Constructing the Myth of the Copenhagen Interpretation

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According to the standard view, the so-called 'Copenhagen interpretation' of quantum mechanics originated in discussions between Bohr and Heisenberg in 1927, and was defended by Bohr in his classic debate with Einstein. Yet recent scholarship has shown Bohr's views were never widely accepted, let alone properly understood, by his contemporaries, many of whom held divergent views of the 'Copenhagen orthodoxy'. This paper examines how the 'myth of the Copenhagen interpretation' was constructed by situating it in the context of Soviet Marxist critique of quantum mechanics in the 1950s and the response by physicists such as Heisenberg and Rosenfeld.

1. The Historiographical Problem of the Copenhagen Interpretation

The emergence of the so-called 'Copenhagen interpretation' of quantum mechanics has been the subject of much interest, both historical and philosophical. As Suzanne Gieser points out, "Many attempts have been made to characterize and analyse the philosophical and epistemological position of the Copenhagen School and especially of Bohr, and its significance in the emergence of the definitive interpretation of quantum mechanics" (Gieser 2005, 56). According to the familiar historical account, what we now refer to as the 'Copenhagen interpretation' had its origins in discussions between Niels Bohr and Werner Heisenberg in the latter part of 1926 and early 1927. The new interpretation, which emerged in the spring of 1927, with the publication of Heisenberg's paper on the uncertainty relations, was subjected to a searching critique at the Solvay conference in October 1927. After withstanding the attacks from Einstein, the Bohr-Heisenberg view quickly found support among such leading physicists as Pauli, Dirac, Born, Jordan and Ehrenfest, thus bringing to an end

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one of the most significant phases in the development of modern physics. Dugald Murdoch expresses the standard view:

> By the end of 1927 the Copenhagen interpretation had established itself as the dominant interpretation of quantum mechanics. Central to that interpretation were the ideas of Bohr, the main outlines of which were presented at the Centennial Conference in Como in September 1927 and the Fifth Solvay Conference in Brussels in October that same year. At the latter conference Einstein first voiced his doubts about the newly emerging orthodoxy. (Murdoch 1994, 303)

The 'Copenhagen interpretation of quantum mechanics', so this story goes, quickly established itself as the orthodoxy in physics, in spite of the persistent criticisms of physicists such as Einstein, Schrödinger, Planck and von Laue throughout the 1930s. But what exactly is the Copenhagen interpretation? This question turns out to be far more difficult to answer than might be imagined. As Henry Stapp notes, although there is an extensive literature which discusses and criticises the Copenhagen interpretation, one is immediately struck by the diversity of views "in prevailing conceptions of the Copenhagen interpretation" which appear in the literature (Stapp 1972, 1068). One of the key reasons for this, as many authors have pointed out, is that Bohr and Heisenberg never set out in any clear fashion the basic commitments of the Copenhagen interpretation, and their writings are somewhat ambiguous and elusive on this point. Here lies the central problem facing the historian wishing to make sense of the emergence of the orthodox interpretation of quantum mechanics. While the writings of physicists like Bohr, Born, Heisenberg, Pauli, Dirac, Jordan and von Neumann are generally thought to form the basis of the Copenhagen viewpoint, they seem to have been written more in what Heisenberg referred to as the *"Kopenhagener Geist",* rather than providing a definite unified agreement on the interpretation of the theory (Heisenberg 1930, x).

Bohr's idea of complementarity is generally regarded as the central plank of the Copenhagen interpretation. As Murdoch puts it: "Bohr's construal of quantum mechanics established itself as the basis of the orthodox interpretation—the 'Copenhagen interpretation'" (Murdoch 1987, 179). However, as has been repeatedly pointed out, Bohr's views were not well understood, nor widely accepted, among even his closest collaborators, many of whom adopted divergent philosophical points of view. Although many of the leading physicists of the 1930s, including Pauli, Heisenberg, Jordan and Rosenfeld declared themselves to be enthusiastic supporters of the Bohr's idea of complementarity, opinions differed widely

as to its epistemological and ontological meaning (Kragh 1999, 211; Jammer 1974, 202). Indeed, over the last thirty years, it has become increasingly apparent that after 1927 there remained unresolved fundamental philosophical differences and disagreements between various physicists in Bohr's circle. To this extent, Mara Beller argues:

> Full acknowledgement of its contradictions casts great doubt on the existence of a common basis for the "Copenhagen interpretation", and perhaps on the notion of a conceptual framework in general . . . It is difficult to find a common denominator, any idea, commitment which was not broken at one time or another. This is not to say there was no consensus about radical departure necessitated by the quantum theory. It is the exact and consistent agreement about what would constitute such a departure that was lacking. (Beller 1999, 187–8)

Notions like wave-particle duality, indeterminism, and the indispensability of the classical concepts are frequently cited as part and parcel of the Copenhagen interpretation, yet as Beller points out, "none of them commands unwavering commitment even from Bohr's closest collaborators" (Beller 1999, 187). Dirac and Wigner, for example, found nothing substantially new or enlightening in Bohr's notion of complementarity, and instead saw Heisenberg's paper on the uncertainty principle as the decisive turning point in resolving the difficulties that had plagued physicists throughout the 1920s.[1] This attitude stands in sharp contrast with physicists like Rosenfeld, Jordan and Pauli, who insisted that Bohr's idea of complementarity was indispensable to any proper understanding of the foundations of quantum mechanics (Pauli [1950] 1994; Jordan 1944; Rosenfeld [1953] 1979).[2]

The difficulties which inevitably emerge from the attempts to make sense of the Copenhagen interpretation stem from the fundamental dis-

1. In his interview with Kuhn, Dirac admitted: "I never liked complementarity . . . It does not give us any new formula" (AHQP). When the report of Bohr's Como lecture arrived in Göttingen in September 1927, Wigner is said to have remarked "Bohr's principle will not change the way we do physics." (*Discussion Sections at Symposium on the Foundations of Modern Physics: The Copenhagen Interpretation 60 Years after the Como Lecture,* 1987, 7) Wigner himself recalled: "When Heisenberg published his article on the Uncertainty Principle, I saw almost at once that he had ended the quantum troubles. I picked up the phone and called someone—it was either von Neumann or Leo Szilard. I said 'Now we can go to sleep. The problem is solved'" (Wigner 1992, 87). It is worth noting that Wigner never mentioned correspondence and complementarity in his writings.

2. In 1933 Pauli went so far as to describe quantum mechanics as the 'theory of complementarity', analogously to the theory of relativity (Pauli [1933] 1980, 7).

agreements which were never settled among the founders of quantum mechanics in the 1930s and which emerged with somewhat greater clarity in the 1950s and 1960s. This has led to the view, which finds its clearest expression in the work of Mara Beller, that we should deny "the very possibility of presenting the Copenhagen interpretation as a coherent philosophical framework" (Beller 1999, 173). One finds a similar point of view in the work of Erhard Scheibe (1973, 9) and John Hendry (1984, 1) and more recently in papers by Catherine Chevalley (1999) and Don Howard (2004). However, recognition of this point raises a number of important questions, which have been only touched on, but to my knowledge have not as yet received any systematic treatment: How are we to account for the appearance (or should we say illusion) of consensus achieved in the absence of any genuine agreement between key protagonists? How did so many different philosophical viewpoints come under the banner of the 'Copenhagen interpretation'? And, what purpose or what philosophical or ideological agendas did the construction of the myth of the "Copenhagen interpretation" serve? While Mara Beller's book *Quantum Dialogue* provides a good starting point for exploring these questions, there is I believe still much more to be said on the subject.

