The Challenge of Quantum Mechanics to the Rationality of Science: Philosophers of Science on Bohr

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Abstract
Bohr's work in quantum mechanics posed a challenge to philosophers of science, who struggled with the question of whether and to what degree his theories and methods could be considered rational. This paper focuses on Popper, Feyerabend, Lakatos and Kuhn, all of whom recognized some irrational, dogmatic, paradoxical or even inconsistent features in Bohr's work. Popper, Feyerabend and Lakatos expressed strong criticism of Bohr's approach to quantum physics, while Kuhn argued that such criticism was unlikely to be fruitful: progress in science is generally not made through philosophical reflection. Feyerabend's criticism of Bohr gradually weakened, as he gained a more detailed understanding of the development of Bohr's views on quantum mechanics, and this went together with an increasingly critical view of normative philosophy of science and was instrumental to his conversion to 'anarchism'. This paper aims to show that quantum mechanics played a central role in their debates and disagreements on the rationality of science and the possibility of a normative philosophy of science.

Introduction
Quantum mechanics is one of the most successful and prestigious scientific theories of the twentieth century; at the same time, it is routinely presented as being somehow puzzling, paradoxical, and impossible to understand. Quantum physicists have themselves contributed to this image: from the beginning, the words 'paradoxical' and 'irrational' were often used in connection with quantum physics. For example, Niels Bohr remarked that the discontinuity introduced in physics through the quantum of action constitutes an element of irrationality (Bohr 1931, 59), and spoke of the "paradoxical character of the question of the nature of light and material particles" (Bohr 1931, 39). Werner Heisenberg wrote that "we cannot escape the paradox of quantum theory", which lies in the fact that any experiment in physics has to be described with the concepts of classical physics, even though the applicability of these concepts is limited (Heisenberg 1958, 56).

This paper explores how philosophers of science in the twentieth century have struggled with the question of the rationality of quantum mechanics. Whereas philosophers of science have often treated Einstein's theory of relativity as a model for good science, it was very different with quantum mechanics. Quantum mechanics seemed to violate ideas of what a scientific theory should look like, and even its consistency could be questioned; its success and prestige therefore posed a challenge.

The paper focuses on Karl Popper, Paul Feyerabend, Imre Lakatos and Thomas Kuhn. All four of them engaged with the history and philosophy of quantum mechanics; in particular Popper and Feyerabend wrote very extensively on the philosophy of quantum mechanics, and Kuhn was in charge of the pioneering project *Sources for History of Quantum Physics*, for which quantum physicists were interviewed and source material was collected. This paper aims to show that their work on the history and philosophy of quantum mechanics was strongly connected with their general views in philosophy of science, and that quantum mechanics was often central to the debates and disagreements between them.

All four of these philosophers of science engaged in particular with the views of Niels Bohr. Bohr was one of the central figures in the development of quantum mechanics, and an imposing personality who was held in high regard in the community of physicists. He was more philosophically minded than most physicists, and was regarded as an intellectual authority when it came to the interpretation of quantum mechanics: the dominant interpretation of quantum mechanics, the Copenhagen interpretation, was named after Bohr's place of residence. It has later been argued that it was in particular Heisenberg who promoted the idea of the 'Copenhagen interpretation' as an account of quantum mechanics that a large majority of quantum physicists could agree on, that Heisenberg's views were actually substantially different from those of Bohr, and that the differences between the views of physicists associated with the Copenhagen interpretation were so significant that it may be better not to speak of a unified 'Copenhagen interpretation' at all (Howard 2004; Camilleri 2009). Nevertheless, the widespread use of the term 'Copenhagen interpretation' from the 1950s onwards reinforced the authority of Bohr. At the same time, Bohr's philosophical writings on quantum physics were very difficult to read and led to much debate about how his views were to be interpreted.

Popper, Feyerabend, Lakatos and Kuhn each struggled in different ways to make sense of Bohr's work in quantum mechanics. Although also appreciation for Bohr's sophisticated ideas can be found (especially in Feyerabend's later writings), the reception of Bohr's views in philosophy of science was generally a troubled one. Quantum mechanics, and in particular Bohr's writings on quantum mechanics, raised questions about the role of rationality and consistency in science. These, in turn, led to a debate about the normative role of philosophy of science: can and should philosophers of science criticize certain aspects of quantum mechanics, despite the fact that quantum mechanics has been very successful as a scientific theory?

The paper is divided into four parts, focusing on Popper, Feyerabend, Lakatos and Kuhn, respectively.

1. Popper

Throughout his career as a philosopher of science, Popper thought that there was something deeply wrong with quantum mechanics. He expressed several concerns with the theory of quantum mechanics and with the approach taken by quantum physicists. Popper's views on quantum physics have been discussed among others in Del Santo (2019) and Howard (2012).

A central issue for Popper was the lack of a realist account of quantum processes: quantum mechanics yields only probabilistic predictions for measurement outcomes, and no description of what happens between measurements. In Popper's view, the proper scientific attitude is a realist one: a scientist should try to understand the world and should search for truth. Despite the fact that according to Popper's falsificationism, a theory can never be *proven* to be true, truth remains the ultimate aim of science. Popper connected realism with rationalism: a search for truth and understanding is part of the 'rationalist tradition' we inherited from ancient Greece (Popper 1963, 136). Instrumentalism, on the other hand, continues the tradition of Cardinal Bellarmino and Bishop Berkeley, who opposed the new insights of Galileo and Newton (Popper 1963, 133-34). In Popper's view, quantum physicists had adopted an instrumentalist attitude: instead of trying to actually understand processes at the quantum level, quantum physicists were merely concerned with accounting for measurement outcomes and tended to focus on formal and technical aspects of quantum physics. By adopting an instrumentalist attitude, physicists had thus unwittingly sided with an anti-scientific tradition:

> In the great fight over the fundamental issue of the rationalist tradition – *whether or not the human intellect, unaided by divine revelation, could uncover some of the secrets of our world* – most of the leaders of the quantum theory (except Einstein and Schrödinger) have taken sides with the Cardinal and the Bishop against Galileo, Kepler, and Newton. (Popper 1982, 173-74, originally written in the 1950s; see also Popper 1963, 133).

In Popper's view, the general acceptance of instrumentalism by quantum physicists could be explained by the fact that quantum mechanics was both very hard to interpret and very successful in its applications: this led to a focus on technical aspects, while interpretative questions were simply put aside (Popper 1963, 134). In fact, Popper argued, most physicists did not reflect on these issues, and did not realize that by adopting an instrumentalist attitude, they had accepted a problematic philosophical position.

Popper moreover objected to what he saw as the subjectivism of quantum mechanics. In Popper's view, rather than providing a description of physical reality, the Copenhagen interpretation of quantum mechanics merely deals with our observations. According to the Copenhagen interpretation the observer, or subject, interferes with the observed phenomena in such a way that we can have no knowledge of quantum phenomena independent of our interaction with them (Popper 1967, 10). Popper had an idiosyncratic interpretation of the problem: according to Popper, the subjectivism of quantum mechanics ultimately stemmed from the use of a subjective notion of probability, according to which probabilities are ultimately interpreted in terms of ignorance. Popper argued that if probabilities are interpreted in this way, also the Heisenberg uncertainty relations must be understood subjectively, as an expression of limit to our knowledge of nature rather than an intrinsic property of nature. Thereby, quantum mechanics as a whole becomes subjective; according to Popper, the subjective notion of probability has "created all those irrationalist symptoms" in quantum mechanics, such as the interference of the subject with the observed object (Popper 1982, 99; Del Santo 2019).

