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Title:
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Date:
2017

Citation:
Camilleri, K. (2017). Why do we find Bohr obscure? Reading Bohr as a philosopher of experiment. Faye, J (Ed.). Folse, H (Ed.). Niels Bohr and the Philosophy of Physics: Twenty-First Century Perspectives, (1), pp.19-46. Bloomsbury Academic.

Persistent Link:
https://hdl.handle.net/11343/312595

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Why Do We Find Bohr Obscure?

Reading Bohr as a Philosopher of Experiment

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It might be difficult to leave Bohr behind, but we continue to discover new ways of reading Bohr.

(Plotnitsky 2006, 142)

1. Introduction

In a letter to Imre Lakatos in 1968, Paul Feyerabend declared "the idea of Bohr's obscurity" to be "nothing but a myth," perpetuated by critics like Popper, who had simply never bothered to read him properly (Feyerabend to Lakatos, January 28, 1968, in Feyerabend and Lakatos 1986, 126–7). Feyerabend had a point. Many of Bohr's critics in those years tended to rely on second-hand sources, and seldom made a serious study of Bohr's philosophical writings for themselves. Yet Feyerabend's efforts to render Bohr's notion of complementarity more intelligible, through a detailed analysis, did little to change the perception in the years that followed (Feyerabend 1968 and 1969). When a later generation of scholars did devote themselves to a close reading Bohr's texts, any illusions that Bohr's meaning would become transparent if only one read him carefully enough were quickly dispelled. In 1985 Abner Shimony expressed the frustration of many, confessing "that after 25 years of attentive—and even reverent—reading of Bohr, I have not found a consistent and comprehensive framework for the interpretation of quantum mechanics" (109). In the three decades since then, Bohr's philosophy of physics has been the subject of numerous books and articles. Thanks to this work, much of which began to emerge in the 1980s and 1990s, we now have a far more nuanced view of his philosophy than we did in the 1960s, when the positivist image of Bohr prevailed. Yet, scholarly opinion on how we should understand his viewpoint remains deeply divided. Bohr, it seems, remains as obscure as ever.

In this chapter I want to revisit the question, "why do we find Bohr obscure?" One obvious response is of course that Bohr *was* obscure. While there is undoubtedly some

truth in this, I would suggest that familiar complaints about Bohr's impenetrable writing style miss the point.[1] Undeniably part of the problem, as Makoto Katsumori (2011, 61) rightly points out, is that Bohr's central notion of complementarity is, by its very nature, not easy to express in the traditional categories philosophers often work with. Catherine Chevalley (1994 and 1999), whose work represents the most systematic treatment of this question to date, suggests that the distortion of Bohr's original line of thought and the failure to locate his thinking in the appropriate cultural and intellectual context are largely to blame. Although I find much to admire in her analysis, my historiographical approach differs from hers in a number of crucial respects. Whereas Chevalley argues that Bohr's words only make sense when read against the background of the post-Kantian philosophical tradition, I take my inspiration from R. G. Collingwood's insight that "we can understand a text only when we have understood the question to which it is an answer."

In addressing the question of Bohr's obscurity, I begin by providing a brief historical overview of the various philosophical interpretations, and historiographical approaches, which have attempted to make sense of Bohr's views. Beginning in the 1930s, many of Bohr's contemporaries attempted to read complementarity through the lens of a particular philosophical tradition, such as logical empiricism, neo-Kantianism, or dialectical materialism, in spite of the fact that Bohr never aligned himself with any school of thought. More sophisticated historiographical approaches began to emerge by the 1970s, as scholars attempted to unravel the web of meanings associated with Bohr's writings. One of the important directions in Bohr scholarship over the past few decades has been the attempt to make sense of Bohr by exploring the relationship between his epistemological viewpoint and one or another philosophical tradition, position, or school of thought. A somewhat different, though not necessarily unrelated, approach seeks to make sense of Bohr's thought by situating him in the right intellectual tradition, through an examination of the historical or philological context in which his views took shape. Much of this work is focused on providing a "philosophical reconstruction" of Bohr's view by drawing on critical methods of textual analysis. While many new insights emerged from these efforts, Bohr has remained something of a philosophical enigma.

Here I add a new layer to this discussion, by turning my attention to one of Bohr's key doctrines—the indispensability of classical concepts. I have deliberately chosen to focus specifically on this aspect of Bohr's thought, and not others, because this has undoubtedly been one of the most intriguing and puzzling aspects of Bohr's philosophy, and as Don Howard (1994, 202) has rightly pointed out, it is "more fundamental to Bohr's philosophy of physics than are better-known doctrines, like complementarity." As Bohr ([1929] 1987a, 16) explained:

> It would be a misconception to believe that the difficulties of atomic theory may be evaded by eventually replacing the concepts of classical physics by new conceptual forms. Indeed, as already recognized, the recognition of the limitation of our forms of perception by no means implies that we can dispense with our customary ideas or their direct verbal expressions when reducing our sense impressions to order. Nor is it likely that the fundamental concepts of the

> classical theories will ever become superfluous for the description of physical experience.

Here I argue that Bohr's meaning has remained elusive because his central preoccupations lay not so much with an interpretation of the quantum-mechanical formalism, which many of his contemporaries saw as *the* problem of quantum theory, but rather with the question of what kind of knowledge of objects can be obtained by means of experiment. At the heart of this view is a functional, as opposed to a structural, conception of the *experimental apparatus*. To this end, I suggest that we should read Bohr first and foremost as a philosopher of experiment.

In the final section, I explore Bohr's doctrine of classical concepts from another angle, this time in the description of quantum *objects*. Bohr's emphasis on the primacy of classical language has often been misconstrued as a doctrine about the *theoretical* language we must use in representing quantum objects, rather than about how concepts function in experimental *practice*. Appreciating this point requires a shift of perspective away from the traditional preoccupation with meaning, characteristic of both positivist and post-positivist philosophy of language, to an understanding of how concepts are *used* in particular contexts of inquiry. Here, I take my cues from some recent work, which has typically been associated with the "turn to practice" in history and philosophy of science. While I am not suggesting that Bohr should be read as anticipating these later developments, I do think that by paying careful attention to the way concepts function in experimental practice, we can open up a different perspective on Bohr's epistemology that serves to correct the persistent failure to read him on his own terms.

2. A historiographical overview: The challenges of reading Bohr

Bohr's obscurity: Early philosophical interpretations

While Bohr's view of complementarity is still commonly seen as having formed the central plank in a unified and widely shared orthodox view that emerged in the late 1920s, commonly known as the "Copenhagen interpretation," extensive historical scholarship over the past thirty years has challenged, if not seriously undermined, the view that Bohr's views were well understood and widely accepted, even among his closest collaborators.[2] Bohr's notion of complementarity was hailed by many of his followers as "the most significant result for philosophy that crystallized out of modern physics," but it was given a variety of different philosophical interpretations by his various spokesmen, and was appropriated by various philosophical camps during the 1930s who sought to enlist him as an ally in pursuit of the their own agendas (Jordan 1944, 131). By the 1960s it was no longer possible to easily distinguish Bohr's own views from those who professed to speak on his behalf (Chevalley 1999; Howard 1994).[3]

The rise of logical positivism in the 1930s undoubtedly played a significant role in shaping the early reception of Bohr's views. In the preface to his textbook on quantum theory in 1936 and an earlier article, Pascual Jordan (1936, vii; 1934) argued that the

development of quantum mechanics and Bohr's complementarity interpretation signified a resounding victory for positivism over realism. As Jan Faye and Ulrich Röseberg have argued, during the 1930s, leading figures of the Vienna Circle, Otto Neurath, Philip Frank, and the Danish philosopher Jørgen Jørgensen, engaged in an ongoing dialogue with Bohr, and formed the view that while he was occasionally prone to lapse into metaphysical language, his philosophical outlook was basically favorable to positivism. In a letter to Carnap in 1934, Neurath reported that Bohr's writings were "full of crass metaphysics," but he was convinced from discussions with Bohr in Copenhagen that his "basic attitude" was in accordance with logical positivism (Neurath to Carnap, November 14, 1934, cited in Röseberg 1995, 112). Writing to Bohr in 1935–36, Frank complained that Bohr's papers were not always expressed as clearly as they might have been, and to this extent, Frank saw it as his task to provide a clarification of his views—to "explain what Bohr really meant" (Röseberg 1995, 109–10; Jacobsen 2012, 128–9; Faye 2007, 37–8). Complementarity, Frank (1975, 179) would confidently declare in 1938, was "fully compatible with the formulations of logical empiricism."

