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Olival Freire Jr.:

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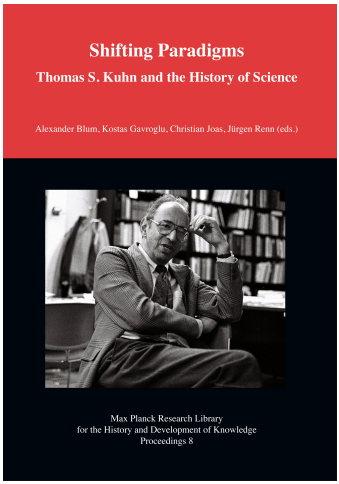

In: Alexander Blum, Kostas Gavroglu, Christian Joas and Jürgen Renn (eds.): *Shifting Paradigms : Thomas S. Kuhn and the History of Science*
Online version at `http://edition-open-access.de/proceedings/8/`

ISBN 978-3-945561-11-9

Printed and distributed by:
Neopubli GmbH, Berlin
`http://www.epubli.de/shop/buch/50013`

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available in the Internet at `http://dnb.d-nb.de`

Chapter 6
Contemporary Science and the History and Philosophy of Science

Olival Freire Jr.

Introduction

A reader of *Structure of Scientific Revolutions* (Kuhn 1970) is surely struck by Max Planck saying a new theory is accepted only when the supporters of the old one have died. This citation had a wider audience than physicists and historians of physics and it has been taken as emblematic of the idea of the incommensurability of paradigms. A second glance at this citation suggests that we reflect on the interaction with and the involvement in recent and contemporary science by historians and philosophers of science, taking particularly the case of quantum theory and Thomas Kuhn's views on it. Kuhn himself trained firstly as a theoretical physicist and later became a historian and philosopher of physics; he was also an admirer of quantum theory as one the most influential achievements in science. He was the head of Archives for the History of Quantum Physics (*AHQP*), a huge project launched in the early 1960s to collect documents and oral histories related to the creation of this physical theory. He devoted one of his main books on the history of science to this subject: *Black-body Theory and the Quantum Discontinuity 1894–1912* (Kuhn 1978). Thus I would like to take Thomas Kuhn and his *Structure of Scientific Revolutions* as a case for discussing the involvement of historians and philosophers of science in recent and contemporary science. In other words, I will take the relationship between one of the major scientific accomplishments Kuhn was influenced by, that is quantum theory, and his own work on the history and philosophy of science.

Furthermore I am particularly interested in the enduring controversy over the interpretation and foundations of quantum theory and Kuhn's own views on it. The debates on these issues first arose at the time of the theory's inception, lasting until the early 1930s. Subsequently, they were revived during Kuhn's lifetime in the 1950s. Thus, I would like to discuss issues such as: How Kuhn's works and views were shaped by quantum physics, its first dominant interpretation and the ongoing controversy that followed it. As it is widely acknowledged

that Kuhn's *Structure of Scientific Revolutions* has had only scant influence on the historiography of science (and the debates at this workshop reflect this), I would like to know if the historiography on the quantum controversy reflects the Kuhnian corpus. Finally, if not through his published works, one wonders how Kuhn, through letters and unpublished material, reacted to the revival of the quantum controversy. This paper attempts to deal with these issues. After a brief review of the attention philosophers paid to quantum theory, I focus on Kuhn's case, mainly presenting Mara Beller's criticisms towards *Structure of Scientific Revolutions*. I then move beyond Beller's criticisms to compare the historiography on the quantum controversy, and the very existence of this controversy, with Kuhn's published and unpublished papers. I conclude by coming back to the general question of the role played by recent and contemporary science in the scholarship produced by historians and philosophers of science.