For this reason, Popper's own work in the foundations of quantum mechanics was largely focused on the notion of probability. In particular, he developed an interpretation of probabilities known as the propensity interpretation, which made it possible to assign objective probabilities to individual events. This interpretation of probabilities was at the basis of Popper's own interpretation of quantum mechanics, which is a type of statistical or ensemble interpretation (Howard 2012; Del Santo 2019; Del Santo and Freire 2021). Although Popper's propensity interpretation has been acknowledged as a major step in the philosophy of probability and a promising contribution to the foundations of quantum mechanics, the general consensus is that it does not solve the difficulties in the foundations of quantum mechanics.[1]

Although Popper's university studies had included physics, he was a philosopher rather than a physicist. Popper's criticism of quantum physics was philosophically motivated, and his perspective was that of a philosopher of science commenting on current physics. Popper explicitly reflected on the relation between physics and philosophy: he noted that many physicists were uninterested in philosophy and suspicious of philosophy of physics, but argued that philosophy could nevertheless

[1] In particular, it seems that Popper's propensity interpretation does not solve the measurement problem. For a critical analysis of Popper's constructive contributions to the field of the foundations of quantum mechanics, see Howard (2012), Del Santo (2019), Del Santo and Freire (2021).

provide useful contributions to physics. He argued that scientists always work with a certain metaphysical research program, and that it is not possible to do science without any presuppositions or preconceived notions (Popper 1982, 161). Given that this is the case, it is useful to make the philosophical presuppositions that underlie a scientific theory explicit and reflect on them, and this is something with which philosophers could help. Furthermore, Popper saw a danger of overspecialization and of physics becoming merely technical physics, and thought that philosophers could bring a broader perspective to physics.

Popper did, at least initially, have some insecurities about his role as a philosopher commenting on quantum physics, and he doubted whether physicists would take his views seriously. In his autobiography, he recalls learning that he had made a mistake in a thought experiment on quantum physics that he had devised: "I took this mistake very much to heart; I did not know at that time that even Einstein had made some similar mistakes, and I thought that my blunder proved my incompetence" (Popper 2002, 104). When, at the Congress for Scientific Philosophy in Copenhagen in 1936, Popper learnt that Einstein had made a similar mistake, this did not provide comfort: "I remained for years greatly discouraged. I could not get over my mistaken thought experiment, and although it is, I think, quite right to grieve over any of one's mistakes, I think now that I attributed too much weight to it" (Popper 2002, 106). Eventually, however, Popper regained his confidence, and interacted extensively with quantum physicists; Del Santo (2019) has shown how in the 1960s, Popper became part of the *Denkkollektiv* of physicists concerned with the foundations of quantum mechanics.

At the above-mentioned Congress in 1936, Popper met Bohr, who made a great impression on him:

> ...he impressed me as the most wonderful person I had ever met or would be likely to meet. He was everything a great and good man could be. And he was irresistible. I felt that I must be wrong about quantum mechanics, even though I certainly could not say that I now understood it, rationally, in Bohr's sense: I did not. But I was overwhelmed. (Popper 1982, 9).

In his autobiography, Popper writes about this meeting with Bohr:

> I left with an overwhelming impression of Bohr's kindness, brilliance, and enthusiasm; I also felt little doubt that he was right and I wrong. Yet I could not persuade myself that I understood Bohr's "complementarity", and I began to doubt whether anybody else understood it, though clearly some were persuaded that they did. (Popper 2002, 104-5).

According to Bohr's principle of complementarity, the results of measurements in quantum mechanics have to be described in terms of concepts of classical physics, for example 'position' and 'momentum', despite the fact that these concepts have a limited applicability. In particular, there are pairs of concepts, such as 'position' and 'momentum', and perhaps also 'wave' and 'particle', that are both needed to describe the state of quantum systems, but that cannot be applied simultaneously: one can either attribute an exact position or an exact momentum to a particle, not both at the same time.[2]

Popper's feeling that he did not fully understand Bohr's notion of complementarity lingered, and when writing about Bohr's account of quantum mechanics he sometimes added disclaimers,

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[2] Held (1994) argues that in Bohr's mature conception of complementarity, there is no complementary relation between waves and particles.

stating that he wasn't sure whether he had understood Bohr correctly (see e.g. Popper 1982, 149). To Popper, it seemed that Bohr was not an outright positivist or instrumentalist like Heisenberg, who explicitly aimed to eliminate unobservables from quantum mechanics (Popper 1982, 9, 156). However, as Popper understood it, the core idea of Bohr's complementarity principle was that there is a limit to our capacity to understand quantum mechanics. Popper interpreted Bohr as stating that we need to use classical concepts such as 'particle' and 'wave' because these are the terms in which we can understand nature. These concepts are however incompatible with each other, and therefore physics can only remain consistent as long as they are not used at the same time, which is essentially what is accomplished with Bohr's principle of complementarity. For Popper, this meant a renunciation of the attempt at a full understanding of quantum processes (Popper 1963, 135).

Popper was also critical of Bohr's response to the EPR paradox, a thought experiment proposed by Einstein, Podolsky and Rosen in 1935 that was intended to show that quantum mechanics did not provide a complete description of physical reality. In Popper's view, Bohr's response to this paradox was unsatisfactory: Bohr appealed to complementarity in an *ad hoc* way, merely circumventing the problem while avoiding outright contradictions (Popper 1963, 135). In a manuscript first written in the 1950s, Popper wrote that he feared that Bohr's principle of complementarity could not be rationally criticized: "it can only be accepted or *denounced* – perhaps as being *ad hoc*, or as being irrational, or as being hopelessly vague" (Popper 1982, 103). Having puzzled over Bohr's views on quantum mechanics, Popper thus ultimately judged them to be unsatisfactory and objectionable from a methodological point of view.

Popper thus made strong normative judgments about quantum mechanics: he argued that the instrumentalist attitude found in quantum physics was detrimental to science, objected to the subjective interpretation of probability in quantum mechanics, and thought that Bohr's approach to quantum physics essentially meant giving up on the possibility of a full understanding of quantum phenomena. This critical analysis of the situation in quantum mechanics motivated his own work in the foundations of quantum physics.

2. Feyerabend

During the 1950s and 1960s, the philosophy of quantum mechanics was the main research area of Paul Feyerabend. His views on quantum mechanics and their relation to his philosophy of science have been discussed in a number of publications in the last few years, including Van Strien (2020a), on which section is largely based though with differences in structure and emphasis, as well as Kuby (2021) and Del Santo (2022). Feyerabend's relationship with Popper has been discussed e.g. by Collodel (2016). The debate between Popper and Feyerabend on quantum physics is discussed in Del Santo (2019) and (2022), in which in particular the role of personal factors is emphasized, as well as some issues which are largely omitted from this section, such as their dispute over Von Neuman's impossibility proof in which Popper accused Feyerabend of plagiarism. The present section has a more philosophical focus and discusses those aspects of Feyerabend's writings on quantum physics which are particularly relevant for understanding his views on the rationality of Bohr's account of quantum mechanics, and his differences with Popper, Kuhn and Lakatos on this issue.