This paper serves as a preliminary exploration of these questions in the hope of gaining a better understanding of what Don Howard has appropriately called the 'myth' of the Copenhagen interpretation. Here I want to pursue the idea, which is to be found in the writings of Chevalley and Howard, that while the *theory of quantum mechanics* is a product of the 1920s, the *Copenhagen interpretation,* contrary to the standard view, is a construction of the 1950s and 1960s. This is not to say that serious attempts were not made to interpret quantum mechanics from a philosophical perspective in the 1930s. But rather that the idea of a unitary interpretation only emerges in the 1950s in the context of the challenge of Soviet Marxist critique of quantum mechanics, Heisenberg's announcement of the unified 'Copenhagen interpretation' in 1955 and Rosenfeld's defence of Bohr's views during this time. However, before we can explore each of these, it is necessary to first examine some of the hidden differences between the defenders of the orthodox view in quantum mechanics which emerged in the period from the 1930 to the 1950s.

2. Niels Bohr and the Philosophical Interpretations of Complementarity

According to Heisenberg, Bohr was "primarily a philosopher, not a physicist" (Heisenberg, 1967, 95). In October 1926 Schrödinger remarked that in his discussions with Bohr in Copenhagen on the problems of quantum mechanics, "the conversation is almost immediately driven into philo-

sophical questions" (Moore 1989, 228). As these observations suggest, epistemological considerations were an integral part of Bohr's approach to physics. The notion of complementarity, which he announced for the first time at the Como conference in September 1927 and which he continued to develop and refine throughout the 1930s, was presented as a new epistemological viewpoint from which to understand quantum mechanics (Bohr 1928).[3] Indeed, for Jordan, Bohr's "idea of complementarity" represented "the most significant result for philosophy that crystallized out of modern physics" (Jordan 1944, 131). Here my aim is not to give an account of Bohr's philosophical view of quantum mechanics—this would take me far beyond the scope of this paper and we already have an extensive literature on that topic—but a brief survey of some of the different philosophical perspectives through which his writings were interpreted, and show how these interpretations formed part of the kaleidoscope of views which would fall under the banner of the 'Copenhagen interpretation'.

Bohr's view of complementarity inspired a range of different philosophical interpretations during the 1930s. The dominant philosophical school during the time was logical positivism which rose to prominence in the late 1920s and early 1930s. It is therefore no surprise to find that many of the contemporaries of the founders of quantum mechanics understood Bohr's notion of complementarity and in particular his emphasis on paying careful attention to conditions of measurement as a clear expression of modern empiricism (Degen 1989, 26). We can see this most clearly in the 1930s through the work of physicists such as Pascual Jordan and Philip Franck, who were keen to enlist Bohr as an ally to the positivist movement.[4] Indeed, for Jordan, though every "investigator will claim the individual right to assume his own position in the finer shades of epistemological methods of interpretation," one cannot avoid the conclusion that positivism is "basically absolutely uniform, among modern quantum physicists and one cannot reject this conception without already rejecting quantum mechanics itself" (Jordan 1944, 159). In this way, Jordan saw the "epistemological attitude which comes to be expressed in modern quantum theory—and conversely in this it receives its most significant support" as that of "positivism." Here Jordan boldly declared that positivism "is the epistemological position of Bohr and Heisenberg" (Jordan 1936, vii). In 1938 Philip Franck, who identified closely with the Vienna

3. Bohr's relevant articles from this period can be found reprinted in volumes 6 and 7 of his collected works (Bohr 1985; Bohr 1996).

4. Ulrich Röseberg has investigated the ways in which Bohr and the logical positivists misunderstood one another (Röseberg 1995).

Circle, published an essay on the interpretation of quantum mechanics aimed to show that Bohr's idea of complementarity was "fully compatible with the formulations of logical empiricism" (Franck 1975, 179).

While logical positivism emerged as the dominant philosophical school of thought that aligned itself with Bohr's view of quantum mechanics and thus with the 'Copenhagen school', it was by no means the only one. In the early 1930s Heisenberg and Weizsäcker engaged in a series of critical discussions in Leipzig with the visiting Kantian scholar Grete Hermann, on how Bohr's view of quantum mechanics could be reconciled with Kant's epistemology. Hermann's work during the 1930s reveals the influence of her background under the particular neo-Kantian school of Leonard Nelson, yet it also owes much to the way in which Heisenberg and Weizsäcker had interpreted Bohr's doctrine of the indispensability of classical concepts (Hermann 1935; 1937a; 1937b; 1937c). The central issue for Heisenberg and Weizsäcker was how Kant's notion of the *a priori* could be transformed in the light of Bohr's idea of complementarity (Heisenberg 1958, 80–83; Heisenberg 1971, 117–124; Weizsäcker 1941a; 1941b, 1952).[5] As Heisenberg explained in 1934, "the question raised by Kant, and much discussed ever since, concerning the *a priori* forms of intuition and categories . . . has been put into new light," for "as Bohr particularly has stressed, the applicability of these forms of intuition, and of the law of causality is the premise of every scientific experience even in modern physics" (Heisenberg 1934, 700). In 1942, Heisenberg argued that space and time are "more than merely empirically given, because they are, as Kant rightly emphasizes . . . the first presupposition of all possible experience" (Heisenberg [1942] 1984, 284). Whereas Jordan and Franck saw Bohr's viewpoint as signifying the triumph of positivism, Weizsäcker saw it as a vindication of Kant's fundamental insight into the nature of human knowledge. As he was to put it: "The alliance between Kantians and physicists was premature in Kant's time, and still is; in Bohr, we begin to perceive its possibility" (Weizsäcker [1966] 1994, 185).

The views of the Jordan and Heisenberg stand in sharp contrast to those of the Belgian physicist Léon Rosenfeld, who was a Marxist and Bohr's assistant in Copenhagen in the 1930s. After the Second World War, Rosenfeld became one of the staunch defenders of Bohr's viewpoint of complementarity, which became identified with the Copenhagen interpretation of quantum mechanics. In vigorously defending Bohr against the charges that he was a positivist or an idealist in the 1950s, Rosenfeld presented a dialectical-materialist interpretation of complementarity, ar-

5. A more thorough analysis of Heisenberg's transformation of the Kantian notion of the *a priori* can be found in Camilleri (2005b).

guing that it constituted "the first example of a precise dialectical scheme" (Rosenfeld [1953] 1979, 481; See also Rosenfeld [1953] 1979; Rosenfeld [1957] 1979). Jacobsen's recent work has shed new light on the critical role that dialectical materialism played in Rosenfeld's defence of Bohr's complementarity from the late 1940s and through the 1950s (Jacobsen 2007). While Rosenfeld occasionally expressed some reluctance about labelling Bohr a dialectical materialist, on other occasions he was happy to state his view, in private correspondence, that Bohr was a Marxist who simply wasn't aware of it (Jacobsen 2007, 17).

