework for "the state functional of the actual universe" (DeWitt, 1967).

[288] DeWitt (1968).

[289] Hugh Everett to Bill Harvey, 20 Jun 1977, *EP* (Series I–8). The book was published in 1973.

[290] DeWitt interview, *op. cit*., p. 15.

[291] Shikhovtsev (2003).

[292] Everett to Harvey, 1977, *op. cit.*

[293] Everett to Lévy-Leblond, 1977, *op. cit*.

[294] DeWitt asserted many years later: "Everett always took the attitude—and I got this from Charlie Misner as well—that he was not really strongly committed to this." (DeWitt interview, *op. cit*., p. 15.) DeWitt confirmed this opinion in a recent letter, arguing that Everett "was lackadaisical and couldn't care less if other physicists would accept his views." (DeWitt to Shikhovtsev, [w/d], *op. cit.*) It is likely that the reception of his ideas in Copenhagen diminished Everett's original enthusiasm. In any case, even in his last years, Everett maintained that the relative state formulation was the "simplest" and the "only completely coherent approach" "to come to grips with the paradoxes of the measurement process", and that the alternative proposals were "highly tortured and unnatural" and "by far more artificial and unsatisfactory." Everett to Jammer, 1973, *op. cit.*; Everett to Raub, 1980, *op. cit*.

[295] For instance, in a paper about cosmology of 1962, in which he mentioned the "so-called 'universal wave function'", Wheeler (1962) cited his own assessment, but *not* Everett's paper.

ideas, and from believing that it was indeed possible to get "his great master" and his young student to agree.[296] Consequently, the reception of Everett's work in Copenhagen must have left him rather disappointed. In his interview, DeWitt recalled that when the EWI was brought to the knowledge of the wider public by his own paper in *Physics Today*, Wheeler "promptly disowned Everett." DeWitt added that he asked Wheeler why he did not "accept Everett more", but never got a satisfactory answer from him.[297] The circumstance pointed out by DeWitt is confirmed by the *incipit* of a letter which Everett received in 1977 from Jean-Marc Lévy-Leblond:

> Dear Dr. Everett,
> I obtained your address through the kindness of Prof. Wheeler, who suggested that I directly ask your opinion on what I believe to be a crucial question concerning the 'Everett & no-longer-Wheeler' (if I understood correctly!) interpretation of Qu. Mech.[298]

Everett himself alludes to Wheeler's ambiguity in a letter of 1980:

> Dr. Wheeler's position on these matters has never been completely clear to me (perhaps not to John either). He is, of course, heavily influenced by Bohr's position (he was a student of Bohr) which essentially regards the entire formalism as merely a calculating device, and does not worry any further about "reality". It is equally clear that, at least sometimes, he wonders very much about that mysterious process, "the collapse of the wave function". The last time we discussed such subjects at a meeting in Austin several years ago he was even wondering if somehow human consciousness was a distinguished process and played some sort of critical role in the laws of physics.[299]

As is apparent from this passage, Wheeler's attitude towards Everett's work was not as clear-cut as described by DeWitt. Everett reported an anecdote according to which, during the meeting in Austin, Wheeler told him that he mostly believed his interpretation, but reserved Tuesdays once a month to disbelieve it.[300] In 1977, being requested to give an opinion on a paper dealing with the EWI, Wheeler answered that he "still [felt] it [was] one of the most important contributions made to quantum mechanics in recent decades". He added nonetheless that he had "difficulty subscribing to it today." As he had done with Lévy-Leblond, he asked the author to "change the reference from Everett–Wheeler to Everett interpretation".[301] (A copy of the letter was forwarded to Everett, who scrawled on the term "difficulty": "Only on Tuesday"!) To be sure, Wheeler continued to pay attention to Everett's ideas, and never gave up the hope to work with him again.[302] The papers he published in the 1970s and 1980s reflect his effort to reach a satisfactory understanding and an appropriate generalization of the Copenhagen view. From such papers, it is apparent that the time elapsed since the discussions of 1956 had not erased his doubts, and that Everett's work had not completely lost its appeal for him.[303]