Feyerabend studied physics and philosophy in Vienna, and after having received his doctorate in philosophy, he spent a period at the London School of Economics to work with Popper, in 1952-53. Feyerabend's views of quantum physics as well as his relation to Popper changed significantly over time, and this development can for our purposes be divided into three phases.[3]

During the *first phase,* from ca. 1952 until the late 1950s, Feyerabend's views were close to those of Popper.[4] Like Popper, Feyerabend was very critical of the standard interpretation of quantum mechanics, and his criticism was philosophically motivated. In particular, like Popper, Feyerabend argued for realism in science, and objected to the anti-realism and positivism which he thought was characteristic of quantum mechanics.

Feyerabend's arguments for realism were somewhat different from those of Popper, and had a stronger emphasis on methodology. Feyerabend argued that a realist attitude tends to contribute to scientific progress: formulating hypotheses about unobserved processes and searching for explanations is often fruitful, and it is conducive to scientific process to take theories to be literally true, rather than seeing them merely as useful instruments. Despite the differences, there is a clear Popperian element in Feyerabend's arguments for realism: Feyerabend argued that we should assume that our current scientific theories describe reality as it really is and are universally valid in order to make them maximally falsifiable, and then test them relentlessly (Feyerabend 1964, 306).[5]

This, in Feyerabend's view, was not happening in quantum mechanics. During most of the 1950s, he regarded quantum mechanics as the product of a misguided positivism, according to which a theory should only deal with observations and measurements and should make no claims about an unobserved reality. Feyerabend argued that positivism is inherently conservative: when we take observations at face value, as stable elements that cannot be doubted or revised, and when we then take knowledge based on such observations to be certain, we remain stuck to current ways of seeing the world and block possibilities for fundamental change. He specifically took Bohr's account of quantum mechanics to be an example of a positivist attitude in modern science (Feyerabend 1958a; Van Strien 2020a, section 2.3). He recognized positivist and conservative elements in particular in two principles proposed by Bohr, namely the principle of complementarity and the correspondence principle.

As we have seen, central to Bohr's principle of complementarity is the idea that measurement outcomes necessarily have to be stated in terms of classical concepts, despite the limited applicability of these concepts. In Feyerabend's interpretation, the reason why Bohr held on to classical concepts was that these were closely connected with observation. He characterized Bohr's conviction of the indispensability of classical concepts as a "defeatist attitude", and argued that there was no reason to assume that we would not be able to develop new concepts that would enable a fuller understanding of quantum phenomena (Feyerabend 1958a, 152).

The second of Bohr's principles which Feyerabend criticized as inherently conservative was the correspondence principle, which played a central role in the development of quantum mechanics. This principle is usually defined as the requirement that predictions of quantum mechanics have to agree with those of classical mechanics in the classical limit. This is somewhat different from how Bohr himself defined the principle, but also for Bohr, the correspondence principle meant that quantum mechanics had to agree with classical mechanics in some specific way (Bokulich and Bokulich 2020). Bohr spoke of quantum mechanics as a "rational generalization" of

[3] This division deviates from that given in Kuby and Fraser (2022, 1). In particular, Kuby and Fraser distinguish an earlier phase during Feyerabend's studies (1946-1951), and do not make a distinction between what I describe as the second and third phase.

[4] On the relation between Popper and Feyerabend, see Collodel (2016), Del Santo (2022).

[5] Feyerabend therefore argued that theories should be "developed in their strongest possible form, i.e. as descriptions of reality rather than as mere instruments of successful prediction" (Feyerabend 1964, 306).

classical mechanics (Bohr quoted in Bokulich and Bokulich 2005, 349). In Feyerabend's view, Bohr constructed quantum mechanics by holding on to the concepts and laws of classical physics as much as possible, rather than by developing a new theory based on new foundations (Feyerabend 1958b, 61). This may initially have been a very successful strategy, but it is a strategy which must eventually lead to stagnation (Feyerabend 1958b, 72).

For Feyerabend, Bohr's scientific methods negatively compared to those of Einstein, who had developed his theory of relativity by making bold assumptions, going beyond the observed facts.[6] Also Popper pointed out Einstein's development of the theory of relativity as a model of good science. Relativity theory was based on bold hypotheses which yielded highly unexpected and exact predictions, which could then be tested: in Popper's view, this was the mark of good science. Quantum mechanics, in contrast, was designed with the instrumentalist aim to account for the observed phenomena while avoiding hypotheses about physical reality. Popper argued that as quantum mechanics merely aimed to account for the observed phenomena, its testability was limited (Popper 1963, 152).

The *second phase* in the development of Feyerabend views on quantum mechanics can be characterized by an increasing conviction that the current theory of quantum mechanics is based on strong physical arguments and that a criticism on a philosophical level therefore does not suffice, which went together with an increasingly critical attitude to Popper, and the development of arguments for pluralism. Some signs of this development can already be seen in 1957, when Feyerabend wrote to Popper: "I think there is much more in the Copenhagen-interpretation (as it has been discussed by *Bohr,* not by the Bohrians) than I thought some time ago when I did not know it well enough" (Feyerabend, quoted in Kuby 2021, 137). As Kuby (2021) has shown, Feyerabend came to this realization after he found out that his own account of measurement in quantum physics, which he presented at a conference in 1957, was in fact close to that of Bohr.[7] This may have been the start of a gradual change in Feyerabend's opinions on Bohr. Whereas in 1958, Feyerabend still argued that Bohr's account of quantum mechanics was based on a misguided positivistic philosophy (Feyerabend 1958a), he soon became convinced that Bohr's work in quantum mechanics had primarily been driven by experimental results and reasoning within physics, rather than by a philosophical point of view (Feyerabend 1962, 121, 162). Moreover, he came to the conclusion that the lack of a realistic description of quantum processes was an inherent feature of quantum mechanics:

> The instrumentalism of the quantum theory is therefore not a philosophical manoeuvre that has been wilfully superimposed upon a theory which would have looked much better when interpreted in a realistic fashion. It is a demand for theory construction which was imposed from the beginning and, in accordance with which, part of the quantum theory was actually obtained. (Feyerabend 1962, 121, footnote 62)

The current theory of quantum mechanics could not be interpreted realistically.[8] What this meant, according to Feyerabend, was that it did not suffice to criticize the theory on a philosophical level or

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[6] Feyerabend (1963, 191); Feyerabend (1965, 218); on Einstein's influence on Feyerabend, see also Oberheim (2016).

[7] On the account of measurement that Feyerabend proposed in this paper, see also Kuby and Fraser (2022).

[8] Kuby (2021) argues that this presented a fundamental problem for Feyerabend, since it was not compatible with his methodological demand that scientific theories be interpreted realistically. I am not sure whether Feyerabend himself saw this as a problem: he could still hold on to the methodological requirement that

to argue for a different interpretation of the theory, as Popper did. Rather, the only way to get rid of the instrumentalism of quantum mechanics and to restore realism would be to propose a different theory of quantum physics (Feyerabend 1962, 114).