A more complete picture of the different philosophical perspectives within the Copenhagen School would require that we examine the writings of Wolfgang Pauli and Max Born, but this is a task which would take us beyond the scope of this paper. Suffice it to say, there is no single philosophical position which emerged from the 1930s underpinning what we commonly refer to as the 'Copenhagen interpretation of quantum mechanics'. As Max Jammer points out in his *Philosophy of Quantum Mechanics,* "the Copenhagen interpretation is not a single, clear-cut, unambiguously defined set of ideas" that can be "necessarily linked with a specific philosophical or ideological position." Rather, it serves as "a common denominator for a variety of related viewpoints" each of which have been associated with a variety of "philosophical views, ranging from strict subjectivism and pure idealism through neo-Kantianism, critical realism, to positivism and dialectical materialism" (Jammer, 1974, 87). Many of these philosophical views were, at one time or another, attributed to Bohr by both his defenders and critics.

Bohr's failure to explain his ideas clearly and unambiguously have become part of the folklore of the history of quantum mechanics. This is of course not to suggest that Bohr did not present a philosophically coherent, and immensely interesting, interpretation of quantum mechanics, but there is undoubtedly a sense in which Bohr's writings and utterances had the distinctive quality of lending themselves, perhaps without the benefit of careful study, to a range of different philosophical interpretations. Though Bohr himself rejected many of the 'isms' of contemporary philosophy, he was at the same time reluctant to publicly criticize different philosophical interpretations of his thought. In his interview with Kuhn in 1962 Bohr lamented that "no man who calls himself a philosopher understands what one means by complementary description" (AHQP), yet it is difficult to find Bohr ever publicly criticizing his colleagues in print. Indeed, as Pauli commented on the occasion of Bohr's sixtieth birthday:

> Bohr himself integrated, in lectures at international congresses and at those carefully planned conferences in Copenhagen, the diverse

> scientific standpoints and epistemological attitudes of the physicists, and thereby imparted to all participants in these conferences, the feeling of belonging, in spite of all their dissensions to one large family. (Pauli [1945] 1994, 51)

Throughout the 1930s and 1940s Bohr was taken as an ally in a number of distinct philosophical movements including certain strands of neopositivism, neo-Kantianism and dialectical materialism. One reason that Bohr's writings were so readily adapted to different philosophical positions is that many of his contemporaries saw it as their task to clarify Bohr's views, which were often not expressed quite as clearly as they might have been. Thus, in 1930s Philip Franck took the view that while Bohr was on occasions prone to lapse into metaphysics, his basic outlook were entirely in keeping with the positivism of the Vienna Circle. To this end, Franck saw it as his task to carry out a logical clarification of Bohr's writings and to say "what Bohr really meant" (Franck 1930; Röseberg, 1995). Also approaching the task of 'clarifying Bohr', but from an entirely different philosophical standpoint, the Russian physicist Vladimir Fock insisted Bohr was essentially correct but voiced his concern that Bohr's unfortunate use of terminology had given "rise to many misunderstandings and to an incorrect interpretation . . . in a positivistic [or subjectivist] sense" (Fock 1957, 646). In 1958 Fock explained that "it is not to be supposed that the use of such expressions reflects any subjectivity on Bohr's point of view; without question this is simply carelessness, and there is no real need to comment on such imprecise expressions" (Fock, 1958, 210). By the 1960s, many Soviet critics saw Fock as having successfully reconciled Bohr's view of complementarity and the 'orthodox' interpretation with a materialist point of view (Graham, 1988, 311–313).

Different philosophical schools of thought seized on the opportunity to reinterpret Bohr's interpretation of quantum mechanics by recasting it in their own distinctive philosophical voice. Yet, by the 1950s the diversity of epistemological viewpoints, each of which aligned themselves with Bohr's view, actually contributed to the impression of a unitary Copenhagen interpretation. Whereas in the 1930s and 1940s the disagreements between Rosenfeld, Franck and Weizsäcker are best construed as a clash of different philosophical interpretations of quantum mechanics, as had been in the case with the theory of relativity (Hentschel 1990), by the mid-1950s in the context of the emergence of a new threat from the Bohm, de Broglie and Vigier, as well as Soviet physicists such as Blokhintsev and Alexandrov, the different schools of thought closed ranks in identifying themselves with Bohr—the canonical author—whose writings were was taken as a direct expression of the 'authentic' Copenhagen interpretation.

To see how this happened we need to turn our attention to debate over quantum mechanics in the intellectual and political context of the Cold War.

3. Soviet Marxism and the Subjectivist Idealism of the Copenhagen School

In the late 1920s and 1930s, a few prominent physicists, notably Einstein, Schrödinger, Planck and von Laue expressed their discontent at the attitude of many physicists regarding quantum mechanics. The key issue here was not whether quantum mechanics was an empirically correct theory—it had received strong experimental confirmation—but whether it could be considered in some sense a 'complete' description of the underlying reality. Writing to Sommerfeld after the Solvay conference in 1927 Einstein explained that in his view quantum mechanics "might be a correct theory of statistical laws, but it still is an insufficient conception of the individual elementary processes" (Einstein to Sommerfeld, 9 November 1927, in Bohr 1985, 41). For Einstein, the "thesis that the ψ-function characterizes the individual system *exhaustively*" was the basic attitude of those who subscribed to what he termed "the orthodox view" (Einstein 1949, 681). And indeed this view was defended in one form or another by the physicists of the 'Copenhagen school' (Scheibe 1990). To this extent, there does appear to have been widespread agreement on this point among a number of physicists—though this should not be taken mean there was unanimity concerning the *interpretation* of quantum mechanics.

The debate between Bohr and Einstein reached its climax in 1935 with the publication of the EPR paper and Bohr's subsequent reply. While the debate was of considerable philosophical interest, it did little to inspire physicists to search for a more complete theory of quantum mechanics. However, this situation would change dramatically in the 1950s with the appearance of a series of publications by physicists such as David Bohm, Louis de Broglie, Jean-Paul Vigier on a hidden variables interpretation of quantum mechanics, as well a number of papers by Schrödinger, Janossy and Blokhintsev which renewed the attack on the orthodox view.[6] According to Max Jammer: "In the early 1950s the almost unchallenged monocracy of the Copenhagen school in the philosophy of quantum mechanics began to be disputed in the West" (Jammer 1974, 250–251). While Jammer here speaks in a way which misleadingly suggests there existed a single unified point of view shared by the Copenhagen school, he does draw attention to a critical turning point in the history of the debate over the interpretation of quantum mechanics. Significantly, Jammer also

6. See Bohm (1952), de Broglie (1953; 1956; 1957), Vigier (1956), Schrödinger (1952), Janossy (1952), Blokhintsev (1952a; 1952b; 1953).

suggested, with considerable insight, that: "The extent to which this process was fomented and supported by social-cultural movements and political factors such as the growing interest in Marxist ideology in the West deserves to be investigated" (Jammer 1974, 250–1). This is an important clue, not only to understanding the emergence of the new wave of criticisms of the 'Copenhagen orthodoxy' in the 1950s, but to understanding the construction of the image of the Copenhagen interpretation itself.[7]

In her work on the history of the interpretations of quantum mechanics, Mara Beller argues that it was the 'victors' who perpetrated the illusion of agreement among themselves (Beller 1999, 188–90). However, as I hope to make clear, this is only part of the story. It was not only the defenders of the orthodoxy who gave the impression that there was a unified philosophical viewpoint lay behind the new quantum mechanics, but their opponents. Nowhere is this more evident than in the case of the Soviet critique of quantum mechanics, which reached its crescendo in the early 1950s. Thanks to the work of historians of science like Loren Graham (1966; 1972; 1988), Olival Freire (1997; 2004), Andrew Cross (1991), and Alexander Vucinich (1991), we are now in a much better position to understand the impact of Soviet-Marxist ideology on the debates over quantum mechanics in the 1950s, and the impact they had on physicists in the West. My interest, however, is not in emergence of a visible heterodoxy in 1950s, but rather the impact Soviet Marxist views may have had in the formation of the image of the 'Copenhagen interpretation' during this time.

