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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with the Copenhagen group, the existence and content of which have been only recently disclosed by the discovery of unpublished documents.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.

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

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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).