From the late 1950s onwards, Feyerabend thus gradually turned from a supporter into a critic of Popper. He began to defend quantum mechanics against Popper's criticisms, which he came to regard as superficial. In Feyerabend's reading, Popper only criticized the standard theory of quantum mechanics on a philosophical level and argued for a different interpretation of the existing theory, but this did not suffice: the strongest arguments for quantum theory in its current form were not philosophical arguments, but arguments from physics, and therefore criticisms should also be made on the level of physics (Feyerabend 1964, 361; Van Strien 2020a, section 2.4).[9] For example, Feyerabend now argued that Bohr's principle of complementarity was not just based on positivist philosophy, but was in fact well supported by physical arguments and embedded in the theory of quantum mechanics (Feyerabend 1958b). He furthermore argued that the indeterminateness of state descriptions in quantum mechanics followed from experimental findings.[10] This does not mean that we are forced to accept the principle of complementarity or the indeterminateness of state descriptions. The principle of complementarity may in principle be avoided by developing a different theory of quantum mechanics based on new concepts (Feyerabend 1958b). And the indeterminateness of state descriptions may be avoided by developing a new theory which contradicts at least one of the experimental findings from which it is derived. Such a contradiction of empirical evidence may be defensible: within a new theoretical framework, previous experiments may be reinterpreted and their implications may change in such a way that a previous experimental finding no longer holds up. Therefore, according to Feyerabend, in order to develop a new theory of quantum mechanics, it would be necessary to contradict current empirical evidence and to develop a new theoretical framework for quantum physics, based on new concepts.[11]

Thus, although Feyerabend now thought that it was not possible to give a realistic interpretation of the theory of quantum mechanics in its current form, he still thought it worthwhile to pursue an alternative theory that does allow for a realistic interpretation:

> ... there still remains the fact that theories which *do* admit of a realistic interpretation are definitely preferable to theories which do not. It was this belief which has inspired Einstein,

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scientists should aim to develop theories which do allow for a realist interpretation. In this case, physicists should develop an alternative theory of quantum physics.

[9] Feyerabend argued that the writings of a number of quantum physicists, in particular Heisenberg, were misleading in this regard: they had a tendency defend their views through an appeal to positivist philosophy, so one could be led to think that one could argue against their accounts of quantum mechanics by refuting their positivistic arguments.

[10] According to Feyerabend, the indeterminateness of state descriptions in quantum mechanics follows from a number of principles which are each based on experimental findings, namely the quantum postulate, the duality of light and matter, and the conservation of energy and momentum (Feyerabend 1962, 107-114; 1964). Therefore, in order to develop a theory of quantum mechanics that does not share the feature of indeterminateness of state descriptions, one has to deny at least one of these. Moreover, in order to develop a realist theory of quantum physics, it was needed to circumvent a theorem derived by Von Neumann in 1932, which was often interpreted as proving the impossibility of 'hidden variable' theories of quantum mechanics. Feyerabend argued that since Von Neumann's derivation uses the postulates of quantum mechanics as premises, it could be circumvented by developing an alternative theory of quantum mechanics, based on different postulates (Feyerabend 1962, 167). As Dieks (2017) shows, this is actually in agreement with Von Neumann's own views.

[11] Feyerabend argues that "any attempt to give a realistic account of the behaviour of the elementary particles is bound to be inconsistent with some very highly confirmed theories" (Feyerabend 1964, 301).

> Schrödinger, Bohm, Vigier and others to look for a modification of the present theory that makes realism again possible. The main aim of the present article is to show that there are no valid reasons to assume that this valiant attempt is bound to be unsuccessful. (Feyerabend 1962, footnote 49)

Feyerabend supported his conviction that it was desirable to develop an alternative theory of quantum mechanics by a general argument for theoretical pluralism in science. Feyerabend's pluralism is not incompatible with his particular brand of realism: for Feyerabend, realism does not entail that one single theory should be taken to be true, to the exclusion of all alternatives. Realism does entail that scientific theories should go beyond observations and should attempt to describe physical reality; but there is no reason why scientists shouldn't make *several different* attempts at describing physical reality.[12] And like his arguments for realism, also his argument for pluralism was essentially methodological: Feyerabend argued that developing alternatives to an established theory is essential for properly testing this theory (Van Strien 2020a). This is the case because it is sometimes only in light of a new theory that an observation turns out to be a refuting instance of an established theory. Feyerabend argued that "there exist potential difficulties for any theory that can be detected only with the help of further theories. If this is correct, then the development of such further theories is demanded by the principle of testability, according to which it is the task of the scientist relentlessly to test whatever theory he possesses" (Feyerabend 1964, 306). Not only did Feyerabend develop this argument for pluralism in the context of quantum mechanics, he also developed it in conversation with the quantum physicist David Bohm, who had challenged the Copenhagen interpretation and developed an alternative interpretation of quantum mechanics (Van Strien 2020a).[13]

There is a clear Popperian element in Feyerabend's claim that pluralism is necessary in order to properly test theories, but Feyerabend went beyond Popper by explicitly arguing that rather than merely introducing and testing various hypotheses, scientists should develop entire rival theoretical frameworks, and in order to do so, they should (at least from time to time) go against empirical evidence.[14]

In 1961, Feyerabend read a draft of Kuhn's *Structure of Scientific Revolutions*, and sent Kuhn two letters with extensive criticism. Feyerabend was in particular critical of Kuhn's monistic outlook, which he thought was detrimental to science: if scientists restrict themselves to a single paradigm, then their theories are not properly put to the test (Feyerabend in Hoyningen-Huene 1995, 365). Feyerabend argued that this was the case in quantum physics, where physicists dogmatically refused to consider alternative approaches, such as the one proposed by Bohm. For Feyerabend, this meant that the current theory of quantum mechanics was not properly put to the test, and that its success may be merely due to the exclusion of alternatives (Feyerabend in Hoyningen-Huene 1995, 365).

To summarize the second phase: during the late 1950s and early 1960s, Feyerabend objected to Popper's criticism of quantum mechanics to an important degree because in his view, Popper merely criticized the current theory of quantum mechanics on a philosophical level: Popper objected to the positivist and instrumentalist features of quantum mechanics and argued for a different interpretation of the existing theory. According to Feyerabend, this did not suffice. Like Popper, he found the use of the principle of complementarity and the lack of a realistic description of quantum

[12] On Feyerabend's realism, see e.g. Preston (1997); on its compatibility with pluralism, see Chang (2021).
[13] Del Santo (2019) notes that also Popper had an intellectual relationship with Bohm.
[14] Or at least in Feyerabend's own view, this went beyond Popper; see Bschir (2015) on the compatibility of Feyerabend's pluralism with Popper's critical rationalism.

processes problematic. However, he argued what was needed was not a new interpretation of the existing theory, but the construction of a new theory of quantum physics. By arguing for the need to develop alternative theories of quantum mechanics, Feyerabend, as a philosopher of physics, took on the role of making recommendations to physicists.