The ideological attack on the 'Copenhagen School' of quantum mechanics was closely tied to the Soviet critique of positivism inspired by Lenin's widely read *Materialism and Empirio-Criticism.* As Wolfhard Boeselager explains, whereas the logical positivists saw themselves as opposed to all metaphysics, transcendentalisms, and idealisms as obscurantist and regressive modes of thought, for the Soviet Marxists these were precisely the hidden agendas of the rise of modern positivism. Soviet Marxists universally condemned positivism as using the terminology of science in the interests of "subjective idealism." As Boeselager explains, it is characteristic of the Leninist method to show that "positivists in reality hold views different from those which they proclaim." To this extent, the Soviet critique of neo-positivism was characterised by a tendency to depict positivism as an anti-scientific and pro-religious "idealist" bourgeois philosophy, in

7. Catherine Chevalley hints at the possibility, but leaves it at that: "it does not seem exaggerated to conjecture that the notion of the Copenhagen interpretation was *identified to the views of Bohr's group within a political and intellectual context widely different from that of the 1920's*" (Chevalley 1999, 62–3 emphasis in original).

spite of claims of its adherents to the contrary. It was also characteristic of Soviet critics of positivism "to lump together authors with sometimes very different views." This technique was also applied to authors from different eras—like Berkeley or Hume—whose views had already been condemned (Boeselager 1975, 38).

The Leninist critique of idealism in physics is in clear evidence in the Soviet attack on the orthodoxy in quantum mechanics which can be traced to the 1930s, but which emerges with greater force in the 1950s. As Vucinich explains for many of the Marxist critics such as Uranovskii and Maksimov "identified physical idealism, a label used interchangeably with 'subjectivism' and 'agnosticism', as the official philosophy of quantum mechanics as presented by the Copenhagen school." This was a term which "covered *all epistemological orientations which violated Lenin's view* that knowledge reflected the external world and which challenged the role of causality as the basic explanatory position in science" (Vucinich, 1991, 238 emphasis added). This is a critical insight into the invention of the myth of the Copenhagen interpretation, for it shows us that the 'Copenhagen interpretation' was as much a construction of those who opposed the orthodox view (that quantum mechanics gives a complete description), as those who defended it.

While before the Second World War, there was some debate within the Soviet Union over the interpretation of quantum mechanics, physicists, after the Second World War the "orthodox interpretation" was officially banned. The defining event was the speech delivered by Stalin's assistant in the Central Committee of the Party, Andrei A. Zhdanov on June 24, 1947. The occasion was a discussion and condemnation of G. F. Aleksandrov's book *History of Western European Philosophy.* While Zhdanov only referred to modern physics briefly in the conclusion of his speech, this marked the beginning of an intense ideological campaign—sometimes referred to as *Zhdanovshchina* in the literature—which had a profound effect on Soviet physics, including the interpretation of quantum theory. The major consequence of all this was a decision reached at the 1947 Meeting of the Academy of Sciences to effectively banish 'complementarity' from the Soviet physics from 1947 to 1958. Any acceptable interpretation of quantum mechanics would have to assert that electrons were 'objectively real' particles and that the theory was perfectly compatible with dialectical materialism.

A surge of publications condemning the 'orthodox' interpretation of quantum mechanics emerged from the Soviet Union during this time. The critics counted among others the physicists A. A. Maksimov, A. D. Aleksandrov, Ia. P. Terletskii, B. G. Kuznetsov, and D. I. Blokhintsev. Perhaps the most important and influential critic was Blokhintsev, whose

paper "Criticism of the Philosophical Views of the Copenhagen School," was published in Russian in 1952. Importantly from our point of view, this paper contains, so far as I can tell, the first mention of the "Copenhagen school." A German translation of the paper appeared in 1953 in the journal *Sowjetwissenschaft.* There Blokhintsev declared: "Among the different idealistic trends in contemporary physics, the so-called 'Copenhagen school' is the most reactionary. The present article is devoted to the unmasking of idealistic and agnostic speculations of this school on the basic problems of quantum mechanics" (Blokhintsev 1953, 546). Blokhintsev's description of his task as "unmasking" the implicit idealist viewpoint of the Copenhagen interpretation is significant. Here the subtle differences between different proponents of neo-positivism are deemed to be trivial. In the spirit of Lenin, there are only sides—materialism and idealism—in the battle against bourgeois ideology. Though certain physicists of the Copenhagen school might try to deny any commitment to positivism, it was the task of the Marxist scholar to unmask and expose the hidden "idealism". The same approach can be found in Blokhintsev's paper, "Critique de la conception idéaliste de la théorie quantique" which was published in French in 1952 by *Les Éditions de Nouvelle Critique,* a publisher with strong ties to the French Communist Party. Here the author claimed that Bohr's *"principle of complementarity is directly the fruit of the idealist positivist theory of knowledge"* (Blokhintsev 1952b, 105 emphasis in original). Soviet Marxist critics of the orthodox view in quantum mechanics did not deem it necessary, or even worthwhile, to make a careful study of the philosophical views of Bohr, or Heisenberg, or Pauli, but simply to critique the subjective idealism which they saw as underpinning the Copenhagen point of view.[8]

Andrew Cross has documented the considerable support that the Soviet critique of quantum mechanics enjoyed in France in the early 1950s, where a number of Marxist physicists expressed their discontent at the "subjectivism" and "idealism" at the heart of the orthodox view (Cross 1991, 746–751). In a letter to Pauli in 1952 Rosenfeld wrote that in Paris "the youth is in arms against [Bohr] 'under the banner of Marxism'," though Rosenfeld hastened to add that in his view this was against the spirit "of what Marx had really meant" (Pauli to Rosenfeld, 20 March 1952 in Pauli 1996, 592). Among the leading physicists of this movement was Jean-Paul Vigier, whose Marxist leanings were a source of motivation in the further development of a new hidden-variables theory of quantum mechanics, which had emerged from the work of David Bohm

8. This is true of later Marxist critiques of the Copenhagen interpretation of quantum mechanics which appeared in the 1970s. See Thekadath (1974) and Jayaraman (1975).