Yet the Vienna Circle was not the only school of the interwar period to lay claim to Bohr as a philosophical ally. In Germany, where Kant's critical idealism continued to exert a significant influence, Bohr was often read through the lens of one or another of the various schools of neo-Kantianism (Weizsäcker 1936, 1941a,b, 1952; Hermann 1935, 1937a,b). In the 1930s, Heisenberg (1934, 700) called on philosophers to reexamine "the question raised by Kant, and much discussed ever since, concerning the *a priori* forms of intuition and categories" in the light of quantum mechanics, 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." Whereas Frank saw Bohr's viewpoint as signifying the triumph of positivism, philosophical discussions in Leipzig during the 1930s between Heisenberg, Weizsäcker, and the visiting Kantian scholar Grete Hermann tended to revolve around Bohr's relationship to Kant. As Weizsäcker ([1966] 1994, 185) would put it some years later: "The alliance between Kantians and physicists was premature in Kant's time, and still is; in Bohr, we begin to perceive its possibility."

Further divisions between Bohr's followers would become apparent during the 1930s. Léon Rosenfeld, who served as Bohr's assistant in Copenhagen and became increasingly interested in Marxism during this time, was deeply critical of physicists who had succumbed to the "scourge" of neo-positivism or Kantianism, and in doing so, mistakenly labeled Bohr a positivist or an idealist. After the Second World War, Rosenfeld ([1953] 1979, 465; [1957] 1979) launched a vigorous defense of complementarity as a striking example of the dialectical method in epistemology. As Anja Jacobsen (2007) has made clear, Rosenfeld's understanding of complementarity was to a large extent shaped by his commitment to Marxism, which drew sharp criticism from Max Born and Wolfgang Pauli during the 1950s.[4] While Rosenfeld later insisted that he had "never played the game of putting a materialist label" on Bohr, he did argue that complementarity constituted "the first example of a precise dialectical scheme" (Rosenfeld to Bohm, December 6, 1966, LRP "Epistemology"; Rosenfeld [1953] 1979, 481). Indeed in private correspondence, Rosenfeld went so far as to claim that Bohr was a Marxist who simply wasn't aware of it (Jacobsen 2007, 17).

The ideological context was in some cases crucial to the reception and interpretation of Bohr's views. This was particularly evident in the Soviet Union, where Bohr was often criticized as endorsing some form of subjectivism or positivism. Such criticisms intensified during the early years of the Cold War, resulting in a decision at the 1947 meeting of the Soviet Academy of Sciences to effectively ban complementarity. By the late 1950s, however, a more relaxed attitude began to emerge, resulting in a reconciliation of Bohr's views with Soviet dialectical materialism (Graham 1988, 311–13). After discussions with Bohr in Copenhagen in 1957, the Soviet physicist Vladimir Fock (1957, 646) declared that Bohr's view of quantum mechanics was essentially correct, but that his use of language had unfortunately given "rise to many misunderstandings and to an incorrect [positivist] interpretation." Here Fock (1958, 210) argued "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." This appears to have been the shared experience of many of Bohr's interpreters, regardless of their philosophical orientation.

By the 1960s, new interpretations began to appear. Klauss Michael Myer-Abich's *Korrespondenz, Individualität und Komplementarität* published 1965 was one of the earliest book-length studies of Bohr's philosophy, and undoubtedly set the tone for much of the later work (Feyerabend 1968 and 1969; Scheibe 1973). Perhaps the most influential contribution in English appeared in two important works published in the 1960s by Aage Petersen (1963; 1968), who served as Bohr's assistant in Copenhagen from 1952 until his death in 1962. Petersen's philosophical reading of Bohr, much like Rosenfeld's, was informed less by careful textual analysis, and more by private recollections of his philosophical discussions with Bohr. Here we encounter a rather different image of Bohr—as a thinker preoccupied with the limits and the constitutive dimension of human language. Indeed it is from Petersen (1963, 11) that we have inherited the adage, frequently attributed to Bohr, that "we are suspended in language." Petersen's shift of focus to the primacy of language found a receptive audience among physicists and philosophers, who had otherwise struggled to make sense of Bohr's own writings.

Given the diversity of readings of Bohr, clashes were inevitable.[5] Yet Bohr never publicly endorsed any particular reading of his work, nor did he ever acknowledge an intellectual debt to particular philosophical tradition or align himself with a school. This was undoubtedly intentional. As Anja Jacobsen (2012, 124) has pointed out, "Bohr had a rather skeptical attitude to professional philosophers" and he frequently lamented the tendency to interpret quantum mechanics through the lens of one or another philosophical "-ism." As Rosenfeld explained in a letter to David Bohm, those who were well acquainted with Bohr did "not take very serious this game of putting labels with various 'isms' upon Bohr. No more seriously, in fact than he himself took it" (Rosenfeld to Bohm, December 6, 1966, LRP "Epistemology"). This attitude is evident in Bohr's (2005, 200) remarks in an interview for *Izvestia* in 1934:

> When one raises the question of which philosophical consequences arise from modern physics, one may not thereby understand the question to mean which old philosophical schools comply with modern physics … Although some

> consequences of modern physics have something in common with the view of many great philosophers, yet it seems to me that if men such as Spinoza or Marx were alive today, they would probably, together with the rest of us, enjoy learning new things from modern physics for the relevance of general philosophy.

Bohr often became disillusioned with philosophers, who appeared more interested in how complementarity related to the philosophical systems of the past than in what was genuinely new about it.[6] In a letter to Martin Strauss in 1935, Rosenfeld reported that Bohr had expressed his disappointment about Heisenberg's willingness to engage in discussions with philosophers about the relationship of quantum physics to Kantian philosophy. Bohr took the opportunity to remind his colleagues in Copenhagen that it was "psychologically (and of course in the first instance substantively!) important that *one does not enter into any compromises with philosophers*" (Rosenfeld to Strauss, November 16, 1935, in LRP "Correspondence particulière"; emphasis mine). Given his suspicion of the corrupting influence of professional philosophers, it is therefore unsurprising that in the famous "Last interview" with Kuhn, Petersen, and Rüdinger, Bohr boldly declared that "I think it would be reasonable to say that no man who is called a philosopher really understands what one means by the complementary description" (November 17, 1962, AHQP).

There is a certain irony here. Although Bohr never aligned himself with any philosophical school, he seldom made public his frustration with physicists who sought to interpret complementarity through the lens of a particular philosophical tradition. In many cases, important differences of opinion between Bohr and the leading physicists with whom he worked were confined to private correspondence (Bohr to Pauli, March 2, 1956, in Pauli 1996, 137). Bohr was often inclined to dismiss such disagreements as reflecting nothing more than matters of emphasis or choices of terminology, though this was not always the view of his interlocutors. While such disagreements rarely made it to print, his correspondence with physicists such as Heisenberg, Born, Pauli, Frank, Fock, and Weizsäcker, spanning more than two decades, reveals a series of hidden tensions and misunderstandings that were often never fully resolved (Beller 1999; Camilleri 2009a).