The *third phase* in the development of Feyerabend's views on quantum physics started in the mid-1960s, and can be characterized by a gradual weakening of Feyerabend's criticisms of quantum physics and of Bohr, together with an intensification of his criticism of Popper. An important episode in this development was a conversation he had with Carl Friedrich von Weizsäcker in 1965. Feyerabend presented his criticism of dogmatism in quantum physics and his arguments for the development of alternative theories, and Von Weizsäcker responded by giving a detailed historical account of the development of the Copenhagen interpretation. Being confronted with the complexities of the historical development of quantum mechanics, Feyerabend felt that his methodological complaints seemed barren:

> Compared with this rich pattern of facts, principles, explanations, frustrations, new explanations, analogies, predictions, etc., etc., my plea seemed thin and insubstantial. It was well enough argued, but the arguments occurred in outer space, as it were; they had no connection with scientific practice. For the first time I felt, I did not merely think about, the poverty of abstract philosophical reasoning. (Feyerabend, 1995, 141).

That this conversation made a deep impact on Feyerabend can be seen from the fact that at least six different versions of this anecdote occur in Feyerabend's later writings and interviews.[15]

In the following years, Feyerabend seems to have largely given up on his methodological criticisms of quantum mechanics and of Bohr. In a paper published in two parts in 1968 and 1969, Feyerabend defended Bohr against Popper's criticisms. The paper was in particular aimed against Popper's "Quantum Mechanics without 'the Observer'" (1967), and a footnote at the beginning states: "This paper is a belated aftereffect of a discussion with Professor C. F. von Weizsaecker in autumn 1965" (Feyerabend 1968, 309). Feyerabend now described Popper's criticisms of Bohr as naive and historically uninformed. Moreover, he argued that Popper's propensity interpretation of quantum mechanics was in fact similar to Bohr's own views, although more limited, and that it did not solve the issues Popper thought it solved.[16] Feyerabend's relation with Popper had been tense for a while, partly for personal reasons, and this paper formed a final break (Collodel 2016; Del Santo 2022).

Feyerabend now emphatically argued against the idea of Bohr's approach being conservative or dogmatic in any way: Bohr had arrived at his conception of complementarity through a long process, during which he was open to many different ideas and tried hard to find alternative approaches. It is therefore unjust to describe quantum physicists who adhere to Bohr's views as dogmatic, and "it is also somewhat optimistic (...) to think that one can teach them a lesson" (Feyerabend 1969, 92). Feyerabend's emphasis on the complexities of the historical development of quantum physics can be seen as indicative of a 'historical turn' in philosophy of science (see Kuby 2021).

Furthermore, whereas before, Feyerabend had criticized Bohr's correspondence principle and his principle of complementarity as inherently conservative, he now regarded the introduction of these principles as creative methodological steps (Van Strien 2020a). Feyerabend now presented

[15] References can be found in Collodel (2016, footnote 50).
[16] For a critical discussion, see Howard (2012).

complementarity as a valuable insight, based on the idea that in quantum physics, many variables cannot be regarded as properties of an individual system but instead depend on the entire experimental arrangement (Feyerabend 1968). In 1968, he wrote in a letter to Lakatos that Bohr's idea of the indispensability of classical concepts had seemed problematic at first, but had proven its worth:

> This is a property of almost all "philosophical" statements made by Bohr. At first sight they seem to be very general and obviously incorrect, but then they have concrete applications in places where no one would have suspected them. (Feyerabend to Lakatos, in Motterlini ed. 1999, 128)

In *Against Method*, he even presented Bohr as a model of a creative scientist. On the first page of the first chapter, Feyerabend writes that for any methodological rule in science, there are circumstances in which it is advisable to violate it, and in a footnote he mentions Bohr as an example, adding a quote by Léon Rosenfeld: "In speculating about the prospects of some line of investigation, [Bohr] would dismiss the usual consideration of simplicity, elegance or even consistency" (Rosenfeld, quoted in Feyerabend 1975, 24).

Feyerabend's increased appreciation of Bohr was thus closely connected to the idea that it can be fruitful for scientists to bend or altogether abandon methodological rules, and that philosophers should not impose methodological rules on science. In *Against Method*, he notes: "Thus Professor von Weizsäcker has prime responsibility for my change to 'anarchism' - though he was not at all pleased when I told him so" (Feyerabend 1993, 276). According to Feyerabend's 'anarchist' position in philosophy of science, there are no universal methodological rules in science. This does not necessarily mean that there are no methodological rules at all, but all rules have their limits, and for any rule there are cases in which it is legitimate to break it.[17] Moreover, methodology should not be imposed on scientists through a normative philosophy of science: scientists need no help from philosophers. Feyerabend does not completely refrain from normative judgments in *Against Method*: he argues that flexibility in scientific methodology is generally fruitful, and he still thinks that it is generally desirable to develop alternatives to current theories. However, this cannot be made into a hard methodological requirement and cannot be imposed on science from the outside (Feyerabend 1975).

Thus, after critically occupying himself with Bohr for years, in the end Feyerabend completely withdrew his criticism of Bohr. After the late 1960s, Feyerabend wrote very little about quantum mechanics. He was still critical of some tendencies within quantum physics; in fact, he thought that Bohr's views on quantum physics were more sophisticated than those of most quantum physicists and that if anything was needed, it was a return to Bohr. But meanwhile, he essentially gave up on his work on the foundations of quantum mechanics. He may still have had a preference for pluralism in the field of quantum physics and for a realistic account of quantum mechanics, but he did not insist on it any more; it seems that he no longer thought that he as a philosopher of science should tell physicists how to do physics.

It can perhaps be said that Feyerabend's criticism of normative philosophy of science, which forms the core of *Against Method*, was a result of his own failure as a philosopher of physics, as he gradually realized that his own criticisms of quantum physics were unfounded. Certain aspects of quantum mechanics may not seem very rational at first sight, but this does not mean that it is

[17] At least according to most readers. See Shaw (2017) for a detailed discussion of Feyerabend's anarchism.

justified to criticize them from a methodological point of view, and moreover, exactly these aspects may also be regarded as creative and revolutionary.

3. Lakatos

When Feyerabend wrote the paper in which he defended Bohr against Popper's criticisms (Feyerabend 1968), he sent a copy to Lakatos, with whom he had an intense correspondence at the time. Lakatos' reaction, however, was very negative. He thought that Feyerabend's criticism of Popper was rude, unfair, and too personal: "On a dozen pages you defend Bohr's personality against Popper. This is nonsense, and ruins your paper" (Lakatos to Feyerabend, 26 March 1968; in Motterlini ed. 1999, 140). He concluded:

> I am sorry, my dear Paul, for this letter. I don't know what the devil is driving you. This paper is subjective, emotional; its valuable material and very important arguments are submerged in irrelevant and, on many points, distorting anti-Popperianisms. You know how fond I am of you and how much I admire you; so I hope you will not be too angry for my *Odd Remarks.* (Lakatos to Feyerabend, 26 March 1968; in Motterlini ed. 1999, 141)

Lakatos and Feyerabend were close friends, and regarding philosophy of science, they agreed on many points: they both argued that theories cannot be unequivocally proven or falsified by data, and both emphasized the complexities of scientific practice and though that philosophy of science should be historically informed. However, there remained a core difference: whereas Feyerabend was increasingly sceptical of conceptions of rationality and progress in science, for Lakatos rationality and progress remained of central importance. Feyerabend's *Against Method* was written as a starting point for a debate with Lakatos: they planned to publish a book together, with the title *For and Against Method*. In this book, Lakatos would defend rationality and progress in science, against Feyerabend's anarchist views. After Lakatos' unexpected death in 1974, Feyerabend decided to publish his half of the book without Lakatos' reply.