(Cross 1991; Freire 2004).[9] The characterization of the orthodox view of quantum mechanics as "idealist" can be found even among many prominent French physicists during this time, even those who were not Marxist in their orientation. This is evident in the writings of Louis de Broglie, who in 1953 argued the "present day interpretation," which he objected to, inevitably ended in subjectivism:

> Actually this interpretation by seeking to describe quantum phenomena solely by means of the continuous ψ-function whose statistical character is certain logically ends in a kind of subjectivism akin to idealism in its philosophical meaning, and it tends to deny the existence of a physical reality independent of observation. (de Broglie 1954, 235)

In the United States, the Marxist philosopher of science Hans Freistadt complained of a "crisis of physics" brought about by the positivist interpretation of quantum mechanics, which was nothing but a thinly disguised form of 'subjective idealism' (Freistadt 1953). Mario Bunge was another who criticised the 'idealism' at the heart of the Copenhagen school in the 1950s. In 1955 Bunge argued: "the celebrated crisis of determinism is nothing but a consequence of the adoption of an idealist theory of knowledge: it is not a simple result of modern physics, but a tenet of neopositivism" (Bunge 1959, 181). Indeed Bunge's critique of the orthodox interpretation is a model of the Leninist method of critique outlined earlier. The Copenhagen school of quantum mechanics is charged with defending a positivistic standpoint. The label positivism is used interchangeably with subjective idealism. Schlick's positivism is identified as irrational (1959, 191). Rosenfeld is denigrated as a positivist in spite of his claims to the contrary. Weizsäcker is branded a "theologian" (1959, 183). And the "subjective idealism" of the orthodox view is equated with the outmoded and already dispensed with metaphysics of Berkeley (1959, 178–9). Drawing the fundamental battle lines of the debate over quantum mechanics, Bunge declared: "What is at stake in this discussion in not a physical dispute about the structure of micro-objects, but the whole theory of knowledge with its old struggle between materialism and immaterialism" (1959, 178).

9. In the early 1950s Vigier published a number of articles on quantum mechanics from a Marxist perspective. See Vigier (1954; 1955; 1957). Bohm himself had strong Marxist leanings, but there is no evidence to suggest that his commitment to dialectical materialism was a source of motivation in the development of his hidden variables theory in the early 1950s. Christian Forstner has argued that only "after Bohm had submitted the article to the Physical Review" did he "reconsider his theory on the basis of the dialectical materialism" (Forstner 2005, 8).

While the polarization of the debate over quantum mechanics was not always interpreted in the West as a clash between Marxist-materialism and subjective idealism, there was nevertheless a tendency in the 50s and 60s to view the debate between orthodox and heterodox as a clash of two diametrically opposed philosophical points of view—positivism and realism. As Alfred Landé put it in 1960: "The positivist viewpoint of Bohr and Heisenberg is of course diametrically opposed to the realism of Einstein" (Landé 1960, 86–7). In 1967 Mario Bunge observed that the "turning of the tide" in contemporary philosophy away from the positivism of the Vienna Circle had, not surprisingly, coincided with the emergence of 'realist' alternatives to the Copenhagen interpretation of quantum mechanics (Bunge, 1968). This tendency to characterize the Copenhagen interpretation as resting on "a positivist attitude" remains widespread to this day (Cushing 1994, 27). Chevalley nicely expresses this way of understanding the debate in physics:

> The majority of usual expositions of the divergence of the interpretations of quantum theory translate this divergence in terms of a conflict between two positions, that of positivism and that of realism . . . In fact, the positivist-realist alternative appeared in the fifties-sixties in connection with the efforts to develop a determinist type of theory (hidden variables) and it had as its aim a simplified description of the situation, to the detriment of the divergences which existed among the founders of quantum mechanics at the time in the Copenhagen-Göttingen group (between Bohr, Pauli, Heisenberg, Born, Dirac, and Jordan) and among their opponents (between Einstein, Schrödinger and de Broglie) . . . But this [picture] has served to impose a completely erroneous vision. (Chevalley 1992, 67–8)[10]

In spite of this all-too-common characterization, it now seems clear that Bohr was not a positivist, and was frustrated by the failure of members of Vienna Circle like Franck to understand complementarity (Röseberg 1995). In fact, as Anton Degen points out, "*most of the founders* of quantum mechanics did not subscribe to a positivist philosophy of science, even though they may have had leanings in that direction earlier in their careers" (Degen 1989, 17 emphasis added). This is particularly evident in the case of Pauli and Heisenberg, whose writings in the 1920s bear the

10. The recent literature on Bohr has to a large extent, corrected this misconception that he was a positivist. See Folse (1985), Murdoch (1987), Honner (1987), Faye (1991), Faye and Folse (1994). For a more complete picture of Einstein's unique version of 'realism', see Fine (1986) and Howard (1993).

influence of Mach's positivism, only to move in very different directions later (Laurikainen 1988; Camilleri 2005a). Not surprisingly in the 1950s there were a series of fervent denials of positivism from the physicists such as Bohr, Rosenfeld, Heisenberg and Pauli.[11] While the positivist-realist dichotomy has served to distort and obscure many of the philosophical views of key protagonists in the debates over quantum mechanics, as we shall see, philosophy was at the heart of the debate in the 1950s.

4. Reconstructing the Orthodoxy: The Invention of the Copenhagen Interpretation

It seems more than just coincidence that the challenge of Bohm's hidden variables theory and the Marxist critique of orthodox quantum mechanics in the 1950s coincides with the first use of the term 'Copenhagen interpretation of quantum mechanics'. As Helge Kragh points out, "the term 'Copenhagen interpretation' was not used in the 1930s but first entered the physicist's vocabulary in 1955 when Heisenberg used it in criticizing certain unorthodox interpretations of quantum mechanics" (Kragh 1999, 210). Catherine Chevalley emphasises that "None of the founders of quantum mechanics—not even Einstein, Schrödinger or L. de Broglie—ever used the term before that time," nor is the expression "to be found in the philosophical literature of that time" (Chevalley 1999, 62). It is therefore worth quoting from Heisenberg's 1955 article where the term first appears:

> The months which followed Schrödinger's visit [in September 1926] were a time of the most intensive work in Copenhagen, from which there finally emerged what is called the "Copenhagen interpretation of quantum theory" . . . From the spring of 1927,

11. Reporting on his conversations with Bohr in Copenhagen in 1957, Fock explained that Bohr had made it clear that "he was not a positivist" (Fock 1957). In 1957 Rosenfeld also objected to the tendency to label Bohr a positivist (Rosenfeld [1957] 1979:498–9). According to Heisenberg, "the Copenhagen interpretation of quantum theory is in no way positivistic. For whereas positivism is based on the sensual perceptions of the observer as the elements of reality, the Copenhagen interpretation regards things and processes which are describable in terms of classical concepts, i.e., the actual, as the foundation of any physical interpretation" (Heisenberg 1958, 127). Similar denials of positivism can be found in Pauli's correspondence. In a letter to Markus Fierz on 6 Jan 1952, Pauli wrote: "What Mr. Bohm found particularly teasing and irritating in me is the circumstance that I declare myself not to be a positivist" (Quoted and translated in Laurikainen 1988, 195). In an exchange of correspondence with the philosopher Franz Kröner in 1953, Pauli strenuously denied that the orthodox view of quantum mechanics was "positivist" in its basic approach to the problem of reality (See Pauli to Kröner, 29 June 1953; Pauli to Kröner 20 October 1953 in Pauli 1999, 184–5, 309–310).