9. Concluding remarks

The epilogue of the Everett affair seems to support the idea that as late as in the 1950s the Copenhagen school still exerted a decisive influence, which could go as far as undermining the career of a brilliant physicist in the US. The interpretive model of the "dictatorial imposition"[304] is nonetheless too crude to account for all the aspects of the Everett episode. Indeed, our analysis suggests that the mechanisms which ensured the supremacy of the Copenhagen view (and led to its decline a few years after Bohr's death, in the new climate of which Everett was a forerunner) were actually subtler than they are habitually depicted to be.[305]

Urged by Wheeler (who was a dedicated Bohrian, but did not belong to the inner circle of Bohr's collaborators), the Copenhagen scientists did not refuse to debate the non-conventional proposal of Wheeler's pupil. Admittedly, the objections raised in Copenhagen were very general, and they resulted only partly from a rigorous appraisal of the merits and shortcomings of Everett's work. But this reflected the fact that what bothered Bohr was not so much the technical aspects of Everett's project as the very concept of physical knowledge which underlay it. The existence of such a chasm in the very premises of Everett's and Bohr's interpretations of the quantum formalism was manifestly not apparent to Wheeler. He was one of the very few "missionaries of the Copenhagen Spirit"[306] in America, but his understanding of some aspects of the Bohrian gospel was neither firm nor unequivocal. This explains at once his doubts on the Copenhagen approach to measurement, and his belief that these doubts could be solved without abandoning the framework of Bohr's view. The discussions that Wheeler had with the Copenhagen group were pretty frank, and, notwithstanding his caution, he did not hesitate to put forward arguments which could sound heretical. When it became clear that they were given no importance whatsoever in Copenhagen, he curbed his enthusiasm for Everett's ideas. But his veneration for Bohr could not remove the tension between his firm belief that Bohr's approach provided indeed a deep insight into quantum physics and the feeling that it missed something crucial, and had to be amended. That this situation was a source of inner trouble for him is suggested by his wavering attitude in the 1970s, as well by his reluctance to mention the events of 1956 in later recollections.

We can contrast this attitude with that of Everett, who never bothered too much about the relationship between his ideas and the Copenhagen view. Everett was an exponent of the new American generation growing up in an intellectual and scientific

[296] Everett interview, *op. cit.*, p. 8.

[297] DeWitt interview, *op. cit.*, p. 7; DeWitt to Shikovtsev, [w/d], *op. cit.*

[298] Jean-Marc Lèvy-Leblond to Hugh Everett, 17 Aug 197[7], *EP*. In a lecture reported in the proceedings of the School "Enrico Fermi" of 1977, Wheeler says: "Imaginative Everett's thesis is, and instructive, we agree. We once subscribed to it. In retrospect, however, it looks like the wrong track." (Wheeler, 1979a, p. 396.)

[299] Everett to Raub, 1980, *op. cit.* Wheeler's temporary interest for Wigner-like approaches coincided with his efforts to clarify the question as to whether Bohr's views did involve any reference to consciousness (see Wheeler's letters to Aage Bohr in Freire, 2007; see also Wheeler & Zurek, 1983, p. 207, and Wheeler, 1981).

[300] Everett interview, *op. cit.*, p. 8.

[301] John A. Wheeler to Paul Benioff, 7 Jul 1977; and 7 Sep 1977, *EP*.

[302] According to DeWitt, "one of the very first things he did when he arrived [at the University of Texas] was actually to invite and pay for Everett to come." (DeWitt interview, *op. cit.*, p. 15.) Furthermore, according to Shikhovtsev (2003), Wheeler planned to bring Everett back to theoretical physics in the framework of a project which aimed to create a working group devoted to the quantum theory of measurement at the Institute for Theoretical Physics in Santa Barbara, but the whole project was eventually abandoned.

[303] Wheeler's idea of a "participatory universe" (Wheeler & Zurek, 1983, pp. 182–183) can be said to have inspired a number of attempts to "go beyond" Bohr's view of measurement along the lines of the relative state formulation (see e.g. Omnès, 1992; Rovelli, 1996; Zurek, 1998). In some of these approaches, the explicit inclusion of the observer in the quantum description of the universe is supposed to enable one to dismiss the postulate of projection (see Barrett, 1999). Furthermore, in order to demonstrate the "emergence of a classical world from a quantum universe" (a definitely Everettian idea), the advocates of such approaches have sometimes put forward evolutionary arguments reminiscent of those sketched by Wheeler in the discussion with the Copenhagen group (see Vaidman, 2002 for a list of references, and Bitbol, 1996b, pp. 414–418 for a discussion).