How should we read Bohr? Historiographical approaches

Given the disagreement among even Bohr's closest collaborators, it has been one of the important tasks of Bohr scholarship over the past thirty years to disentangle his views from those who professed to speak on his behalf. As Don Howard (1994, 204). has put it, the "history of misreadings of Bohr has so obscured his intentions, that one must first deconstruct the misreadings, so that one can reconstruct Bohr's words and their meanings." Yet here it is important to realize that the divergent, and in some cases misleading, interpretations that we have inherited from an earlier generation of commentators were as much a *response* to the difficulties they encountered in reading Bohr himself, as they were a *contributing factor* to those difficulties. The proliferation of interpretations can be seen as both a symptom and a cause of the hermeneutic challenges of reading Bohr.

Here I turn my attention from history to historiography. Over the past few decades, a number of distinctive historiographical approaches have emerged, which draw on different methods of analysis. While these approaches differ, they are not necessarily mutually exclusive, and have often been effectively combined in an effort to gain a deeper insight into Bohr's writings. One such approach signifies a continuation of the attempts to make sense of Bohr by reading him through the lens of a particular philosophical school or tradition, such as Kantianism or positivism, or to explore the relationship between his thought and modern philosophical positions such as realism and anti-realism. Michael Cuffaro (2010, 309), for example, has recently argued that "Bohr's complementarity interpretation of quantum mechanics" can only be properly understood once it is recognized that it "follows naturally from a broadly Kantian epistemological framework." While acknowledging that Bohr's views differ in certain crucial respects from those of Kant, he maintains that any proper "interpretation of Bohr should *start* with Kant" (310; emphasis in the original). Yet, even among those scholars who see certain parallels between Bohr and Kant, there is no agreement on how to understand this relationship.[7] Bohr's views do not appear to fit neatly within traditional philosophical categories, and there remains considerable debate and disagreement about how best to locate his philosophical position with respect to the contemporary realism/anti-realism debates.[8]

In an effort to locate Bohr's views with respect to familiar philosophical reference points, some scholars have sought to explore connections with twentieth-century analytic and continental philosophical traditions. Edward Mackinnon has suggested that Bohr's conception of language shows some similarities with certain views we find in Wittgenstein's *Philosophical Investigations*, while Roger Fjelland suggests that "there are interesting parallels between Bohr's philosophy and [Husserlian] phenomenology" (MacKinnon 1985, 115; Fjelland 2002, 58). More recently, Makoto Katsumori (2011, 89–151) has examined the connections between Bohr's idea of complementarity and post-Heideggerian hermeneutic philosophy and the deconstruction of Jacques Derrida. Katsumori's work is indicative of a recent trend, exemplified in the work of Arkady Plotnitsky (1994), John Honner (1994), and Karen Barad (2007), to explore certain parallels between Bohr's thought and some aspects of postmodernism, post-structuralism, or deconstruction.[9] Yet, as Folse (1994, 119) noted more than twenty years ago, these efforts leave one with the strong feeling that "the philosopher's categories cannot characterize complementarity without distortion."

A somewhat different historiographical approach can be seen in the work of scholars such as Jan Faye and Catherine Chevalley, both of whom have sought to gain a deeper insight into Bohr by locating him within his appropriate intellectual context. Faye's (1991) work focuses largely on Bohr's intellectual background in Denmark, in tracing the philosophical impulses that shaped the development of Bohr's thinking about atomic physics. Drawing on methods of intellectual biography, Faye (1991) shows that Bohr's early forays into philosophy were shaped by his close relationship with the Danish philosopher Harald Høffding, as well as his participation in the philosophical seminars and discussions of the Ekliptika circle in Copenhagen. More contentious perhaps is Faye's claim that Høffding exerted an important influence on Bohr's notion of complementarity, as is evident in the various strands of Kantian and

pragmatist thought that can be traced in Bohr's philosophical writings of the 1920s and 1930s. Notwithstanding the criticisms of David Favrholdt, there is considerable evidence to suggest that Høffding was an important formative influence on Bohr's philosophical development. However, precisely what that influence was and to what extent Bohr's view of complementarity owes something to this influence remain difficult questions to answer.[10]

Like Faye, Catherine Chevalley (1991; 1994; 1999) adopts a historical contextualist approach, but her work focuses on tracing the historical sources of "Bohr's epistemological vocabulary." Chevalley (1994, 51) contends that Bohr's language has proved difficult to decipher "because we have become blind to the specific tradition in which it makes sense." This tradition, as she explains, emerged in the German-speaking world in the second half of the nineteenth century, in the work of such figures as Wilhelm von Humboldt, Herman von Helmholtz, and Heinrich Herz. Bohr's attempt to come to grips with the paradoxes of quantum theory in the 1920s, which culminated in the idea of complementarity, was "formulated within the philosophical language that developed in the German culture starting with Kant." To this extent, Chevalley (1999, 65–6) suggests that we need to read Bohr "in a philological way" because "Bohr's interpretation makes sense only if one takes into account both the detailed genesis of quantum theory and the very precise lexicon of post-Kantian epistemology."[11]

Chevalley's contextualist approach throws up many valuable insights. Yet language can be deceptive. As Folse (1985, 219) points out, while it is true that Bohr's "description of phenomenal objects has a certain Kant-like appearance," such an appearance is misleading, given that complementarity has nothing to do with "how phenomena arise in the subject's consciousness." Here we can see a certain parallel with the difficulties that emerged in the nineteenth-century reception of Kant. As Ernst Cassirer noted in 1929, much of the confusion surrounding Kant's critical turn in philosophy was a result of the fact that it was expounded in the language of eighteenth-century faculty psychology. Thus, while Kant sought to give new meaning to terms like "receptivity," "spontaneity," "sensation," and "understanding," the use of these terms tended to evoke the very metaphysical and psychologistic interpretations that Kant had explicitly set out to overcome (Cassirer [1929] 1957, 194–5). My suspicion is that we encounter something similar in Bohr. Recognition of this point raises the possibility that tracing the linguistic similarities between Bohr's vocabulary and the "lexicon of post-Kantian epistemology" might prove to be more of a hindrance than a help in making sense of his views.

Complementing the forms of contextualist analysis of scholars like Faye and Chevalley are the methods of textual analysis and reconstruction exemplified in the work of philosophers such as Clifford Hooker (1972 and 1994), Erhard Scheibe (1973), Henry Folse (1985), Dugald Murdoch (1987), and Don Howard (1994). While each of these authors differ on specific points of interpretation, they each attempt to reconstruct Bohr's meaning through a rigorous critical analysis of his texts and modes of argumentation, often paying careful attention to subtle shifts of terminology and the historicity of Bohr's thought, as it unfolded during the 1920s and 1930s (Röseberg 1994). Much of this scholarship builds on the earlier work of Feyerabend (1968, 309; and 1969, 103), who in the late 1960s urged philosophers to disregard the muddled and

distorted versions of Bohr's views handed down to us by both his supporters and his critics, and to instead go "back to Bohr." In a similar vein, Howard advocates the "need to return to Bohr's own words, filtered through no preconceived dogmas," under the provisional assumption that "his words make sense." Here Howard (1994, 201) emphasizes the need to "apply the critical tools of the historian" and the "synthetic tools of the philosopher" to the difficult task of reconstructing from Bohr's writings a coherent philosophy of physics.