> therefore, there existed a complete, unambiguous mathematical procedure for the interpretation of experiments on atoms or for predicting their results . . . Since the Solvay conference of 1927, the "Copenhagen interpretation" has been fairly generally accepted, and has formed the basis of all practical applications of quantum theory. (Heisenberg 1955, 14–16)

Heisenberg's historical narrative of the genesis of the Copenhagen interpretation has been rehearsed in numerous subsequent treatments of the development of quantum mechanics (Petersen 1968, 1; Miller 1988, 27). As Heisenberg's account above suggests, the Solvay conference marks a significant turning point in the history of quantum physics in establishing the Copenhagen orthodoxy. This was certainly the feeling of many physicists, notably Ehrenfest, who participated in the discussions in Brussels. However this historical reconstruction serves to conceal the internal division between key protagonists such as Bohr, Heisenberg, Born and Dirac, which is evident in the discussion that took place at the 1927 Solvay conference itself, and the fact that important shifts and trajectories in both Bohr's and Heisenberg's thought occurred *after* 1927.[12] Indeed, Rosenfeld later recalled that by 1927 Bohr still "felt that this search for a firm foundation had not yet been completed in quantum mechanics, in spite of the considerable clarification the question had already received from Heisenberg's analysis of the indeterminacy relations." It was not until 1936 that Bohr felt he had brought to a conclusion the "task of deepening and consolidating the conceptual foundation of quantum theory" (Rosenfeld 1967, 115).[13]

The 1927 Solvay conference did bring about widespread agreement that the development of quantum theory had been brought to a "provisional conclusion" through the "systematic construction of the mathematical formalism of wave mechanics" (Pauli [1950] 1994, 36). But here I maintain it did not bring about agreement on how to philosophically in-

12. Important here was the transformation of the respective views of wave-particle duality and complementarity of Heisenberg and Bohr after 1927. Following the publication of the Jordan-Wigner paper on the quantization of matter waves, on 23 July 1928 Heisenberg wrote to Bohr declaring that "I now believe the fundamental questions are completely solved" (AHQP).

13. This account accords with the point made by Murdoch and Faye, according to which Bohr's thought undergoes a transformation in 1935. As Murdoch puts it, "After 1935 Bohr expressed the indefinability thesis in what may be called 'semantic' as distinct from 'ontic' terms" (Murdoch 1987, 145). In a similar vein Jan Faye writes: "After 1935 his grounds for asserting complementarity were not so much epistemological as they were conceptual or semantical" (Faye 1991, 186).

terpret that theory. This task actually *began* in 1927 with Bohr's Como paper, but was taken up by a number of physicists and philosophers in the 1930s and 1940s. Yet Heisenberg's 1955 article gives the impression that a certain philosophical point of view definitely emerged in Copenhagen during this time. As he explained: "What was born in Copenhagen in 1927 was not only an unambiguous prescription for the interpretation of experiments, but also a language in which one spoke about Nature on the atomic scale, and in so far a part of philosophy" (Heisenberg 1955, 16).

It is important to realize that while many of the physicists of the Copenhagen school disagreed with Heisenberg's own philosophical interpretation of quantum mechanics, they agreed with Heisenberg that what was at stake in the debate was fundamentally a philosophical question. In 1953 Born argued that ultimately the controversy surrounding quantum mechanics was "not so much an internal matter of physics, as one of its relation to philosophy and human knowledge in general" (Born [1953] 1956, 140). As Rosenfeld explained in a letter to Pauli: "I deliberately put the discussion on the philosophical ground, because it seems to me that the root of the evil is there rather than in physics" (Rosenfeld to Pauli, 20 March 1952 in Pauli 1996, 587–588). This point is spelled out in the paper, in which Rosenfeld stated that it was his aim to "confine the debate to the field of epistemology, for the crucial issue here is one of logic, not of physics" (Rosenfeld [1953] 1979, 465). Olival Freire argues that "physicists in the early 1950s saw the controversy as a strictly philosophical dispute concerning ontology . . . and epistemology" (Freire 2000a, 23). As late as 1968, Weizsäcker explained that the difficulty some physicists had in understanding and accepting "the Copenhagen Interpretation of quantum theory . . . should not be surprising once we realize that we are here concerned with the basic problems of philosophy" (Weizsäcker, 1971, 25).

But there is something of a paradox here. On the one hand many of the leading physicists of the 'Copenhagen school' saw their battle against the emergence of heterodox views in the 1950s, such as Bohm's, as an expression of a strictly philosophical debate concerning ontology and epistemology, and not of physics. Yet on the other hand, they were in disagreement amongst themselves as to what epistemological point of view was necessary to combat the emerging heterodoxy (Freire, 2005a, 19–28). Rosenfeld, for example, was explicitly critical of Heisenberg's "idealism" (Rosenfeld [1953] 1979, 482). One can imagine his immense displeasure at reading Heisenberg's 1955 article, in which he criticized opponents of the Copenhagen interpretation for their desire to return to "the ontology of materialism" (Heisenberg 1955, 17). Heisenberg concluded his article on a typically 'Kantian' note—"The idealistic argument that certain ideas are *a priori* ideas, i.e. come before all natural science, is here correct" (Heisen-

berg 1955, 28). In a little known critical review of Heisenberg's *Physics and Philosophy,* Rosenfeld expressed his disappointment at the general philosophical attitude in the book:

> With regard to the epistemological problems just mentioned, Heisenberg's exposition naturally follows the line of argument which he has himself so decisively contributed to establish, and which he calls, in homage to Niels Bohr's great leadership, the "Copenhagen interpretation" . . . But the account he gives of the "Copenhagen" ideas is unfortunately not so good as it ought to be; and certainly not one of the physicists now working in Copenhagen would subscribe to the general philosophical attitude underlying this account. Altogether it would be better to discard such an ambiguous expression as "Copenhagen interpretation," were it only because it falsely suggests that there could be other possible interpretations of quantum theory. (Rosenfeld 1960, 831)

In spite of Rosenfeld's reservations about the term 'Copenhagen interpretation', it was to become part of the vocabulary of physicists, as well as historians and philosophers of physics. Indeed, Pauli expressed his delight at reading Heisenberg's "beautiful presentation of the 'Copenhagen interpretation'," particularly his treatment of the ontological and epistemological questions (Pauli to Kröner, 24 February 1955 in Pauli 2001, 120–1). Yet as we have seen, there remained important unresolved philosophical disagreements between the key defenders of the so-called 'Copenhagen interpretation'. This poses an interesting question—how were the internal divisions sustained, while at the same time presenting a unified front? One answer to this question is that the physicists rarely published their philosophical disagreements during this critical period. While Rosenfeld did on a few occasions express his disagreement with colleagues like Heisenberg in public, this was rare. If one examines the correspondence between physicist like Born, Bohr, Heisenberg, Pauli, Rosenfeld and Weizsäcker in the 1950s, one finds a series of disagreements, confusions and misunderstanding, largely about philosophical points, very few of which ever made it to print. A few examples may serve to illustrate this point.

In the 1950s Born and Pauli were particularly critical of Rosenfeld's tendency to interpret Bohr's ideas within a Marxist framework. In a letter to Heisenberg in 1954, Pauli expressed his satisfaction at having managed to exercise some editorial control over the volume commemorating Bohr's seventieth birthday, in preventing Rosenfeld, whom he labeled $\sqrt{Trotsty \times Bohr}$, from using his discussion of complementarity as an opportunity to promote a materialist viewpoint (Pauli to Heisenberg,