[304] Jammer (1974, p. 250).

[305] See Howard (2004).

[306] Heilbron (2001).

context which had little to do with that of the German-speaking Europe between the two wars: his attitude prefigures that of many physicists and philosophers of the 1960s, for whom Bohr came to represent a positivism out of date. Everett pointed out what he considered to be the limitations of Bohr's approach and straightforwardly ascribed them to Bohr's dogmatic and conservative stance. There was no effort on his part to reach a deeper understanding of the philosophical background of complementarity, and no hesitation to seek a formulation of quantum mechanics in which Bohr's reflections on the nature of scientific knowledge could be simply bypassed.

References

André, G. (Ed.). (1953). *Louis de Broglie: Physicien et penseur*. Paris: Albin Michel.
Assmus, A. (1992). The Americanization of molecular physics. *Historical Studies in the Physical and Biological Sciences*, *23*(1), 1–34.
Barrett, J. A. (1999). *The quantum mechanics of minds and worlds*. Oxford: Oxford University Press.
Becker, L. (2004). That von Neumann did not believe in a physical collapse. *British Journal for the Philosophy of Science*, *55*(1), 121–135.
Bell, J. S. (2004). *Speakable and unspeakable in quantum mechanics*. Cambridge: Cambridge University Press.
Beller, M. (1999a). *Quantum dialogue: The making of a revolution*. Chicago: University of Chicago Press.
Beller, M. (1999b). Jocular commemorations: The Copenhagen Spirit. *OSIRIS, 2nd series*, *14*, 252–273.
Ben-Dov, Y. (1990). Everett's theory and the "many-worlds" interpretation. *American Journal of Physics*, *58*(9), 829–832.
Bitbol, M. (1996a). *Schrödinger's philosophy of quantum mechanics. Boston studies in the philosophy of science*. Dordrecht: Kluwer Academic Publishers.
Bitbol, M. (1996b). *Mécanique quantique. Une introduction philosophique*. Paris: Flammarion.
Bitbol, M. (1998). *L'aveuglante proximité du réel, anti-réalisme & quasi-réalisme en physique*. Paris: Flammarion.
Bitbol, M. (2000). *Physique et philosophie de l'esprit*. Paris: Flammarion.
Bohm, D. (1951). *Quantum theory*. New York: Prentice-Hall.
Bohm, D. (1952). A suggested interpretation of the quantum theory in terms of "hidden" variables—I and II. *Physical Review*, *85*(2), 166–179 180–193.
Bohr, N. (1937). Causality and complementarity. *Philosophy of Science*, *4*, 289–298 (Reprinted in: N. Bohr, *Causality and complementarity: Supplementary papers. The philosophical writings of Niels Bohr*, Vol. 4 (pp. 83–91). Edited by J. Faye & H. Folse. Wooldbridge (CT): Ox Bow Press, 1998. Page numbers refer to the reprint.).
Bohr, N. (1939). The causality problem in atomic physics. In *The new theories of physics* (pp. 11–45). Paris: International Institute of Intellectual Cooperation (Reprinted in: N. Bohr (Ed.), *Causality and complementarity: Supplementary papers. The philosophical writings of Niels Bohr*, Vol. 4 (pp. 94–121). Edited by J. Faye & H. Folse. Wooldbridge (CT): Ox Bow Press, 1998. Page numbers refer to the reprint.).
Bohr, N. (1948). On the notions of causality and complementarity. *Dialectica*, *2*, 312–319.
Bohr, N. (1949). Discussion with Einstein on epistemological problems in atomic physics. In P. A. Schilpp (Ed.), *Albert Einstein—Philosopher–Scientist* (pp. 199–242). Evanston: The Library of the Living Philosophers.
Bohr, N. (1963). *Essays 1958–1962 on atomic physics and human knowledge*. New York: Interscience Publishers.
Bohr, N. (1998). Causality and complementarity: Supplementary papers. In J. Faye, & H. Folse (Eds.), *The philosophical writings of Niels Bohr*, Vol. 4. Wooldbridge (CT): Ox Bow Press.