Yet, as Howard (1994) makes clear, we should not fall prey to the illusion that we can simply let Bohr's words "speak for themselves." One can no longer simply assume that we can "interpret Bohr's words as if they stand there unadorned, waiting for an informed and sympathetic eye to read their author's intentions" (204). At certain places in the text, it is necessary to go beyond Bohr's words, "in order to clarify the direction in which they were tending" (225). A philosophical reading of Bohr must therefore remain faithful to Bohr's words, but it can and should go beyond those words, "to ask what Bohr would have said, in certain contexts, consistent with what he says elsewhere" in order to "bring out better their intended meaning." At such points, "interpretation passes over into reconstruction" (204). I agree with Howard that any philosophical reading of Bohr will inevitably involve some degree of reconstruction. Yet here there is a danger of straying too far from the text. The plausibility of any reconstruction will not only depend on whether it makes good sense of Bohr, but also on what kind of textual (or other forms of) evidence can be marshaled in support of it. Where such evidence is lacking, the reconstruction may look suspiciously like a distortion of "what Bohr really meant."

The problems briefly touched on here are, to a large extent, reflective of more general methodological difficulties that will be familiar to intellectual historians of different stripes. Yet, Bohr's writings present a particularly acute example of the challenges of intellectual history. Reading Bohr therefore demands we remain attentive to these general historiographical issues, as well as the specific aspects of his philosophical viewpoint that have persistently remained opaque. As Henry Folse (1994, 119) has astutely observed, we typically find Bohr at his "most elusive" when we endeavor to discern in his writings answers to "questions which our philosophical perspective seems to make inevitable." Here I think Folse puts his finger on the crux of the problem. It is our "philosophical perspective," not so much in the doctrines we bring to the reading of Bohr's texts, but in the questions we pose, that has, more often than not, led us astray.

My contention is that Bohr's philosophical viewpoint has remained elusive in large part because we have persistently asked the wrong questions in interrogating his texts. There is still much to be said for R. G. Collingwood's (1939, 36–7) insight that "we can understand a text only when we have understood the question to which it is an answer." Much of the literature has simply uncritically assumed that Bohr's intention was to provide an "interpretation of quantum mechanics." Yet, Bohr never set out to defend a particular interpretation of quantum mechanics, at least in the sense in which that term is usually understood. Much to the frustration of many contemporary philosophers, Bohr resolutely refused to be drawn into what he saw as unhelpful philosophical debates about realism and idealism (Honner 1994). Questions about the physical meaning of the wave function, or the ontological status of the interphenomenal object,

or the measurement problem were at best peripheral concerns, which only served to obscure the crucial epistemological problem that Bohr sought to make the central focus of his writings.[12] It is therefore no surprise that Bohr devoted relatively little attention to these questions, leaving it to others to reconstruct a coherent position from his cursory remarks on these subjects.

3. Niels Bohr as philosopher of experiment

The doctrine of classical concepts and the epistemology of experiment

By the 1930s Bohr's writings reflect his primary interest in the conditions of possibility of *experimental knowledge* of quantum objects. Putting experiment at the center of Bohr's epistemological concerns may seem trivial, but it provides a critical point of departure for overcoming many of the persistent difficulties that have plagued Bohr scholarship. Our preoccupations with the paradoxes of quantum *theory* have, to some extent at least, blinded us to what by the mid-1930s had assumed central importance for Bohr. While I can only sketch my account in the pages that follow, it is noteworthy that Bohr's foray into quantum electrodynamics in the early 1930s, culminating in his classic paper with Rosenfeld in 1933, was not only an attempt to make sense of the newly emerging quantum field theory, but was intended to provide a deeper insight into the measurability of field and charge quantities (Bohr and Rosenfeld [1933] 1979). The importance of this work for the development of complementarity has, to my knowledge, received surprisingly little attention, with the notable exception of Arkady Plotnitsky (2006, 119–42; 2012, 89–106), though it marks an important step in the unfolding of Bohr's epistemological views on quantum theory in the 1930s.[13]

Given the restrictions of space, I will not deal here with the development of Bohr's early views on complementarity prior to 1935. The period from 1926 until the EPR paper, as many scholars have noted, saw a gradual refinement of Bohr's thought. Whereas Bohr's original formulation of complementarity at the Como conference in 1927 had focused primarily on stationary states, by the time he presented his paper at the Unity of Science Congress in 1936, we find a somewhat different account in which the mutually exclusive experimental arrangements now assumed central importance (Camilleri 2009b, 111–19). This mature position was developed further in lectures in the late 1930s, which reveal important terminological shifts from his earlier writings.[14] Yet, these shifts in the formulation of complementarity did not shake his conviction in the indispensability of classical concepts. In striking contrast to the views expressed by Ernst Cassirer, Bohr remained convinced that classical concepts would continue to play an ineliminable role in modern physics.[15] In perhaps his most widely quoted passage on the subject, Bohr (1949, 209) wrote:

> It is decisive to recognize that, *however far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms*. The argument is simply that by the word "experiment" we refer to a situation where we can tell others what we have done and what we have learned and

> that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics. (Emphasis in the original)

What should we make of this passage? In order to reconstruct the steps that led Bohr to this view, we must first understand that Bohr had long recognized that the entanglement of the object and the instrument posed difficulties for the ordinary understanding of experimental observation. A quantum-mechanical treatment of the observational interaction would paradoxically make the very distinction between object and instrument ambiguous. The impossibility of "separating the behaviour of the objects from their interaction with the measuring instruments" in quantum mechanics "implies an ambiguity in assigning conventional attributes to atomic objects" (Bohr 1948, 317). At the same time, such a distinction is a necessary condition for empirical inquiry. After all, an experiment is carried out precisely to reveal information about the "autonomous behavior of a physical object" (Bohr 1937, 290). To speak of an interaction between two separate systems—an object and measuring instrument—is to speak in terms of classical physics.

Bohr regarded this condition of isolation to be a simple logical demand, because, without such a presupposition, an electron cannot be an object of experimental knowledge at all. The crucial point, as Bohr explained at the 1936 Unity of Science Congress, is that in contrast with the situation in classical physics, in quantum mechanics "it is no longer possible sharply to distinguish between the autonomous behaviour of a physical object and its inevitable interaction with other bodies serving as measuring instruments." Yet it lies in "the very nature of the concept of observation itself" that we can draw such a distinction (Bohr 1937, 290). Bohr ([1938] 1987c, 25–6) elaborated on this point at some length from his 1937 lecture "Natural Philosophy and Human Cultures":

> We are faced here with an epistemological problem quite new in natural philosophy, where all description of experiences so far has been based on the assumption, already inherent in the ordinary conventions of language, that it is possible to distinguish sharply between the behaviour of objects and the means of observation. This assumption is not only fully justified by everyday experience, but even constitutes the whole basis of classical physics . . . [In light of this situation] *we are, therefore, forced to examine more closely the question of what kind of knowledge can be obtained concerning objects*. In this respect, we must . . . realize that *the aim of every physical experiment*—to gain knowledge under reproducible and communicable conditions—leaves us no choice but to use everyday concepts, perhaps refined by the terminology of classical physics, not only in accounts of the construction and manipulation of measuring instruments but also in the description of actual experimental results. (Emphasis mine)

The real question for Bohr was not what kind of reality is described by quantum mechanics, or how we should interpret the quantum formalism, but rather "what kind of knowledge can be obtained concerning objects" by means of experiment. Thus, for Bohr, the epistemological problem was how experimental knowledge of quantum

objects is possible. To this extent, Bohr's philosophical preoccupations were fundamentally at odds with, or at least rather different from, what many physicists and philosophers of physics see as the problem of quantum mechanics, namely, the interpretation of its formalism.[16] To put it simply, Bohr's doctrine of classical concepts is not primarily an interpretation of quantum mechanics (although it certainly bears on it), but rather is an attempt to elaborate an epistemology of experiment.