13 May 1954 in Pauli 1999, 620–1). Around the same time, Born wrote to Rosenfeld, enclosing a 10-page typed manuscript on "Dialectical Materialism and Modern Physics," in which he took Pauli to task for his tendency to read into complementarity a striking confirmation of Marxist dialectical philosophy. Born decided not to publish the paper (Freire, 2001). Writing to Bohr in 1955 Pauli expressed his puzzlement with Bohr's claim in a recent lecture on 'The Unity of Science' that the "notion of complementarity does in no way involve a departure from our position as detached observers of nature" (Bohr [1955] 1961, 74). From Pauli's point of view, quantum mechanics demanded an *"abandonment of the idea of the isolation (detachment) of the observer from the course of physical events outside himself"* (Pauli to Bohr, 15 February 1955 in Pauli 2001, 104–6 emphasis in original). Bohr replied in his customary conciliatory manner, assuring Pauli, "we have the same view, but I am afraid that we sometimes use different terminology" (Bohr to Pauli, 2 March 1956 in Pauli, 2001, 137). Yet, as Pauli explained in a letter to Kröner in 1956, in his mind this was an issue that he and Bohr had never fully resolved (Pauli to Kröner, 30 November 1956 in Pauli 2001, 784–5). In 1955 Bohr wrote to Weizsäcker, explaining that in a recent paper on complementarity, Weizsäcker (1955) had completely misunderstood what he had meant by the complementary relation between a space-time description and causality (Bohr to Weizsäcker, 20 December 1955, Bohr's Scientific Correspondence 33, 2, AHQP).[14] In this instance Weizsäcker did publish a note along with the paper in his 1963 edition of his book *Zum Weltbild der Physik,* indicating his misunderstanding, but this edition was never translated into English and was not widely available (Weizsäcker 1963, 330). It nevertheless remains one of the very rare cases where a misinterpretation of Bohr was admitted publicly.[15]

14. See also Weizsäcker to Bohr, 17 January 1956; Bohr to Weizsäcker, 28 January 1956; Bohr to Weizsäcker, 5 March 1956, Bohr's Scientific Correspondence, 33, 2, AHQP). In the 1927 Como lecture, Bohr employed the term 'causal description' to refer to the *conservation of energy,* while a space-time description referred to pinpointing the electron's position in space at a given time. Both Heisenberg and Weizsäcker offered a different view of complementarity of space-time and causality, interpreting the causal description of a system to mean a description of the evolution of the ψ-function. Thus for Weizsäcker, the idea of complementarity did not express the mutually exclusive relationship *between* classical concepts, as it did for Bohr, but rather refers to the necessity of invoking two modes of description in quantum mechanics—a classical and a quantum description (See Camilleri 2007).

15. One could perhaps add to this list by mentioning that in 1953 Born wrote to Bohr, complaining that it was unclear to him how one could maintain an objective description if one denied there was anything "behind the phenomena" (Born to Bohr, 10 January 1953; Born to Bohr, 10 March 1953, AQHP). Again we find no mention of this philosophical exchange in the published literature.

Yet, there were other ways in which the key protagonists concealed their disagreements from public view. In Heisenberg's historical account of the emergence of the Copenhagen interpretation in 1927, he did acknowledge that at the time of the discussions with Bohr in Copenhagen "there remained conceptual differences" between he and Bohr, but that these "referred to the different starting points or to the different ways of expressing things, but no longer to different interpretations of the theory" (Heisenberg, 1960, 46–7). One of the key issues which subsequently arose was the 'completeness' of quantum mechanics, which was at the centre of Bohr's debate with Einstein in the 1930s. Yet, little mention is ever made of the fact that Heisenberg's also wrote a reply to EPR in 1935 which was never published, in which he defended the 'completeness' of quantum mechanics on altogether different grounds (Heisenberg, [1935] 1985). After an exchange of correspondence with Bohr in the second half of 1935, Heisenberg decided not to publish, but it appears that there were unresolved differences over the question of whether the dividing line between the measuring instrument and the quantum object can be placed arbitrarily (Heisenberg to Bohr 10 August 1935; Bohr to Heisenberg 10 September 1935; Bohr to Heisenberg 15 September 1935; Heisenberg to Bohr 29 September 1935, Bohr Scientific Correspondence, 20, 2, AHQP).[16]

In the 1950s, in the wake of the work of Bohm, de Broglie, Vigier and others, interest again surfaced in the question of the 'completeness' of quantum mechanics. But if Bohr's views were understood by his colleagues in the 1930s, they were soon forgotten. In examining the discussions that took place in the 1950s, it becomes apparent that there was widespread disagreement and confusion about what should be understood by the term 'completeness'. This was especially noticeable in discussions at the Symposium on *Observation and Interpretation of Modern Physics* in Bristol in 1957 (Körner 1957). Whereas Bohr had appealed to an epistemological analysis of the mutually exclusive conditions of observation, by the 1950s there was no agreement, even among those who defended the 'completeness' of quantum mechanics, what the term really meant, let alone any agreement on what grounds to defend the thesis. Many of the 'orthodox' physicists such as Heisenberg and Heitler simply appealed to the axiomatic or logical completeness of quantum mechanics

16. In his reply to Patrick Heelan's paper in 1975, Heisenberg recalled his disagreement with Bohr in the mid-1930s "on the problem of whether the cut between that part of the experiment which should be described in classical terms and the other quantum-theoretical part had a well defined position or not. I argued that a cut could be moved around to some extent while Bohr preferred to think that the position is uniquely defined in every experiment" (Heelan 1975, 137).

(Heitler 1949, 194; Maheu 1971, 145–6). In his contribution to the de Broglie *Festschrift* in 1952 Pauli referred to von Neumann's proof that the experimentally verified statistical law of wave mechanics is incompatible with the existence of hidden variables (Pauli 1953, 38–9). Taking an altogether different approach in 1957, Marcus Fierz conceded that although "at the moment there is no possibility of proof" that quantum mechanics gives the most complete description possible, one could just as easily question the completeness of 'classical mechanics'. In this context Fierz argued that any physicist searching to find a more complete theory of quantum mechanics "should not only discard Bohr's ideas on what a physical theory can be, but even those of Newton" (Körner 1957, 96).

David Hull, in his book *Science as a Process,* has argued that "the rigidity with which scientists insist on their preferred terminology is matched only by the semantic plasticity of that terminology" (Hull 1988, 295). This is not only true of terms like 'completeness', but also of terms such as 'wave-particle duality' and 'indeterminacy' and 'complementarity' as they were employed frequently in the 1950s. Thus, while we might agree with Edward McKinnon that "the Copenhagen interpretation includes the uncertainty principle; the idea that photons, electrons, and other 'particles' exhibit both wave and particle properties; [and] the probabilistic interpretation of the *wave function,*" (MacKinnon 1985, 110) exactly what each of these terms meant varied for different physicists. As I have argued elsewhere, Bohr and Heisenberg understood the wave-particle duality and the complementarity of space-time and causal descriptions in radically different ways, a fact which was disguised by Heisenberg's rather loose use of the term 'complementarity' in the 1950s (Camilleri 2006; 2007). Indeed Heisenberg attributed his own distinctive view of wave-particle duality and complementarity to Bohr or the Copenhagen interpretation in general (Heisenberg 1958, 50–51).[17]

Many physicists in the orthodox camp such as Heisenberg, Born, Pauli, and Rosenfeld presented themselves as loyal supporters of Bohr's view of wave-particle duality or complementarity, while their writings betray a series of misunderstandings, confusions, and outright disagreement on several key issues. Terms like wave-particle duality, complementarity, indeterminacy and completeness formed part of the shared vocabulary for the Copenhagen school in the 1950s, but beneath the veneer of agreement, one finds hidden disagreements as to what these terms meant. The

17. As Catherine Chevalley puts this succinctly: "Certainly almost everyone would include Complementarity and the Uncertainty Principle [in any account of the Copenhagen interpretation of quantum mechanics]—but then there is no agreement about what is Complementarity, nor is there about the exact status of Heisenberg's principle" (Chevalley 1999, 71).

continual reference to these 'basic tenets' of the orthodox interpretation, in the absence of any genuine consensus over their meaning, was one of key factors which served to give the impression of a unified stance.