Born, M. (1953). The interpretation of quantum mechanics. *British Journal for the Philosophy of Science*, *4*, 95–106.
Brush, S. G. (1980). The chimerical cat: Philosophy of quantum mechanics in historical perspective. *Social Studies of Science*, *10*(4), 393–447.
Butterfield, J. N. (2002). Some worlds of quantum theory. In R. Russell, & J. Polkinghorne, et al. (Eds.), *Quantum Mechanics. Scientific Perspectives on Divine Action*, Vol. 5 (pp. 111–140). Vatican City: Vatican Observatory Publications.
Byrne, P. (2007). The many worlds of Hugh Everett. *Scientific American, December 2007*, 98–105.
Camilleri, K. (2008). Constructing the myth of the Copenhagen interpretation. *Perspectives on Science*, to appear.
Cartwright, N. (1987). Philosophical problems of quantum theory: The response of American physicists. In L. Krüger, G. Gigerenzer, & M. S. Morgan (Eds.), *The probabilistic revolution*, Vol. 2 (pp. 407–435). Cambridge, MA: MIT Press.
Cassinello, A. (1994). La interpretación de los muchos universos de la mecánica cuántica. Apuntes históricos. *Arbor, Revista de Ciencia, Pensamiento y Cultura*, *148*(584), 47–68.
Chevalley, C. (1994). Niels Bohr's and the Atlantis of Kantianism. In J. Faye, & H. Folse (Eds.), *Niels Bohr and contemporary philosophy. Boston studies in the philosophy of science* (pp. 33–55). Dordrecht: Kluwer Academic Publishers.
Chevalley, C. (1997). Mythe et philosophie: la construction de "Niels Bohr" dans la doxographie. *Physis, Rivista Internazionale di Storia della Scienza*, *34*(3), 569–603.
Chevalley, C. (1999). Why do we find Bohr obscure? In D. Greenberger, W. L. Reiter, & A. Zeilinger (Eds.), *Epistemological and experimental perspectives on quantum mechanics* (pp. 59–73). Dordrecht: Springer.
Daneri, A., Loinger, A., & Prosperi, G. M. (1962). Quantum theory of measurement and ergodicity conditions. *Nuclear physics*, *33*, 297–319 (Reprinted in: J. A. Wheeler & W. H. Zurek (Eds.), *Quantum theory and measurement* (pp. 657–679). Princeton: Princeton University Press, 1983.).
DeWitt, B. S. (1967). Quantum theory of gravity. I. The canonical theory. *Physical Review*, *160*, 1113–1148.
DeWitt, B. S. (1968). The Everett–Wheeler interpretation of quantum mechanics. In C. M. DeWitt, & J. A. Wheeler (Eds.), *Battelle rencontres 1967. Lectures in mathematics and physics* (pp. 318–332). New York: W.A. Benjamin Inc.
DeWitt, B. S. (1970). Quantum mechanics and reality. *Physics Today*, *23*(9), 155–165.
DeWitt, B. S., & Graham, N. (1973). *The many-worlds interpretation of quantum mechanics*. Princeton, NJ: Princeton University Press.
Dicke, R. H., & Wittke, J. P. (1960). *Introduction to quantum mechanics*. Reading, MA: Addison-Wesley.
Dirac, P. A. M. (1958). *The principles of quantum mechanics* (4th ed.). Oxford: Clarendon Press.
Everett III, H. (1955a). *Objective vs subjective probability. EP* (Box 1, Folder 6).
Everett III, H. (1955b). *Quantitative measure of correlation. EP* (Box 1, Folder 6).
Everett III, H. (1955c). *Probability in wave mechanics. EP* (Box 1, Folder 6).
Everett III, H. (1956). *Wave mechanics without probability* [?]. *EP* (Box 1, Series II, Folder 1).
Everett III, H. (1957a). *On the foundations of quantum mechanics*. Ph.D. thesis, Princeton University.
Everett, H., III (1957b). "'Relative State' formulation of quantum mechanics". *Review of Modern Physics*, *29*, 454–462 (Reprinted in: J. A. Wheeler & W. H. Zurek (Eds.), *Quantum theory and measurement* (pp. 315–323). Princeton: Princeton University Press, 1983. Page numbers refer to the reprint.).