Quantum Debates and Philosophers

Kuhn was not the first philosopher to become interested in the debates on the interpretations and foundations of quantum theory. In the early stage of those debates, philosophers such as Karl Popper, Hans Reichenbach, Gaston Bachelard, Grete Hermann and Alexandre Kojève ventured into the field once the reserve of professional physicists. In the 1950s, with the controversy reheated especially because of the appearance of the causal interpretation suggested by David Bohm, there was new fuel for the philosophy of science. However, while in the 1930s philosophers mostly produced works of a more epistemological nature, in the sense of providing a critical analysis of an existent scientific theory, they now divided along the same lines as physicists. Some were sympathetic towards Bohm's enterprise, as in the case of Paul Feyerabend who praised Bohm's *Causality and Chance*, as containing "an explicit refutation of the idea that complementarity, and complementarity alone, solves all the ontological and conceptual problems of microphysics" (Feyerabend 1960, 321). Others aligned with Bohr's point of view, notably Norwood Hanson, who maintained that "when an interpretation of a theory has been as successful as this one [Copenhagen interpretation] has been, there is little practical warrant for the 'alternative interpretations' which have, since Bohm, been receiving prominence" (Hanson 1959, 1). And yet, there were cases, such as Bachelard, who retired from the debate as it became heated and de Broglie reconverted to the deterministic description of quantum phenomena (Freire Jr. 2004a). Since then, the debate on the foundations of quantum physics has been an attractive topic for philosophers of science. In the 1960s a new batch of philosophers entered the quantum field. Some were trained both in philosophy and physics, such as Abner Shimony, others were trained in physics, such as

Jeffrey Bub and Mario Bunge, and some of these physicists were philosophically minded, such as Bernard d'Espagnat. From the 1970s on there was a true industry of philosophically inclined investigations on the foundations of quantum theory. In hindsight, it can be said that it is hard to find a scholar with some training in physics and an interest in philosophy of science who has not devoted some attention to the issues in this field. As we will see, Kuhn seems to be one of those rare cases.

Thomas Kuhn and the Interpretation of Quantum Theory

The case of Kuhn and the quantum debates is not new in the literature. The first to spot the problem was the philosopher and historian of science, Mara Beller. She concluded her *Quantum Dialogue – The Making of a Revolution* (Beller 1999) by revisiting the debates on the quantum theory to make the contrast between dialogue, which she considered had been instrumental in the creation of quantum physics, but which had been abandoned just after its inception, and paradigms, which jointly with normal science and the incommensurability of paradigms, became a core concept in Kuhn's ideas about how science evolves. For Mara Beller (Beller 1999, 287–306), "the notion of paradigm has not only clear totalitarian implications but also dogmatic ideological roots." These roots Beller found in the alleged "close historical links […] between the notion of incommensurable paradigms and the ideology of the Copenhagen dogma." Beller's targets were Norwood Hanson's and Thomas Kuhn's views on the ways science evolves and the criticisms they leveled at Bohm's causal interpretation. As for disclosure of potential conflicts of commitment, it should be noted that Beller indeed sympathized with Bohm and the causal interpretation. While Hanson is the most documented case of such a link, insofar as he was involved with the criticism of Bohm's ideas and the defense of Bohr's, Kuhn's case is by far the most interesting, given the wider audience of his *Structure of Scientific Revolutions*. However, evidence of Kuhn's interest in the quantum controversy is scant. Beller cites just one fragment in which Kuhn makes reference to Bohm: "A similar […] feeling seems to underlie the opposition of Einstein, Bohm, and others, to the dominant probabilistic interpretation of quantum mechanics." However, the full citation is a little weaker as it includes "though more moderately expressed" between "similar" and "feeling" (Kuhn 1970, 163).

Kuhn was the head of the *AHQP* project (Kuhn et al. 1967) and in this capacity he became familiar with the quantum controversy, but only with the initial controversy until the early 1930s. According to the project report, "to reduce preparation time and also the number of men to be interviewed, the period to be covered systematically by interviews was terminated in the very early thirties rather than at

the end of that decade as originally planned" (Kuhn n.d.). Thus, the revival of the hidden variables in the early 1950s, to use Max Jammer's words (Jammer 1974, 278), was beyond the scope of the *AHQP*. Furthermore, a quick perusal of the unpublished documents deposited in Thomas S. Kuhn Papers at MIT, in particular folders concerning his correspondence with Feyerabend, Lakatos and the *AHQP* project, did not provide evidence of Kuhn's interest in the debates about the interpretation of quantum theory, neither before nor after his writing of *Structure.* As enticing as Beller's suggestion is, it lacks plausible corroboration with documentary evidence. Thus let us go beyond Beller's argument and exploit Kuhn's attitude to the controversy over the interpretation of quantum physics further.