In order to bring out this point clearly, it is instructive to borrow the terminology employed by Peter Kroes (2003, 76) in his distinction between a structural and a functional description of an experimental apparatus: "The structural description represents the object [serving as the measuring instrument] as a physical system, whereas the functional description represents the object as a technological artefact." While it is always possible to conceptualize a measuring instrument, such as a mercury thermometer, "from a purely physical (structural) point of view" as an object with certain dynamical properties and obeying physical laws, the same object can also be described "from an intentional (functional) point of view" as an instrument designed "to measure a particular physical quantity" such as temperature. From the epistemological point of view, the functional description is more fundamental (75). As Kroes explains:

> Every experiment has a goal (to measure x or to detect y, or to show phenomenon z, etc.) and it is in relation to this goal that every part of the experimental setup is attributed a function, as well as actions performed during the experiment. *For describing and understanding an experiment, reference to functions is unavoidable*. In contrast the description of the results of the outcome of an experiment (the observations, data, measurements) is free of any reference to functions at all . . . Thus whereas experiments are described in a functional way, the description of the results of experiments and of physical reality as constructed on the basis of those results is of a structural kind. *This means that the structural description of physical reality rests implicitly on a functional description of at least part of the world*. (74; emphasis added)

This passage provides a valuable insight through which we can make good sense of why Bohr saw it as necessary to draw an epistemological distinction between the functioning of the instrument and the object under investigation. Indeed, the language Kroes employs here bears a striking similarity with the way Bohr often expressed himself. In his discussion of the interaction of object and instrument, Bohr always refers to the aims and functions of the experiment. Here it is worth noting that for Kroes, a "functional description also makes use of structural concepts; it makes reference to the structural properties of the object [serving as the measuring instrument], not only in describing but also in explaining the design features of the object" (76).

Bohr's central insight was that if a measuring instrument is to serve its *purpose of furnishing us with knowledge of an object*—that is to say, if it is to be described functionally—it must be described classically. Of course, it is always possible to represent the experimental apparatus from a purely structural point of view as a quantum-mechanical system without any reference to its function. However, any functional description of the experimental apparatus, in which it is treated as a means to an end,

and not merely as a dynamical system, must make use of the concepts of classical physics. This is true even when we measure quantum properties, such as spin. In the Stern–Gerlach experiment, for example, the experimental apparatus and the magnetic field must be treated classically for the purpose of performing the experiment. Put simply, any functional description of the experimental apparatus must be couched in the language of classical physics. In Bohr's (1958b, 310) view, "All unambiguous information concerning atomic objects is derived from permanent marks—such as a spot on a photographic plate, caused by the impact of an electron—left on the bodies which define the experimental conditions." In his lecture "On Atoms and Human Knowledge," Bohr (1958a, 169–70) expanded on this point:

> In the analysis of single atomic particles, this is made possible by irreversible amplification effects—such as a spot on a photographic plate left by the impact of an electron, or an electric discharge created in a counter device—and the observations concern only where and when the particle is registered on the plate or its energy on arrival with the counter. Of course, *this information presupposes knowledge of the position of the photographic plate relative to other parts of the experimental arrangement*, such as regulating diaphragms and shutters defining space–time coordination or electrified and magnetized bodies which determine the external force fields acting on the particle and permit energy measurements. The experimental conditions can be varied in many ways, but the point is that in each case *we must be able to communicate to others what we have done and what we have learned, and that therefore the functioning of the measuring instruments must be described within the framework of classical physical ideas*. (emphasis added)

The properties of the quantum object, such as its position, charge, spin, and energy, can only be known by virtue of traces it leaves on an experimental system. It must be possible to interpret the results of the experiment in such a way that the visible and permanent traces on the apparatus can be explained as having been caused by an interaction with the object under investigation. If this were not the case—if no such causal inferences were possible—then such systems could not provide us with knowledge of the quantum object. To this end, we must implicitly presuppose a causal chain of events triggered by the object itself, through the apparatus, finally registering at the macroscopic scale, if the measuring apparatus is to serve its purpose. Or, to put it another way, it must be possible to trace this "sequence of cause and effect" from the observation of a spot on a photographic plate or an electric discharge in a counter device, back to the interaction with the object itself. In this sense, "the concept of causality underlies the very interpretation of each result of experiment" insofar as it forms the basis of any functional description of the apparatus (Bohr 1937, 293).

This point has generally not been well appreciated. Manuel Bächtold (2008, 627–8), for instance, argues on the basis of quantum mechanics that a classical physical description simply cannot be applied to the measurement apparatus "during the measurement process." Yet here the *functional-epistemological* account of the apparatus drops out of the picture entirely. If one could not say unequivocally that a visible macroscopic effect, such as a spot on a photographic plate, was caused by a particle striking

the plate, then such an observation would in effect count for nothing. As Bohr put it in a letter to Schrödinger in 1935, "The description of any measuring arrangements must, in an essential manner, involve the arrangement of the instruments in space and their functioning in time, *if we shall be able to state anything at all about the phenomena*" (Bohr to Schrödinger, October 26, 1935, in Bohr 1996, 511; emphasis in the original).

Functional versus dynamical accounts of the experimental apparatus

Bohr's functional epistemology of the experimental apparatus, as outlined above, has often been misunderstood because of a failure to carefully distinguish it from the dynamical problem of the quantum-classical transition. As many commentators have pointed out, while Bohr insisted that we must employ the concepts of classical physics to describe whatever part of the system we have designated to function as a measuring instrument, it is always possible to give a quantum-mechanical description of the apparatus in its entirety. Bohr acknowledged that measuring instruments, like all systems, macroscopic or microscopic, are strictly speaking subject to the laws of quantum mechanics (though I would stress he did not assume that the apparatus could be described as an isolated dynamical system). As he explained at the Warsaw conference in 1938:

> In the system to which the quantum mechanical formalism is to be applied, it is of course possible to include any intermediate auxiliary agency employed in the measuring process. Since, however, all those properties of such agencies which, according to the *aim of measurements* have to be compared with the corresponding properties of the object, must be described on classical lines, their *quantum mechanical treatment will for this purpose be essentially equivalent with a classical description*. (Bohr 1939, 23–4; emphasis added)

Here again, we should note the use of normative language in referring to the aims and purposes of the experiment. While it is of course always possible to describe the apparatus as a quantum-mechanical system, Bohr insists that in doing so we would forfeit a functional account of the apparatus as a means of acquiring empirical knowledge. Measuring instruments, for Bohr, must admit of a classical description, otherwise they could not perform their epistemic function as measuring instruments. To this extent, any "quantum mechanical treatment" of a measuring instrument will, by virtue of its function as a measuring instrument, "be essentially equivalent with a classical description." However, this raises the further question of why, from a purely dynamical point of view, the quantum-mechanical treatment is, or can be treated as, essentially equivalent to a classical description. As Kroes (2003, 76) points out, from a purely structural (as opposed to a functional) point of view, the design and geometric configuration of the apparatus "is just some physical property" and "a completely contingent feature of the object."

Bohr's epistemological explanation for why we must use a classical description thus raises the question of what dynamical features of a macroscopic system entitle us to neglect the "quantum effects." Bohr (1935, 701) here appears to simply assume that

there exists a macroscopic "region where the quantum-mechanical description of the process concerned is effectively equivalent with the classical description." Thus we are led to ask: How is it that classical physics can be employed, at least to a very good approximation, under certain dynamical conditions (typically those corresponding to measuring scenarios)? This is a salient question, given that, strictly speaking, the world, as Bohr recognized, is nonclassical.

Here I want to suggest that Bohr never provided a sustained or satisfactory dynamical explanation for the quantum-to-classical transition, leaving only fleeting remarks on the subject throughout his writings. He seems to have regarded the explanation as trivial, and on most occasions has been content to refer to the "massive" nature of macroscopic bodies serving as measuring instruments. A functional account of the experiment "is secured by the use, as measuring instruments, of rigid bodies sufficiently heavy to allow a completely classical account of their relative positions and velocities" (Bohr 1958b, 310). One finds a similar view expressed on several occasions in Bohr's later writings. As he put it in 1958, "all measurements thus concern bodies sufficiently heavy to permit the quantum [effects] to be neglected in their description" (Bohr 1958a, 170). One might well ask, how heavy is "sufficiently heavy"?