There was a political background to their debate: at one point, Lakatos wrote to Feyerabend that their joint book was to be "based on the assumption that you are a wild anarchist and I a wild reactionary and the whole thing set against the dramatic background of the student revolt and the New Left Uprising…" (Lakatos to Feyerabend, 20 Nov 1970, in Motterlini ed. 1999, 220). During the student protests in 1968, Feyerabend was sympathetic with the students (although not unequivocally), while Lakatos was disturbed by these attacks on order and academic autonomy (Collodel 2015, Martin 2019). For Feyerabend, 'anarchism' denoted something positive: it stood for openness, tolerance and humanism (although he later preferred the term 'dadaism' since it had less heavy connotations). For Lakatos, however, 'anarchism' had connotations of disorder, chaos and violence. Lakatos was also critical of Kuhn's philosophy of science, and thought that scientific revolutions, as Kuhn described them, were "irrational, a matter for mob psychology" (Lakatos 1970, 178, Collodel 2015, Martin 2019). In Lakatos' view, the philosophies of science of both Kuhn and Feyerabend were dangerous in undermining the rationality of science.

Unlike Feyerabend, Popper and Kuhn, Lakatos did not publish extensively on the philosophy or history of quantum mechanics, and in fact he once wrote to Feyerabend that he didn't "know anything about that bloody subject" (Lakatos to Feyerabend, 30 June 1972; in Motterlini ed. 1999, 282). Nevertheless, Lakatos' influential paper *Falsification and the Methodology of Scientific Research Programmes* (1970) contains an extensive case study on Niels Bohr's early work on the atom. Lakatos argues that Bohr's early model of the atom (1913) was built on inconsistent foundations. As Lakatos described the issue in a lecture in 1973:

> The photon jumps from one orbit to the other, but how long does it take to jump? And where is it *in between* jumps? Does the whole thing happen like this? Bohr's theory actually states that there is a certain time when the electron disappears in one point and re-appears in another point. It cannot possibly be anywhere in the middle for any moment of time. What a mad theory! It is inconsistent with our most elementary geometry. It is inconsistent with Maxwell's equations. If Bohr used an ammeter at any point in his experiments, he used Maxwell's theory which was inconsistent with his own! That is sheer madness. (Lakatos, in Motterlini ed. 1999, 82)

(A mistake has to be noted here: in the first line, 'photon' should have been 'electron'.) Lakatos argued that Bohr's atomic theory makes use of Maxwell's electrodynamics, while at the same time contradicting it: whereas Maxwell's electrodynamics predicts that the atom would emit a continuous spectrum, the spectrum of Bohr's atom is discrete. According to Lakatos, Bohr had failed to show exactly under which conditions Maxwell's electrodynamics can be used and when it becomes invalid (Lakatos in Motterlini ed. 1999, p. 82).[18]

Lakatos writes that Popper demanded that scientific theories be consistent, and argues that this demand is too strong: sometimes, it is fruitful to temporarily tolerate inconsistency, and in fact "some of the most important research programmes in the history of science were grafted on to older programmes with which they were blatantly inconsistent" (Lakatos 1970, 142, 143n). Lakatos finds Bohr's decision to continue developing his atomic model despite its inconsistencies methodologically acceptable; in fact, this strategy turned out to be extremely fruitful. However, he writes that the success of this strategy may have led Bohr to fully embrace inconsistency:

> It may well have been the success of his 'grafted programme' which later misled Bohr into believing that such fundamental inconsistencies in research programmes can and should be put up with *in principle,* that they do not present any serious problem and one merely has to get used to them. (Lakatos 1970, 142)

This, according to Lakatos, was a big mistake. He argued that while a theory is still developing, or while a research program is in a progressive phase, inconsistencies may temporarily be tolerated, but eventually they need to be treated as a problem that must be addressed: "If science aims at truth, it must aim at consistency; if it resigns consistency, it resigns truth" (Lakatos 1970, 143). The rational position is therefore to only temporarily allow inconsistency. The "anarchist position", in contrast, is "to extol anarchy in the foundations as a virtue" (Lakatos 1970, 145). According to Lakatos, this was what happened with the introduction of the new theory of quantum mechanics in 1925, and in particular with Bohr's principle of complementarity, which Lakatos regards as a way to establish inconsistency as an integral part of the theoretical framework. In Lakatos' interpretation, the principle of complementarity states that light can sometimes be treated as a wave and sometimes as consisting of particles, even though these descriptions are incompatible. Lakatos regarded the acceptance of this principle as an unprecedented acceptance of irrationality in science:

[18] Kragh (2012, 366) has argued that Bohr did in fact address this issue, and that his atomic model was not internally inconsistent.

> In the new, post-1925 quantum theory the 'anarchist' position became dominant and modern quantum physics, in its 'Copenhagen interpretation', became one of the main standard bearers of philosophical obscurantism. In the new theory Bohr's notorious 'complementarity principle' enthroned [weak] inconsistency as a basic ultimate feature of nature, and merged subjectivist positivism and antilogical dialectic and even ordinary language philosophy into one unholy alliance. After 1925 Bohr and his associates introduced a new and unprecedented lowering of critical standards for scientific theories. This led to a defeat of reason within modern physics and to an anarchist cult of incomprehensible chaos. (Lakatos 1970, 145)

By arguing that the inconsistency in Bohr's early atomic theory was methodologically permissible, Lakatos argued that Popper's demands for rationality in science were too strong: a theory which is still being developed need not immediately be abandoned if it turns out to be inconsistent. By arguing that the acceptance of Bohr's principle of complementarity led to chaos and anarchism, however, Lakatos also implicitly criticized Feyerabend for letting go of criteria of rationality and consistency altogether. It seems that Lakatos had recognized that Feyerabend's praise of Bohr was a starting point for his anarchist philosophy of science, and that he therefore criticized Bohr in order to criticize Feyerabend. Lakatos' evaluation of Bohr's work in quantum physics can be read as a qualified defence of Popper against Feyerabend's criticisms: although Popper's demands for rationality in science were a little too strong, there are demands for rationality to be made of science, and philosophers of science can and should make normative judgments about science. And such judgments can indeed be made against Bohr.