Everett III, H. (1973). The theory of the universal wave function. In B. S. DeWitt, & N. Graham (Eds.), *The many-worlds interpretation of quantum mechanics* (pp. 3–140). Princeton, NJ: Princeton University Press.
Faye, J. (1991). *Niels Bohr, his heritage and legacy. An anti-realist view of quantum mechanics*. Dordrecht: Kluwer Academic Press.
Faye, J. (2002). Copenhagen interpretation of quantum mechanics. In E. N. Zalta (Ed.), *The Stanford encyclopedia of philosophy (summer 2002 edition)*. Available online: ⟨http://plato.stanford.edu/archives/sum2002/entries/qm-copenhagen/⟩.
Feyerabend, P. (1964). Problems of microphysics. In R. G. Colodny (Ed.), *Frontiers of Science and Philosophy* (pp. 189–283). London: George Allen and Unwin.
Forman, P. (1971). Weimar culture, causality, and quantum theory, 1918–1927: Adaptation by german physicists and mathematicians to a hostile intellectual environment. *Historical Studies in the Physical Sciences*, *3*, 1–115.
Forstner, C. (2008). The early history of David Bohm's quantum mechanics through the perspective of Ludwik Fleck's thought-collectives. *Minerva*, *46*, 215–229.
Freire, O., Jr. (1997). Quantum controversy and marxism. *Historia Scientiarum*, *7*(2), 137–152.
Freire, O., Jr. (2004). The historical roots of "foundations of quantum mechanics" as a field of research (1950–1970). *Foundations of Physics*, *34*(11), 1741–1760.
Freire, O., Jr. (2005). Science and exile: David Bohm, the cold war, and a new interpretation of quantum mechanics. *Historical Studies in the Physical and Biological Sciences*, *36*(1), 1–34.
Freire, O., Jr. (2007). Orthodoxy and heterodoxy in the research on the foundations of quantum physics: E. P. Wigner's case. In B. S. Santos (Ed.), *Cognitive justice in a global world: Prudent knowledge for a decent life* (pp. 203–224). Lanham, MD: Lexington Books.
French, A. P., & Kennedy, P. J. (Eds.) (1985). *Niels Bohr: A centenary volume*. Cambridge, MA: Harvard University Press.
Graham, L. (1988). The Soviet reaction to Bohr's quantum mechanics. In H. Feshbach, T. Matsui, & A. Oleson (Eds.), *Niels Bohr and the world: Proceedings of the Niels Bohr centennial symposium* (pp. 305–317). New York: Harwood academic publications.
Heilbron, J. (2001). The earliest missionaries of the Copenhagen spirit. In P. Galison, M. Gordin, & D. Kaiser (Eds.), *Science and society—The history of modern physical science in the twentieth century*, Vol. 4 (pp. 295–330). New York: Routledge.
Heisenberg, W. (1955). The development of the interpretation of the quantum theory. In W. Pauli (Ed.), *Niels Bohr and the development of physics* (pp. 12–29). London: Pergamon Press.
Home, D., & Whitaker, M. A. B. (1992). Ensemble interpretations of quantum mechanics. A modern perspective. *Phsyics Reports*, *71*, 223–317.
Honner, J. (1987). *The description of nature: Niels Bohr and the philosophy of quantum physics*. Oxford: Clarendon Press.
Hooker, C. A. (1991). Projection, physical intelligibility, objectivity and completeness: The divergent ideals of Bohr and Einstein. *The British Journal for the Philosophy of Science*, *42*(4), 491–511.
Howard, D. (1985). Einstein on locality and separability. *Studies in History and Philosophy of Science*, *16*, 171–201.
Howard, D. (2004). Who invented the "Copenhagen interpretation"? A study in mythology. *Philosophy of Science*, *71*, 669–682.
Institut International de Physique Solvay (1928). *Électrons et photons-Rapports et discussions du Cinquième Conseil de Physique tenu à Bruxelles du 24 au 29 octobre 1927*. Paris: Gauthier-Villars.