The question of why Bohr thought it was possible that a classical description can be "essentially equivalent to a quantum mechanical one" has been a source of much confusion. Yet these difficulties can be avoided, if we take care to distinguish between Bohr's functional-epistemological account of the experimental apparatus (on which he placed great emphasis) and his account of the quantum–classical transition (on which Bohr said very little). On the few occasions when Bohr did discuss the latter, he typically appealed to the "heaviness" or the "macroscopic dimensions" of the measuring apparatus.

By the 1950s, few of Bohr's contemporaries found this to be a satisfactory resolution. In a letter to Aage Petersen in May 1957, Hugh Everett said he could find no justification for the dogmatic assumption that "macrosystems are relatively immune to quantum effects" (Everett to Petersen, May 31, 1957, quoted in Osnaghi et al. 2009, 106). Here Everett stressed that such a view was nothing more than a "postulate," as it "most certainly does not follow from wave mechanics." Indeed some of Bohr's defenders were inclined to agree. Indeed two years earlier Heisenberg (1955, 23) had drawn attention to the fact that the "classicality" of macroscopic systems was "by no means trivial" and demanded further explanation. In his contribution to the volume commemorating Bohr's seventieth birthday, Heisenberg acknowledged, "there are many solutions of the quantum-mechanical equations" for macroscopic systems "to which no analogous solutions can be found in classical physics" (Heisenberg 1955, 23). In an intriguing anticipation of later developments, Heisenberg surmised that macroscopic systems behave "classically," which is to say, they do not exhibit quantum superposition, because they cannot be isolated from their environments.

For his part, Bohr appears to have simply assumed that any macroscopic system serving as a measuring instrument *must be* describable by means of a classical approximation—otherwise we could not rely on such system to perform an epistemic function. It is simply the case that without such a presupposition, it would not be possible to acquire empirical knowledge. Why such a presupposition holds dynamically,

however, was not a question to which Bohr ever gave serious attention. The task then fell to Bohr's followers to resolve the problem of "what physical condition must be imposed on a quantum-theoretical system in order that it should show the features which we describe as 'classical' " (Weizsäcker 1971, 29). While many solutions were offered, based largely on thermodynamic considerations, it was not until the development of decoherence some decades later that physicists were able to provide anything like a satisfactory resolution to this problem (Camilleri and Schlosshauer 2015, 80–3). Bohr's functional-epistemological description of experiment, however, should be sharply distinguished from these later efforts, which gathered momentum in the 1960s, to provide a dynamical explanation of the quantum-classical transition.

Quantum objects and classical concepts

I want to now shift my attention from the description of the experimental apparatus to the description of the object of experimental inquiry. Here we confront the question of why classical concepts remain indispensable, not only for the description of the *functioning* of the experimental apparatus, but also for the interpretation of the *results* of measurement. Bohr's emphasis on the primacy of classical concepts in quantum theory has given rise to a host of different philosophical interpretations regarding his conception of the language (Petersen 1963 and 1968; Favrholdt 1993; MacKinnon 2011; Katsumori 2011). Undoubtedly Bohr's philosophical interpretation of quantum physics was shaped by his reflections on the limits of language and the problems of unambiguous communication. While Bohr's brief remarks on the subject do not amount to a fully developed philosophy of language, it is clear that he saw the importance of an epistemological focus on concepts in quantum physics.

Before proceeding, it is important to clarify a source of ambiguity that inevitably arises in Bohr's frequent use of the term "classical." This term, which is typically contrasted with the term "quantum," has been the source of misunderstanding, as it is frequently employed to refer variously to concepts, properties, phenomena, laws, or theories, without regard for these subtle but important distinctions. While Bohr often left it to his readers to decipher the precise meaning of ambiguous phrases such as "classical description," in his more careful moments he did distinguish between the use of classical concepts (such as position, charge, and momentum), employed in the description of quantum objects, and classical theories (such as Newtonian mechanics or Maxwell's field equations), used to describe the experimental apparatus. In his reply to the EPR paper, for example, Bohr (1935, 701) emphasized the necessity of using "classical *concepts* in the interpretation of all proper measurements, even though the classical *theories* do not suffice in accounting for the new types of regularities with which we are concerned in atomic physics" (emphasis added).

Bohr sometimes gave the impression he adopted a "verificationist conception of linguistic meaning" (Beller and Fine 1994, 18). However, such a misconception arises as a result of Bohr's somewhat ambiguous use of the term "definition" in the late 1920s and early 1930s.[17] Bohr's epistemological preoccupations with language lay less with the problems of meaning and reference, which would dominate analytic philosophy of language throughout the twentieth century, and more with the conditions of *applicability*

of concepts. As Bohr (1937, 293) explained at the 1936 Unity of Science Congress in Copenhagen, it is simply the case that in dealing with quantum phenomena, we must "use two different experimental arrangements, of which only one permits the unambiguous use of the concept of position, while only the other permits the application of the concept of momentum." This situation, which demonstrates the complementary features of a quantum description, brings to light the previously "*unrecognized presuppositions for an unambiguous use of our most simple concepts*" (289–90; emphasis added). As Bohr (1949, 211) later explained:

> A sentence like "we cannot know both the momentum and the position of an atomic object" raises at once questions as to the physical reality of two such attributes of the object, which can be answered only by referring to the [experimental] conditions for the unambiguous use of space-time concepts, on the one hand, and dynamical conservations laws on the other hand.

As this passage suggests, Bohr understood perfectly well that beyond certain experimental conditions, the use of such concepts becomes ambiguous. While the indeterminacy relations are frequently taken to express a "nonclassical" feature of quantum objects, for Bohr, they were a constant reminder that we are simply forced to use the concepts of classical physics, albeit within certain limits of applicability. Whenever we speak of the indeterminacy of an electron's position or momentum, we use the words "position" and "momentum," and to this extent we invariably fall back on the *use* of classical concepts. Here we can see vividly that the *use*, or better still the *usefulness*, of concepts like position and momentum did not imply for Bohr that objects *have* such well-defined properties. It is merely that such concepts help us to "grasp" the phenomena. As Friedrich Steinle (2012) has pointed out, "Concepts do not have a truth-value." Unlike theories, concepts cannot be true or false; they "cannot be proved, or be confirmed or disconfirmed as such." One can only say they are useful or not, "appropriate or not" in different epistemic contexts (105). To this extent, concepts may "prove their worth" in certain limited domains of applicability.

But why did Bohr think that classical concepts are indispensable? Recall that for Bohr (1939, 23–4), it is inherent in the very "aim of measurements" that the relevant properties of the experimental apparatus must be "compared with the corresponding properties of the object." This seemingly innocuous statement holds an important clue to a deeper understanding Bohr's view. In order to perform a measurement, we must presuppose, even if only provisionally, that the object "interacts" with our measuring instrument somewhere in space and time. As Bohr explained, "A measurement can mean nothing else than the unambiguous comparison of some property of the object under investigation with a corresponding property of another system, serving as a measuring instrument" (19). If we did not operate under the assumption that the interaction between the object of investigation and the measuring apparatus involves an exchange of energy and momentum somewhere in space and time, it would not be possible to *design* or *perform* an experiment, nor could we *communicate what had occurred* during the experiment.