4. Kuhn

Whereas Popper and Feyerabend wrote extensively on the philosophy of quantum physics, there seems to be a general absence of philosophical engagement with quantum mechanics in Kuhn's work. Kuhn hardly mentioned quantum mechanics in his *Structure of Scientific Revolutions* (published in 1962), or in his later writings on revolutions in science, although other authors have often mentioned the development of quantum mechanics as a prime example of a scientific revolution. This absence is remarkable, since Kuhn was a leading philosopher of science, and since for many years he worked intensely on the history of quantum mechanics.[19] In the early 1960s, Kuhn was in charge of the project *Sources for History of Quantum Physics*: together with John Heilbron and Paul Forman, Kuhn collected and archived materials pertaining to the development of quantum physics and conducted detailed interviews with dozens of quantum physicists (te Heesen 2020; 2022). In 1978, Kuhn published the book *Black-Body Theory and the Quantum Discontinuity*, on Max Planck's work on the early quantum theory. These historical projects were aimed at giving a detailed account of the development of quantum mechanics in the first decades of the twentieth century. But despite his detailed knowledge of quantum physics and its history, Kuhn did not participate in the debate on the acceptability of the Copenhagen interpretation or on the desirability of developing alternative interpretations of quantum mechanics. A closer look shows that Kuhn's apparent lack of interest in the philosophy of quantum mechanics was no coincidence: Kuhn thought that philosophical criticism

[19] This observation has also been made by Freire (2016).

was unlikely to contribute to progress in science.[20] In this regard, he agreed with Feyerabend's later views and disagreed with Popper and Lakatos.

Kuhn's writings on paradigm shifts and revolutions in science proved to be very appealing to people who saw themselves as unorthodox thinkers and who developed unconventional ideas in an attempt to break through current paradigms. In fact, one person who read Kuhn this way was the quantum physicist and criticist of the Copenhagen interpretation David Bohm. In Bohm's view, the main merit of Kuhn's *Structure of Scientific Revolutions* was that it could help scientists become aware of the paradigms they worked in, so that they could more easily break free from them (Bohm 1964; Van Strien 2020b). Kuhn himself, however, saw this very differently: he held that progress in science comes about through work on specific, technical puzzles within an existing paradigm, rather than through actively trying to bring about a revolution. Therefore, what enables revolutions is not so much creative thinking and freedom from dogma, but rather focused and specialized work. A certain amount of dogmatism in fact helps to keep scientists focused on concrete puzzles within the current theory, rather than losing themselves in speculations (Kuhn 1963).

This conviction came out clearly in his work on the history of quantum physics. In 1969, Kuhn published a paper together with John Heilbron, in which they gave a detailed account of how Bohr arrived at his famous model of the atom in 1913. Among others, Heilbron and Kuhn describe how during the development of this model, Bohr developed his correspondence principle. The analysis of Heilbron and Kuhn shows how Bohr's revolutionary insights in the structure of the atom arose from specific problems with which he struggled during research stays in Cambridge and Manchester:

> Why did he approach the quantization problem in the particular way he did, one which bore impressive fruits at once and which, a year later, began to revolutionize physics?
> We are persuaded that the answer to these and to similar questions lies not in the general conviction of the need for quantum theory which Bohr drew from his thesis research, but rather in certain *specific* problems with which he busied himself until almost the end of his year in England (Heilbron and Kuhn 1969, 212).

The work on these specific problems led Bohr's research into directions which he himself could not have foreseen (Heilbron and Kuhn 1969, 223). Kuhn later remarked that Bohr's work on the atomic model "illustrates with particular clarity the revolutionary efficacy of normal research puzzles"; "Like much of the research that produces revolutions, Bohr's biggest achievements in 1913 were products, therefore, of a research programme directed to goals very different from those obtained" (Kuhn 1970b, 257). Thus, while working on the atomic model, Bohr did not have a grand vision, but was merely solving technical puzzles, proceeding step by step. According to Kuhn, this is how science usually works; and thanks to Bohr's great ingenuity, his work on specific problems ultimately led to revolutionary developments in physics. Later, in *Black-Body Theory and the Quantum Discontinuity* (first published in 1978), Kuhn made a similar claim about Planck: Planck's quantization of energy in his work on black body radiation in 1900 is generally regarded as the first step in the development of quantum mechanics, but Kuhn argues that at this point, Planck was merely working on a technical

[20] See also Beller (1999, 287), who argues that "close historical links exist between the notion of incommensurable paradigms and the ideology of the Copenhagen dogma". Her claim is based on the fact that Kuhn, in developing his notion of paradigm, was influenced by N. R. Hanson, and that Hanson in turn was influenced by Heisenberg, who argued that quantum mechanics is a 'closed theory'. But although there are indeed similarities between Heisenberg's notion of closed theories and Kuhn's notion of paradigms, there are significant differences as well. Furthermore, there is evidence that Kuhn was not aware of Heisenberg's notion of closed theories when he developed the notion of paradigm (Bokulich 2006; see also Freire 2016).

puzzle and was not aware of taking a revolutionary step (Kuhn 1987; see also Galison 1981; Darrigol 2001).

The fact that Kuhn had little to say about the philosophy of quantum mechanics is therefore itself telling: he did not think that progress in science was made by debating philosophical issues. If revolutions indeed arise from normal science, it is completely fine if physicists do not engage with foundational issues and philosophical concerns. Scattered remarks reveal that Kuhn indeed thought that a philosophically motivated pursuit of alternative interpretations of quantum mechanics was not very promising. For example, in a lecture in 1973, Kuhn argued that Einstein's opposition to quantum mechanics was not motivated by criteria for what makes a good scientific theory, but was merely an expression of distaste; this explained why most quantum physicists had not taken Einstein's opposition seriously (Kuhn 1977, 337).[21] In a discussion of Bohm's alternative interpretation of quantum mechanics in 1974, Kuhn remarked: "Although I may stand to be corrected, it is my impression that one of the greatest difficulties faced by people who are concerned to revise the Bohr interpretation is that none of the problems that emerge from them makes any contact whatsoever with the technical problems that physics has faced in recent years" (Kuhn in Suppe ed. 1974, 411). And in *Criticism and the Growth of Knowledge* (1970), Kuhn recalls a discussion he had with Feyerabend about the merits of Bohm's alternative approach to quantum mechanics, and writes:

> I confessed to Feyerabend that I shared Bohm's discontent but thought his exclusive attention to it almost certain to fail. No one, I suggested, was likely to resolve the paradoxes of the quantum theory until he could relate them to some concrete technical puzzle of current physics (Kuhn 1970b, 246).

The fact that Kuhn writes here that he "shared Bohm's discontent" suggests that also Kuhn may have shared a certain distaste for the Copenhagen interpretation of quantum mechanics, but he thought that it was pointless to dwell on this. No matter how counterintuitive or contradictory a physical theory may seem, physics should be left to the physicists.

It thus seems that Kuhn agreed with the views which Feyerabend expressed from the late 1960s onwards: there is little point in criticizing theories in physics from a philosophical or methodological perspective. There is, however, a significant difference between Feyerabend and Kuhn: whereas Kuhn, in his historical work on Bohr, emphasized that Bohr's achievements in quantum physics were an outcome of his work on technical puzzles and thus of 'normal science', Feyerabend emphasized that Bohr was never a normal scientist in Kuhn's sense. In Feyerabend's view, Bohr did not just work on technical problems, but questioned the foundations of physical theories and asked philosophical questions (Feyerabend 1969, 82). Feyerabend disagreed with Kuhn's view that progress in science is generally made through work on technical puzzles rather than through asking philosophical questions. And for this reason, there remained for Feyerabend a limited role for normative philosophy of science, even after his turn to 'anarchism': despite the fact that he no longer insisted that quantum physicists should develop alternatives to the current theory, he still valued creativity, openness and plurality. For example, he remained critical of Von Neumann's overly formal approach to quantum mechanics ("One cannot say that von Neumann has advanced the quantum theory. But he certainly made the discussion of its basis more long-winded and

[21] And in *The Structure of Scientific Revolutions*, Kuhn compares Einstein's and Bohm's opposition to the standard interpretation of quantum mechanics to the opposition to Newton and Lavoisier in the eighteenth century, suggesting that their opposition will be equally futile in the long run (Kuhn [1962] 1970a, 163).

cumbersome" – Feyerabend 1993, 197). In Kuhn's framework, in contrast, there is much less place for normative criticism of quantum mechanics, or of science in general.