Jacobsen, A. (2007). Léon Rosenfeld's Marxist defense of complementarity. *Historical Studies in the Physical and Biological Sciences*, *37*(Supplement), 3–34.

Jammer, M. (1966). *The conceptual development of quantum mechanics*. New York: McGraw-Hill.

Jammer, M. (1974). *The philosophy of quantum mechanics—The interpretations of quantum mechanics in historical perspective*. New York: Wiley.

Kaiser, D. (1992). More roots of complementarity: Kantian aspects and influences. *Studies in History and Philosophy of Science*, *23*, 213–239.

Kaiser, D. (2002). Cold War requisitions, scientific manpower, and the production of American physicists after World War II. *Historical Studies in the Physical and Biological Sciences*, *33*(1), 131–159.

Kent, A. (1990). Against many-world interpretation. *International Journal of Modern Physics*, *A5*, 1745–1762 An updated version is available online: 〈http://arxiv.org/abs/gr-qc/9703089〉.

Körner, S. (Ed.). (1957). *Observation and interpretation: A symposium of philosophers and scientists*. London: Butterworth.

Kragh, H. (1999). *Quantum generations—A history of physics in the twentieth century*. Princeton, NJ: Princeton University Press.

Kuhn, T. S. (1970). *The structure of the scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.

Lakatos, I., & Feyerabend, P. K. (1999). In M. Motterlini (Ed.), *For and against method, including Lakatos's lectures on scientific method, and the Lakatos–Feyerabend correspondence*. Chicago: University of Chicago Press.

London, F., & Bauer, E. (1939). *La théorie de l'observation en mécanique quantique*. Paris: Hermann.

Margenau, H. (1954). Advantages and disadvantages of various interpretations of the quantum theory. *Physics Today*, *7*(10), 6–13.

Margenau, H. (1958). Philosophical problems concerning the meaning of measurement in physics. *Philosophy of Science*, *25*, 23–33.

Margenau, H. (1963). Measurement and quantum states—I. *Philosophy of Science*, *30*, 1–16.

Mehra, J., & Rechenberg, H. (2001). *The historical development of quantum theory*, Vol. 6 (Part 2). New York: Springer.

Murdoch, D. (1987). *Niels Bohr's philosophy of physics*. Cambridge: Cambridge University Press.

Olwell, R. (1999). Physical isolation and marginalization in physics—David Bohm's Cold War exile. *Isis*, *90*, 738–756.

Omnès, R. (1992). Consistent interpretations of quantum mechanics. *Reviews of Modern Physics*, *64*(2), 339–382.

Paty, M. (1995). The Nature of Einstein's Objections to the Copenhagen Interpretation of Quantum Mechanics. *Foundations of Physics*, *25*, 183–204.

Pauli, W. (1955). *Niels Bohr and the development of physics*. London: Pergamon Press.

Pauli, W. (1994). *Writings on physics and philosophy*. In C. P. Enz & K. von Meyenn (Eds.), translated by R. Schlapp. New York: Springer.

Petersen, A. (1968). *Quantum physics and the philosophical tradition*. Cambridge, MA: MIT Press.

Petersen, A. (1985). The philosophy of Niels Bohr. In A. P. French, & P. J. Kennedy (Eds.), *Niels Bohr, a centenary volume* (pp. 299–310). Cambridge, MA: Harvard University Press.

Rédei, M., & Stöltzner, M. (Eds.). (2001). *John von Neumann and the foundations of quantum physics*. Dordrecht: Kluwer Academic Press.

Rosenfeld, L. (1965). The measuring process in quantum mechanics. *Supplement of the Progress of Theoretical Physics*, *1965*, 222–231.

Rovelli, C. (1996). Relational quantum mechanics. *International Journal of Theoretical Physics*, *35*, 1637–1678.

Rozental, S. (Ed.). (1967). *Niels Bohr. His life and work as seen from his friends and colleagues*. Amsterdam: North-Holland.

Scheibe, E. (1973). *The logical analysis of quantum mechanics*. Oxford: Pergamon Press.

Schilpp, P. A. (Ed.). (1949). *Albert Einstein—Philosopher–Scientist*. Evanston: The Library of the Living Philosophers.

Schrödinger, E. (1952). Are there quantum jumps?—I and II. *British Journal for the Philosophy of Science*, *3*, 109–123 233–242.

Schrödinger, E. (1953). The meaning of wave mechanics. In G. André (Ed.), *Louis de Broglie: physicien et penseur* (pp. 1–32). Paris: Albin Michel.

Schrödinger, E. (1958). Might perhaps energy be a merely statistical concept. *Nuovo Cimento*, *9*, 162–170.

Schweber, S. (1986). The empiricist temper regnant: theoretical physics in the United States 1920–1950. *Historical Studies in the Physical Sciences*, *17*(Part 1), 55–98.

Shikhovtsev, E.B. (2003). *Biographical sketch of Hugh Everett*, III. Available online: 〈http://www.hep.upenn.edu/~max/everett/everettbio.pdf〉.