By the mid 1960s there were growing signs of a fundamental disunity within the Copenhagen school. When discussions turned to the question of completeness of quantum mechanics at the Unesco Colloquium on *Science and Synthesis* in 1965, Jean-Paul Vigier soon recognised that much of the confusion stemmed from the fact "we have here representatives of several versions of the Copenhagen interpretation" (Maheu 1971,143). In discussing the idea of "the Copenhagen school" that quantum mechanics requires the introduction of the observer into physics, Vigier observed, quite correctly, that "the various representatives of this school, even those present at this forum, are not basically in agreement" (Maheu 1971, 145). What is critical to realize here is that the claim of 'orthodoxy' in quantum mechanics came to be identified with a range of different physical and philosophical interpretations of quantum mechanics.

In response to the growing sense of confusion, a number of philosophers attempted to clarify the issue by redefining and reconstructing what they saw as the Copenhagen interpretation of quantum mechanics. This often took the form of attempting to give "a minimal account of what many physicists would understand as the 'core meaning' of the Copenhagen interpretation" (Hanson 1959, 327–8). One finds this approach in the writings of philosophers such as Hanson (1958; 1959), Ballentine (1970), Stapp (1972), and MacKinnon (1985, 110). As Stapp put it: the task now was to "give an account of the *logical essence* of the Copenhagen interpretation" which in no way should be confused with "the inhomogeneous range of opinions which constitute the Copenhagen interpretation itself" (Stapp 1972, 1099).

Yet, while the pragmatic definitions of the Copenhagen interpretation presented by philosophers like Hanson and Stapp did serve to clarify many issues, at the same time they inadvertently contributed to the confusion. By ignoring many of the epistemological issues which were the subject of so much discussion in the 1930s, the attempt to give a 'minimal account' of the Copenhagen interpretation failed to deal with what many physicists saw as critical to the *interpretation* of quantum mechanics. More importantly, however, these papers only served to further entrench the view that beneath the mass of conflicting voices, there is such a thing as the 'Copenhagen interpretation' which should be the object of careful historical and philosophical inquiry, thereby perpetuating the myth. On careful inspection we find, in many cases, substantial disagreement among philosophers of science like Stapp, Ballentine and Hanson on how to understand the 'Copenhagen interpretation', which has only served to increased the num-

ber of different ways in which the term has been employed since the 1950s. Lamenting this state of affairs, Asher Peres points out that there appear to be "at least as many different Copenhagen interpretations as people who use that term, perhaps even more" (Peres 2002, 29).

5. Conclusion

The last twenty years have witnessed a concerted effort on the part of scholars to distinguish Bohr's views from those who claimed to speak on his behalf. As Chevalley has argued: "what makes Bohr difficult to read is the fact that his views were identified with the so-called *'Copenhagen Interpretation of Quantum Mechanics',* while such a thing only emerged as a frame for discussion in the 1950s" (Chevalley 1999, 59 emphasis in original). Don Howard adopts much the same attitude, contending that "the Copenhagen interpretation corresponds only in part to Bohr's view" (Howard 2004, 669). Elsewhere he puts it in more forceful terms: "Bohr's complementarity interpretation is not at all what came to be regarded as the Copenhagen interpretation" (Howard 2004, 675). However, I would stress, it is a misunderstanding to think that *any* single view can be regarded as *the* Copenhagen interpretation. Any one of a number of different philosophical viewpoints, some of which bear little resemblance to Bohr's own views, has at one time or another come to stand for the Copenhagen interpretation.

In examining the myth of the Copenhagen interpretation, this paper has attempted to shed new light on 'how' and 'why' the myth was invented. While Beller focuses on the rhetorical strategies employed by physicists, her account does not pay sufficiently close attention to the way in which the shared vocabulary of the Copenhagen school tended to obscure the differences which existed within it. The semantic plasticity or if you like, the polysemy, of concepts such as completeness, wave-particle duality, indeterminacy, and complementarity served to disguise the disagreements between the physicists who used these terms with little regard for the different meanings these terms had. We also see that in the 1950s, confronted with the emergence of the attempts to give a deterministic interpretation of quantum theory, physicists such as Born, Bohr, Heisenberg, Pauli and Rosenfeld kept their more philosophical differences regarding the interpretation of quantum mechanics out of the public spotlight, preferring instead to raise them in private correspondence. More important still, were the historical reconstructions of physicists like Heisenberg in the 1950s, which attempted to trace the emergence of the Copenhagen orthodoxy and with it a particular philosophical viewpoint to the late 1920s. Their rewriting of history completely ignores the different philosophical points of view which took shape in the 1930s and 1940s.

But perhaps the most important factor behind the invention of the Copenhagen interpretation in the 1950s was the philosophical and ideological critique that emerged from the Soviet Union after the Second World War. By identifying Bohr's view (and by implication, the views of his followers such as Heisenberg, Born, Dirac, Pauli), with positivism or what amounted to same thing—subjective idealism, the Soviet-Marxist critique served to polarize views over the interpretation of quantum mechanics into two distinct philosophical positions. The views of soviet Marxists found support in the West, particularly in France in the 1950s. By the 1960s, one finds a similar tendency to polarize the controversy in terms of the debate between positivism and realism. To this extent, the different philosophical interpretations of quantum mechanics, such as logical positivism, neo-Kantianism, and even dialectical materialism, which had emerged in the 1930s and 1940s, had by the 1960s been effectively lumped together under the rubric of a positivist or subjectivist 'Copenhagen interpretation of quantum mechanics'.

It is worth noting that the idea that there exists a single philosophical framework behind the Copenhagen interpretation continues to persist today. In many cases this is done to give legitimacy to a particular philosophical viewpoint, which is identified with the orthodox view. By way of example I cite the work of Kalervo Laurikainen and Roger Fjelland. Laurikainen's study of Pauli's philosophical thought has led him to conclusion that the conscious observer does indeed play a central role in quantum mechanics, a view which he (in my view mistakenly) attributes to Pauli. While acknowledging there were considerable disagreements between Bohr, Heisenberg and Pauli over the interpretation of quantum mechanics, Laurikainen arrives at the conclusion that Pauli, not Bohr, "was the most consistent representative of the philosophical attitude which can be discerned behind the original Copenhagen interpretation" (Laurikainen 1988, 158). Fjelland, on the other hand, attempts to show "that there are interesting parallels between Bohr's philosophy and phenomenology (in particularly the latter Edmund Husserl), and that the Copenhagen interpretation of quantum mechanics is much closer to phenomenology than to positivism" (Fjelland 2002, 57–8). Both these authors refer to the 'Copenhagen interpretation' in an effort to lend weight to their own philosophical agendas.

A deeper understanding of the important disagreements within the 'Copenhagen school' and the strategies which helped to form the impression of consensus, remains important because it can help to rid us of the persistent tendency to look for the 'authentic' Copenhagen interpretation, as a guiding historiographical concept. It is time to dispel the myth of the "Copenhagen interpretation" and let the different philosophical interpre-

tations of quantum mechanics which emerged in the 1930s take their place alongside one another, not as part of a single homogenous orthodox view, but as competing attempts to make sense of one of the enigmas of twentieth century thought—the interpretation of quantum mechanics.

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