However, Kuhn did argue that it is important for *historians* of science to pay attention to inconsistencies in scientific work. In the period 1961-64, Kuhn conducted interviews with quantum physicists, as part of the project *Sources for the History of Quantum Physics*; Bohr was one of the main supporters of this project (see te Heesen 2020; 2022).[22] Kuhn found these interviews very frustrating, mainly because the physicists he interviewed often lacked a detailed memory of important episodes, and often seemed to misremember their own discoveries. Kuhn and his colleague Heilbron came well-prepared to these interviews, having read relevant papers and other materials, and they found that on this basis they were often able to correct the physicist they were interviewing (te Heesen 2022, 108). Moreover, Kuhn had his own expectations for these interviews: having developed the concepts of crises and paradigm shifts in science, he was interested in identifying periods of confusion and moments of discovery at which suddenly, all pieces fell into place. However, the history of quantum physics turned out to be messier than that (the Heesen 2022). A strong example of Kuhn's frustration and disappointment can be found in his interviews with Bohr in 1962. These interviews took place in five sessions, the last of these on the evening before Bohr's sudden and unexpected death. As part of his preparation, Kuhn read the paper in which Bohr had first proposed his atomic model in 1913. Kuhn noted that the paper included a description of the model but also "a number of phrases incompatible with that model. In particular, Bohr sometimes wrote as though the hydrogen spectrum were emitted by an electron falling into the ground state from outside the atom and strumming all the stationary states that it passed along the way" (Kuhn 1987, 365). It seemed to Kuhn that this was a remainder of somewhat older work on atomic models, out of which the new model had developed. Kuhn therefore concluded that "When [Bohr] wrote the paper announcing his discovery, he had two incompatible models in mind, and he occasionally confused them, mixed the two up" (Kuhn 1987, 367). Kuhn found evidence for his suspicion in an unpublished manuscript by Bohr. During the interviews, however, Bohr denied that his atomic model built on such earlier work.

Reflecting on the issue many years later, Kuhn attributed the unreliability of scientists' memories of their discoveries to the fact that it is often hard for scientists to recognize that "their discoveries were the product of beliefs and theories incompatible with those to which the discoveries themselves gave rise" (Kuhn 1987, 366). Kuhn stressed that such confusions are of great relevance to a historian of science, since they "provide essential clues to the reconstruction of his route to the discovery" (Kuhn 1987, 367).

Thus, also Kuhn identified inconsistencies in Bohr's work: he found that Bohr's early work on quantum physics, especially his atomic model, contained internal inconsistencies and was to some degree confused (this in contrast to Planck, whose early work on quantum physics Kuhn found to be much more consistent). However, whereas Lakatos had argued that inconsistencies can temporarily be tolerated while a new theory is being developed but must at some point be addressed, and whereas Feyerabend had argued that allowing for inconsistencies could actually be fruitful, Kuhn did not comment on them from a philosophical perspective and did not ask whether these inconsistencies were admissible or fruitful. Rather, he only commented on them from the

[22] At some point, the plan was that Feyerabend would also be involved in this project. In the summer of 1961, Feyerabend wrote to Popper: "I was supposed to participate in a very interesting project to reconstruct the history of the quantum theory from interviews (...). However I was dropped from the project as it was discovered that mentioning my name leads to violent reaction in Copenhagen." (Feyerabend to Popper, in Collodel and Oberheim 2020).

perspective of a historian of science, arguing that such inconsistencies can provide important clues for reconstructing the development of a theory. Elsewhere, Kuhn did argue that consistency was a characteristic of a good scientific theory and as a value in theory choice (Kuhn 1977). But it seems that, in contrast to Popper and Lakatos, he did not think that it was the role of a philosopher of science to make normative judgments about this. A philosopher of science has to respect the autonomy of the scientist and should only describe how scientific development takes place.

Conclusions

Despite the strong differences between the accounts of the history and philosophy of quantum mechanics we find in Popper, Feyerabend, Lakatos and Kuhn, they have something in common: according to all four of them, there is only a limited degree to which the development of quantum mechanics was guided by a rational methodology. This comes out especially in their evaluations of the work of Niels Bohr. Popper criticized Bohr for being satisfied with an instrumentalist account, rather than seeking a full understanding of quantum phenomena, and for using the principle of complementarity in an *ad hoc* way to avoid objections. Lakatos argued that Bohr accepted inconsistencies and incoherence in his theories, rather than trying to resolve them. And Feyerabend, in the 1950s and early 1960s, criticized Bohr for constructing quantum theory from a patchwork of elements of classical physics, rather than developing a coherent new theory that would allow for a realist interpretation. When in later years, Feyerabend praised Bohr as a creative and innovative scientist, it was because of Bohr's ability to make steps that seemed methodologically objectionable, but nevertheless turned out to be highly effective.

Kuhn expressed far less judgment of Bohr's methodology (or lack of methodology) than Popper, Feyerabend and Lakatos, but also in Kuhn's view, the degree to which Bohr's work was guided by a rational methodology was limited. When Bohr developed his atomic model, he had no general strategy for the development of a new theory but was merely working on a technical puzzle, proceeding step by step. This, however, is according to Kuhn the normal way in which science develops. What surprised Kuhn, however, when he interviewed Bohr and other quantum physicists, was that many quantum physicists were unable to give an account of how exactly they had developed their theories. Bohr turned out to be unable to recall the exact process through which he had arrived at his atomic model, and was unable to give a rational step-by-step reconstruction of his discovery.

Thus, in different ways, quantum mechanics, with its complex historical development and apparently irrational features, posed a challenge for the prospects of a normative philosophy of science. Popper and Lakatos expressed strong normative judgments, and essentially argued that something had gone fundamentally wrong in one of the most prestigious areas of modern science. Feyerabend increasingly argued that Popper's criticism was naive, and that Popper was insufficiently aware of the complex historical development of quantum mechanics and the way it had been driven by experimental results; for Feyerabend, this was instrumental to his increasing skepticism of normative philosophy of science. Finally, Kuhn's detailed studies of the history of quantum mechanics supported his view that scientific development is driven by work on technical puzzles, and therefore he did not think there was any point in criticizing quantum mechanics from a philosophical point of view (even if the development of quantum mechanics generally might have been less orderly than the revolutions he described in *The Structure of Scientific Revolutions*). Generally, the challenge which quantum mechanics has posed to philosophy of science is that it is hard to argue for methodological principles in science when one of the most prominent and successful scientific theories of the twentieth century might not satisfy them.

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