Beyond Beller's Criticism – What Kuhn Missed from the Quantum Controversy

Kuhn lived to see the quantum controversy play a role in the development of our own understanding of quantum theory as well as receive the attention of historians and philosophers. I am mainly speaking of the whole work—both theoretical and experimental—related to Bell's theorem, which led to the acknowledgment of entanglement as an irreducible quantum feature. Bell's theorem contrasted quantum theory with any attempt to complete quantum theory having local realism as an assumption. It was published in 1965 and from the early 1970s to the early 1980s there was a rush to perform experiments to decide on the disjunction carried out by Bell's theorem (Freire Jr. 2006). Experiments were resumed in the late 1980s exploiting technical advances (taking as sources of photon pairs photons from parametric down conversion in non-linear crystals) and merging these experiments with the then burgeoning field of quantum information. The experiments confirmed the quantum predictions, thus confirming much of the strangeness of quantum theory but they also helped to direct attention beyond physics to the debates on the foundations and interpretation of the quantum theory.

It is noticeable that the interest in foundations of quantum physics triggered by activities related to Bell's theorem did not pass unnoticed by historians, sociologists and philosophers of science. One of the most remarkable cases is the writing and publication of *The Philosophy of Quantum Mechanics – The Interpretations of Quantum Mechanics in Historical Perspective*, in 1974, by the historian of physics Max Jammer.

In fact, Max Jammer seems to have been the first author to grasp the historical relevance of the subject. He had intended to write a book on the development of relativistic quantum mechanics and quantum field theories after the completion of his book on the conceptual origins of quantum mechanics. However, his plans were changed when he realized that a new and more urgent subject had appeared.

Since Jammer's own appraisal of his change of plans is so evidential of the intellectual climate in the early 1970s concerning the foundations of quantum theory, I beg the reader's pardon for quoting in extenso his preface, written in 1988, to the second edition of his *The Conceptual Development of Quantum Mechanics* (Jammer 1989, emphasis is mine).

> As stated in the preface to the first edition in 1966, I had hoped to continue this line of research with a sequel volume on the conceptual development of relativistic quantum mechanics and quantum field theory. However, John Stewart Bell's paper on hidden variables which appeared in the July 1966 issue of the "Reviews of Modern Physics", together with his paper on the Einstein-Podolsky-Rosen paradox, threw new light on the interpretations of quantum mechanics. They initiated a development in which, among many others, the experimentalist John F. Clauser, who at that time attended my lectures at Columbia University in New York, and the theoretician Jeffrey Bub, with whom I had long discussions at the Minnesota Center for the Philosophy of Science in Minneapolis, were actively involved. *Prompted by these developments, I wrote "The Philosophy of Quantum Mechanics"* (Wiley-Interscience, New York, 1974) […]
>
> The reader may perhaps wonder why a book on the development of modern physics, dealing with historical issues that apparently are "faits accomplis", should have to be revised and emended, especially as Werner Heisenberg and Paul Dirac had approved the final draft. The reason is, of course, that the development of quantum mechanics, said to have reached its apex about sixty years ago, is nevertheless still an unfinished business today.
>
> In fact, the conceptual revolution brought about by quantum mechanics is so radical and penetrating that any theoretical innovation discovered today is apt to produce a re-interpretation and re-evaluation of results obtained in the past. A good example is the 1935 Einstein-Podolsky-Rosen Paradox which was presented at the end of the 1966 edition, but whose real significance became clear only through the above-mentioned work of Bell and his followers beginning in 1966.

Jammer was not the only scholar to have his attention diverted by the renewal of the controversy on quantum theory. Still focusing on the 1970s and the 1980s, we may cite studies by Pinch (1977), Brush (1980), Harvey (1980), Harvey (1981), Benzi (1988), Cross (1991), Graham (1972), in addition to works in the philosophy of science, such as Redhead (1987), and popular science books,

such as Bernstein (1991). Noteworthy is the interest that the then new sociology of science, from the Edinburgh school, dedicated to the subject, with the works by Pinch, Harvey and Cross. Surely, the very existence of an ongoing scientific controversy attracted the attention of new practitioners of sociology of science; after all, scientific controversies cannot be understood by framing them exclusively with theoretical and experimental reasons. Other factors, even non-cognitive ones, need to be used in order to render them as intelligible events. In addition to the controversy related to non-locality, we would also like to point out the entire work on the very existence of a measurement problem in quantum theory was developed in the 1950s and the 1960s, but this need not be a concern here. In the 1980s, there were in fact two major intellectual events related to quantum theory. Firstly, physicists widely accepted entanglement, or quantum non-locality, as a new physical feature predicted by theory and corroborated by experiments. Second, historians, sociologists and philosophers were working on the process—a scientific controversy—which had led to the establishment of this new physical feature and its philosophical meanings, and to taking sides in the ongoing controversy.