To this extent, Bohr (1939, 19) held that a measurable property is one that can be defined (in an operational sense) by means of "everyday language or in the terminology of classical physics." Here it might be objected that spin constitutes a "nonclassical" measurable quantity, but even in the case of the Stern-Gerlach experiment, spin measurements involve the deflection of particles by means of a magnetic field. While such experiments performed with a beam of particles display the phenomenon of spin quantization, to which there is no corresponding phenomenon in classical physics, the deflection of each individual particle is ultimately interpreted as a change in the particle's *momentum* caused by the magnetic field in *space* and *time*. Indeed it is difficult to associate any other meaning with the term "deflection." As Bohr ([1929] 1987b, 94) would put it in 1929: "It lies in the nature of physical observation that all experience must be ultimately expressed in terms of classical concepts." Note Bohr's careful use of phrases like "expressed in terms of" in this context. It is not the case that every measurable quantity *is* a classical property, but rather that any measurement must *make use of* classical concepts. In this sense, the very *aims of measurement* force upon us the need to use classical concepts, even though, as Bohr (1937, 293) explicitly recognized, "the whole situation in atomic physics deprives of all meaning such inherent attributes as the idealizations of classical physics would ascribe to the object."

Seen in this light, it becomes apparent that the fundamental question for Bohr, in formulating his doctrine of classical concepts, was not "what concepts are necessary to theoretically represent the quantum objects," as a Kantian might have asked, but rather "what concepts are necessary to investigate such objects by means of experiment." This is, strictly speaking, an epistemological question, though it is not one that attracted the attention of many of Bohr's contemporaries. The properties we ascribe to objects when we perform a measurement (such as position, momentum, charge, spin), or better still, the *concepts we use* in describing the results of such measurements, are inextricably tied to the *epistemic aims of experimental inquiry*. To this extent, I would suggest we can make better sense of Bohr's words once we realize that, for him, *classical concepts are necessary insofar as they form part of the normative structure of experimental practice*. This normative view of concepts comes close to the view recently articulated by the philosopher Ingo Brigandt (2012, 97):

> Concepts refer to the world and represent the world in a certain fashion. Consequently, concepts have usually been construed as consisting of some beliefs about the concept's referent; an intension, an inferential role, a definition, or an analytic statement. However, note that the *epistemic goal* pursued by a concept's use operates on a different dimension than reference and inferential role. For the epistemic goal does not consist in a belief *about states of the world*—not even in a desire as to how aspects of the world studied by science should be like. Instead its goal is *for scientific practice* . . . Such goals have to be taken into account to understand the dynamic operation of science, including the epistemology of scientific concepts. (Emphasis in the original)

This is an account of concepts I suspect Bohr would have found congenial. As we have already noted, in many of the passages in which he articulated the indispensability of

classical concepts, Bohr (1949, 209) explicitly referred to the "aims of experiment," as well stressing that "by the word 'experiment' we refer to a situation where we can tell others what we have done and what we have learned." By the 1930s, Bohr was primarily interested in how concepts function in experimental practice, not how they function as linguistic elements in our theories. This is why he repeatedly insisted that the doctrine of classical concepts was inextricably intertwined with the pragmatics of communication and the conceptual presuppositions of experimental inquiry. It was in this sense that Bohr (1931, 692) remained convinced that "the language of Newton and Maxwell will remain the language of physics for all time."

Much of the confusion surrounding the doctrine of classical concepts, I suspect, stems from a tendency to misconstrue it as a doctrine pertaining primarily to theory, rather than experimental practice. Such misunderstandings have persisted in large part because Bohr frequently emphasized the relevance of the doctrine of classical concepts for any proper interpretation of the quantum formalism. Of course, formal considerations played an important role for Bohr, as is evident in his detailed analysis of the measurability of quantum fields in 1933 and his reply to the EPR paper in 1935. But such considerations were included primarily to explore the conditions of experimental inquiry. A deeper appreciation of this point may well provide an important clue to making sense of Bohr's reply to the EPR paper. As Beller and Fine (1994, 8) have noted, whereas Einstein, Podolsky, and Rosen characterized the state of a system by means of a wave functions, in his reconstruction of their argument, Bohr placed little emphasis on this way of formulating the problem of "completeness of quantum mechanics." For Bohr, the "object of knowledge" is not the object represented symbolically by a wave function, or a vector in Hilbert space, but rather, it is the object disclosed to us by means of the effects or traces registered on the experimental apparatus, which in turn must be interpreted by means of the concepts of classical physics. To put it in Kroes's (2003, 74) terms, "*the structural description of physical reality rests implicitly on a functional description of at least part of the world*" (emphasis added).

Here it remains to say a few words about Bohr's view of the relationship between classical concepts and the quantum formalism. As Manuel Bächtold (2008, 631) rightly points out, "the observables of quantum mechanics," defined by means of corresponding Hermitean operators, "are not strictly identical to the concepts of *classical physics*" (emphasis in the original). However, this poses no problem for Bohr's view of the indispensability of classical concepts. As we have seen, Bohr emphasized that the use of such concepts was indispensable for experimental praxis—that is, for conducting and interpreting experiments. He did not insist that they must appear explicitly as formal elements of the theory. But classical concepts serve another important function. As Bohr ([1929] 1987a, 16) explained, "It continues to be the application of these concepts alone which *makes it possible to relate the symbolism of the quantum theory to the data of experience*" (emphasis added). There is no suggestion here that the observables of quantum mechanics (or quantum electrodynamics for that matter) are *identical* to classical concepts. Rather, Bohr's point was that we can only connect these observables, as part of the formalism, to actual measurements by means of the concepts of classical physics.

4. Conclusions

"It would be a misconception," Bohr ([1929] 1987a, 16) wrote in 1929, "to believe that the difficulties of atomic theory may be evaded by eventually replacing the concepts of classical physics by new conceptual forms." In the wake of the development of quantum theory, it was no longer likely "that the fundamental concepts of classical theories will ever become superfluous for the description of physical experience" (16). Such remarks have often been interpreted as indicating some kind of Kantian element in Bohr's philosophy. Yet the real problem for Bohr was not so much that we lack the cognitive faculties or the capacity to develop a new conceptual framework or a new *theoretical language*, but that the very success of quantum mechanics, and for that matter, quantum field theory, lies precisely in accounting for a vast array of *experimental evidence*, which, by its very nature, depends on the use of classical concepts.

Here we might ask whether, or to what extent, the reading of Bohr's doctrine of classical concepts I have presented here reflects the influence of the Kantian or pragmatist philosophical traditions—both of which are visible in Høffding's work. The lack of clues within Bohr's published writings as to his philosophical influences has made this a difficult question to answer. While there is certainly a Kantian ring to Bohr's doctrine of classical concepts, and we do find evidence of the use of Kantian terminology at certain points in Bohr's writings,[18] Bohr rarely alluded to Kant in his writings, and when he did refer to philosophical systems, he often highlighted the extent to which the new developments in modern physics brought to light the need for new perspectives. As Bohr (1949, 239) himself made clear in his contribution to the 1949 Einstein volume, "Even in the great epoch of critical philosophy in the former century, there was only a question to what extent *a priori* arguments could be made for the adequacy of space-time coordination and causal connection of experience, but never a question of rational generalizations or inherent limitations of such categories of human thinking." Here Bohr noted the difficulties of reaching "mutual understanding not only between philosophers and physicists, but even between physicists of different schools" (240). My sense is that Bohr found these difficulties particularly acute, because his epistemological preoccupations were rather different from those of most of his contemporaries.

While I have not pursued a contextualist reading of Bohr here, I remain convinced that this is a fruitful avenue of historical inquiry in spite of the formidable difficulties it presents. Yet, such an approach needs to proceed in conjunction with careful reconstructive analysis of his texts, with due attention to the way in which Bohr may have spoken the language of early-twentieth-century philosophy, while investing it with new meaning. This remains a task for the future. My aim here has not been to present a systematic and coherent reading of Bohr's philosophy, but merely to bring out certain crucial features of his epistemological viewpoint, which have been neglected or obscured by many commentators. In doing so, I have taken my cues from Collingwood's "logic of question and answer."