Shimony, A. (1963). Role of the observer in quantum theory. *American Journal of Physics*, *31*(6), 755–773.

Sopka, K. R. (1980). *Quantum physics in America 1920–1935*. New York: Arno Press.

Stapp, H. P. (1972). The Copenhagen interpretation. *American Journal of Physics*, *40*, 1098–1116.

Stapp, H. P. (1994). Quantum theory and the place of mind in nature. In J. Faye, & H. Folse (Eds.), *Niels Bohr and contemporary philosophy. Boston studies in the philosophy of science* (pp. 245–252). Dordrecht: Kluwer Academic Publishers.

Stöltzner, M. (1999). What John von Neumann thought of the Bohm interpretation. In D. Greenberger, W. L. Reiter, & A. Zeilinger (Eds.), *Epistemological and experimental perspectives on quantum mechanics* (pp. 257–262). Dordrecht: Springer.

Teller, P. (1981). The projection postulate and Bohr's interpretation of quantum mechanics. In P. Asquith, & R. Giere (Eds.), *PSA 1980: Proceedings of the 1980 Biennial meeting of the Philosophy of Science Association*, Vol. 2 (pp. 201–223). East Lansing: Michigan State University.

Vaidman, L. (2002). The many-worlds interpretation of quantum mechanics. In: E. N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy*, 2002. Available online: 〈http://plato.stanford.edu/archives/sum2002/entries/qm-manyworlds〉.

Von Neumann, J. (1955). *Mathematical foundations of quantum mechanics. Translated by R. T. Beyer*. Princeton, NJ: Princeton University Press.

Werner, F. G. (1964). The foundations of quantum mechanics. *Physics Today*, *17*(1), 53–60.

Wheeler, J. A. (1956). A septet of Sibylis. *American Scientist*, *44*, 360–377.

Wheeler, J. A. (1957). Assessment of Everett's "Relative State" formulation of quantum theory. *Review of Modern Physics*, *29*, 463–465 (Reprinted in: J. A. Wheeler & W. H. Zurek (Eds.), *Quantum theory and measurement* (pp. 324–325). Princeton: Princeton University Press, 1983. Page numbers refer to the reprint.).

Wheeler, J. A. (1962). The Universe in the light of general relativity. *The Monist*, *47*, 40–76.

Wheeler, J. A. (1979a). Frontiers of time. In G. Toraldo di Francia (Ed.), *Problems in the foundations of physics. Proceedings of the international school of physics E. Fermi* (pp. 395–497). Dordrecht: North-Holland (Partially reprinted in: J. A. Wheeler & W. H. Zurek (Eds.), *Quantum theory and measurement* (pp. 182–213). Princeton: Princeton University Press, 1983.).

Wheeler, J. A. (1979b). Mercer Street and other memories. In G. E. Tauber (Ed.), *Albert Einstein's theory of general relativity* (pp. 182–184). New York: Crown Publishers.

Wheeler, J. A. (1981). Not consciousness but the distinction between the probe and the probed as central to the elemental quantum act of observation. In R. G. Jahn (Ed.), *The role of consciousness in the physical world* (pp. 87–111). Boulder: Westview.

Wheeler, J. A. (1985). Physics in Copenhagen in 1934 and 1935. In A. P. French, & P. J. Kennedy (Eds.), *Niels Bohr, a centenary volume* (pp. 221–226). Cambridge, MA: Harvard University Press.

Wheeler, J. A. (2000). *Geons, black holes and quantum foam. With K. W. Ford*. New York: Norton.

Wheeler, J. A., & Zurek, W. H. (Eds.). (1983). *Quantum theory and measurement*. Princeton, NJ: Princeton University Press.

Wigner, E. P. (1961). Remarks on the mind-body question. In I. J. Good (Ed.), *The scientist speculates* (pp. 284–302). London: Heinemann (Reprinted in J. A. Wheeler & W. H. Zurek (Eds.), *Quantum theory and measurement* (pp. 168–181). Princeton: Princeton University Press, 1983.).

Wigner, E. P. (1963). The problem of measurement. *American Journal of Physics*, *31*, 6–15.

Zurek, W. H. (1998). Decoherence, einselection and the existential interpretation (the rough guide). *Philosophical Transactions of the Royal Society of London*, *A356*, 1793–1820.