Thus one may ask why Kuhn did not, as far as I am aware, say something on the debates on the foundations of quantum theory. Resuming Beller's views of the paradigm as a totalitarian concept for the practice of science with its dogmatic roots in the way the Copenhagen interpretation was preached and accepted, it is fair to ask if indeed the paradigm idea did not play its role in preventing Kuhn from noticing and valuing the renewed controversy over quantum theory. However, as we have seen, there is scant documentary evidence for Beller's claims. Thus, they will remain an overstatement, albeit a plausible one. The fact remains that Kuhn had all the skills as a physicist, historian and philosopher to contribute to the analysis of these debates, and he missed the opportunity. At the very least, we deal here with a weakness, or a lacuna, in Kuhn's reflections and legacy.

Philosophers and historians continued to be attracted to the study of the quantum controversy. Directly or indirectly related to it, we may list the following studies, among others, in the last two decades.[1] Taking these considerations as backdrop, it is easier to understand why this continued and enlarged scholarly work in the history and philosophy of science did not enter into a dialogue with Kuhn's works. In brief, Kuhn was silent on the major intellectual events related to the quantum controversy. In the rare cases where there was a dialogue, such

[1] Beller (1999); Bromberg (2006, 2008); Byrne (2011); Camilleri (2009a, 2009b); Cushing (1994); Forstner (2008); Freire Jr. (1999, 2003, 2004b, 2005, 2006, 2007, 2009, 2011a, 2011b, 2015); Freire and Lehner (2010); Freire Jr., Pessoa Jr. and Bromberg (2010); Gilder (2008); Howard 2004); Jacobsen (2007, 2012); Kaiser (2007, 2012); Olwell (1999); Osnaghi, Freitas and Freire Jr. (2009); Paty (1993, 1995); Pessoa Jr. (1998); Pessoa Jr., Freire Jr. and De Greiff (2008); Schlosshauer (2011); Wick (1995).

as Beller's, it resulted in a strong criticism of Kuhn's views. In a lower tone, at the beginning of my research on these subjects, I claimed that Kuhn's view could not help us understand the renewal of the quantum controversy (Freire Jr. 1999).

Conclusion

In defense of Kuhn, one can argue that historians and philosophers may be influenced by recent and contemporary science, but they do not necessarily follow contemporary science. I illustrate this point with two cases related to the quantum controversy. The first one is close to Kuhn, as it involves one of his students and enduring correspondents, the historian Paul Forman. The connections between Forman's claim of the social roots of acausality in quantum mechanics and the causal interpretation of quantum mechanics suggested by Bohm in 1952 are conspicuous. While Forman was aware of Bohm's work, he had not been particularly influenced by it. Instead, he was influenced by Einstein's enduring criticism of quantum mechanics, which Forman read as a quest for determinism. "What did impress me was Einstein's attachment to the goal of causal description," Forman recently recalled (Freire Jr. 2011a). The second one concerns Abraham Pais who, despite concluding his biography of Einstein after Alain Aspect's influential experiments on Bell's theorem in 1982, missed the far-reaching influence of the EPR *Gedankenexperiment* in twentieth-century physics (Pais 1982, chapter 25c).

Let us conclude by dismissing Beller's stronger claim and assuming a weaker one, that is, that Kuhn's inclinations in the quantum debate, even keeping silent on the renewal of the controversy, were in agreement with Bohr and Heisenberg, rather than with Einstein and Bohm. Let us assess this weaker statement. This stand did not contribute to Kuhn's appreciation of the intellectual, far-reaching meaning of the renewal of the controversy over the quanta. As we have seen, Kuhn was neither the first nor the last philosopher of science to take sides in the quantum dispute. In fact, almost all the philosophers of science in the twentieth century with some training in physics involved themselves in this controversy. Let us push the question further, could it have been otherwise? Since historians and philosophers do not work in cultural vacuums, I do not think so. The influence of achievements in quantum theory and the complementarity views on Kuhn are no different from the influence of Newtonian science on Kant's epistemology, the French revolution on Hegel's philosophy of history, or Darwinian evolution on current evolutionary epistemology. If Kuhn's philosophy was biased by the influence of quantum physics, and it seems to us it was, he was not alone in this kind of influence. Ultimately Kuhn did and had to have views on recent and contemporary science, views that shaped, at the very least, his research choices.

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