Finally, it is worth emphasizing that it has not been my aim in this chapter to defend Bohr's doctrine of classical concepts. There are many places where I think Bohr's arguments are open to criticism. I have said nothing here about the arguments contained in

his reply to the EPR paper, nor have I examined the formal analysis of complementarity that Bohr presented in several papers in the 1930s invoking the limitations forced on us by the quantum of action. Furthermore, even if one accepts that we *do* use classical concepts in making sense of measurements, one might doubt whether this entails the stronger conclusion that we *must* use classical concepts.[19] Ultimately this rests on whether one thinks Bohr was right in asserting that a functional-epistemological description of experiment in physics must, by its very nature, employ the conceptual framework of classical physics. Bohr's views are not immune to criticism. Yet, if we are to mount a serious critique of Bohr, I would maintain we must first understand him—as a philosopher of experiment.

Archival sources and abbreviations

AHQP Archive for the History of Quantum Physics. Microfilm. American Philosophical Society, Philadelphia.
LRP Léon Rosenfeld Papers, Niels Bohr Archive, Copenhagen.
NBA Niels Bohr Archive, Niels Bohr Institute, Copenhagen.

Notes

1 According to a classic story recounted by David Bohm: "Bohr's statements are designed to cancel out in the first approximation with regard to their meaning, and in the second approximation with regard to their connotation. It is only in the third approximation that one can hope to find what Bohr wants to say" (Bohm to Rosenfeld, December 13, 1966, LRP "Epistemology").
2 Even the physicists commonly associated with the "Copenhagen school" were often guilty of gross misrepresentations. A classic example can be found in Heisenberg's widely read *The Physical Principles of the Quantum Theory*, based on his 1929 Chicago lectures. In the preface Heisenberg (1930, x) announced, "The book contains nothing that is not to be found in previous publications, particularly in the investigations of Bohr." Yet, his account of wave-particle duality (which he later termed "wave-particle equivalence") and complementarity diverged in several critical respects from the view we find in Bohr's writings of the late 1920s (Camilleri 2009b, 77–84, 112–23). While Heisenberg presented himself as a faithful disciple of Bohr, by the mid-1930s crucial differences had emerged in their publications and private correspondence, which were not always acknowledged, and often went unnoticed.
3 As Catherine Chevalley (1999, 59) 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 emerged as a frame for philosophical discussion only in the mid-1950s." There are now good reasons to think that the "Copenhagen interpretation" was a retrospective postwar invention (Howard 2004; Camilleri 2009a).
4 Pauli to Heisenberg, May 13, 1954, in Pauli (1996, 620–1); Pauli to Rosenfeld, September 28, 1954, LRP "Correspondence particulière"; Born to Rosenfeld, January

21, 1953, Born sent Rosenfeld a ten-page typescript entitled "Dialectical Materialism and Modern Physics" on October 24, 1955, LRP "Correspondence particulière."

5 In an exchange of correspondence between Bohm and Rosenfeld in December 1966, we get a glimpse of the divisions among Bohr's spokesmen. Bohm had informed Rosenfeld that he had found Petersen's discussion of Bohr's views on language most illuminating. Yet Rosenfeld remained suspicious of the account Bohm had presented, and responded by saying, "I suspect that you have been badly misinformed by Petersen, or that you have perhaps misunderstood what Petersen told you" (Rosenfeld to Bohm, December 6, 1966; Bohm to Rosenfeld, December 13, 1966, LRP "Epistemology").

6 Abraham Pais (1991, 421) recalled that after attending a philosophical meeting, Bohr declared to Jens Lendahl: "I have made a great discovery, a very great discovery: all that philosophers have ever written is pure drivel."

7 There is now an extensive literature on the relationship between Bohr and Kantian philosophy (Honner 1982; Kaiser 1992; Folse 1985, 217–21; Murdoch 1987, 229–31; Folse 1978; Pringe 2009; Bitbol 2013).

8 The contributions by Favrholdt, Faye, Folse, Krips, McKinnon, and Murdoch in the 1994 volume edited by Faye and Folse, provide different perspectives on the extent to which Bohr's views are best construed as a "realist" or "antirealist" (Faye and Folse 1994).

9 It should be noted that the primary aim of many of the these works is not to provide a close reading of Bohr's own philosophical position, but rather to attempt to go "beyond Bohr," as it were, in using his work as a source of inspiration for developing new philosophical insights.

10 David Favrholdt has criticized Faye's thesis, in arguing that Bohr's notion of complementarity owes little to Høffding and to his Danish intellectual heritage, but was forged as an original response to the specific problems of quantum theory that Bohr confronted him as a physicist (Favrholdt 1992). See also Moreira (1994) and Faye (1994).

11 In addition to Chevalley both Steen Brock (2003) and Voetmann Christiansen (2006) have argued that Bohr's views reflect the influence of a particular strand of neo-Kantianism, which runs from Helmholtz through Herz to Høffding.

12 See, for example, Zinkernagel (2016).

13 As Plotnitsky (2012, 89) has argued, "Bohr saw quantum electrodynamics and other forms of quantum field theory as confirming his key ideas concerning the epistemology of quantum phenomena and quantum mechanics, and possibly giving these ideas more radical dimensions." But it is also evident that "quantum electrodynamics and quantum field theory had a shaping reciprocal impact on Bohr's work on quantum mechanics and complementarity."

14 Rosenfeld (1967, 115) later recalled that 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." Both Murdoch and Faye have also argued that Bohr's thought undergoes a transformation in 1935. As Murdoch (1987, 145) puts it, "After 1935 Bohr expressed the indispensability thesis in what may be called 'semantic' as distinct from 'ontic' terms." In a similar vein Jan Faye (1991, 186) writes: "After 1935 his grounds for asserting complementarity were not so much epistemological as they were conceptual or semantical."

15 In his book on causality entitled *Determinism and Indeterminism in Modern Physics*, Cassirer ([1936] 1956, 194–5) declared: "There seems to be no return to the lost

paradise of classical concepts . . . If it appears that certain concepts, such as those of position, velocity, or of the mass of an individual electron can no longer be filled with definite empirical content, we have to exclude them from the theoretical system of physics, important and fruitful though their function may have been."

16 See Zinkernagel (2016).

17 Mara Beller and Arthur Fine (1994, 18) have argued that Bohr adopted "an operational and verificationist conception of linguistic meaning" in his reply to the EPR paper. By contrast, Edward MacKinnon (1982, 271) has argued, "Bohr did not believe that the meaning of such terms as 'position' and 'trajectory' could be determined by such operational definitions . . . Instead, the critical problem requiring analysis accordingly is how such already meaningful concepts function in quantum contexts." Much of the confusion arises as a result of Bohr's ambiguous use of the term "definition" in many crucial passages in the late 1920s. Yet, it is evident from several other passages that the term "definition" is used interchangeably with the term "application" or "use." As MacKinnon (1982, 274) has argued: "When Bohr speaks of 'definition,' he does not mean stipulating the meaning of some term of some context. He refers rather to rules governing the usage, in quantum contexts, of a term whose classical meaning is already determined." MacKinnon's reading is supported by a close reading of several of Bohr's writings in the 1930s, in which we find repeated reference to the conditions of applicability of classical concepts.

18 Bohr ([1929] 1987a, 5, 16) refers to "forms of perception" in his "introductory Survey" in 1929.

19 Weizsäcker (1952, 128, 130), for example, was inclined to adopt the weaker view: "We ought not to say, 'Every experiment that is even possible *must* be classically described,' but 'Every actual experiment known to us *is* classically described, and we do not know how to proceed otherwise.' This statement is not sufficient to prove that the proposition is *a priori* true for all, merely possible future knowledge; nor is this demanded by the concrete scientific situation. It is enough for us to know that it is *a priori* valid for quantum mechanics … We have resolved not to say, 'Every experiment *must* be classically described' but simply, 'Every experiment *is* classically described.' Thus the factual, we might almost say historical situation of physics is made basic to our propositions" (emphasis in the original).

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