Pluralism and Anarchism in Quantum Physics

Paul Feyerabend's Writings on Quantum Physics in Relation to his General Philosophy of Science

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Abstract

This paper aims to show that the development of Feyerabend's philosophical ideas in the 1950s and 1960s largely took place in the context of debates on quantum mechanics. In particular, he developed his influential arguments for pluralism in science in discussions with the quantum physicist David Bohm, who had developed an alternative approach to quantum physics which (in Feyerabend's perception) was met with a dogmatic dismissal by some of the leading quantum physicists. I argue that Feyerabend's arguments for theoretical pluralism and for challenging established theories were connected to his objections to the dogmatism and conservatism he observed in quantum physics. However, as Feyerabend gained insight into the physical details and historical complexities which led to the development of quantum mechanics, he gradually became more modest in his criticisms. His writings on quantum mechanics especially engaged with Niels Bohr; initially, he was critical of Bohr's work in quantum mechanics, but in the late 1960s, he completely withdrew his criticism and even praised Bohr as a model scientist. He became convinced that however puzzling quantum mechanics seemed, it was methodologically unobjectionable – and this was crucial for his move towards 'anarchism' in philosophy of science.

Introduction

During the first two decades of his career, Paul Feyerabend worked intensely on the foundations of quantum mechanics. He had studied physics in Vienna; but after getting stuck with his doctoral research he instead wrote a PhD dissertation in philosophy. However, he retained a strong interest in physics, and throughout the 1950s and the early 1960s, the foundations of quantum mechanics were a main area of his research. His writings on quantum physics fill up the fourth volume of his collected papers (*Philosophical Papers 4: Physics and Philosophy*, ed. S. Gattei and J. Agassi, 2015), and about half of the first volume.

Despite the sheer volume of his writings on the foundations of quantum physics, Feyerabend's work in this area seems not to have made a lasting impact. Whereas Jammer in his classic work *The Philosophy of Quantum Mechanics* (1974) makes a couple of references to Feyerabend's contributions to the field, in more recent literature on the foundations of physics one finds scarcely any mention of Feyerabend. Feyerabend's real impact, of course, is in general philosophy of science. In 1970 he published an essay titled "Against Method", followed in 1975 by a book with the same title, with which he became well established, as well as notorious, as a philosopher of science arguing against the idea that science has a fixed methodology.

After that, he wrote little on the philosophy of physics, and rarely returned to his previous work on the foundations of quantum physics. As contributions to the field of foundations of physics, Feyerabend's publications on quantum mechanics had some merit, but they were not ground-breaking. However, they form a major context for the development of Feyerabend's philosophy of science. I aim to show that there are close connections between Feyerabend's writings on quantum physics and his general philosophy of science: his ideas in philosophy of science were partly motivated by concerns about quantum physics, shaped by his developing insight in the intricacies of quantum physics, and influenced in particular by the quantum physicists David Bohm and Niels Bohr.

In particular, I argue that Feyerabend's arguments for pluralism in science were heavily influenced by the quantum physicist David Bohm, who was his colleague in Bristol in 1957 and 1958, and who had proposed an alternative approach to quantum physics and convinced Feyerabend of its feasibility. Feyerabend perceived how the community of quantum physicists dogmatically excluded alternative approaches, including the one of Bohm. Moreover, Feyerabend identified conservative and dogmatic elements in quantum theory itself, in particular in Niels Bohr's account of quantum mechanics. However, in the mid-1960s, Feyerabend came to realize that his criticisms of quantum physics were at least partly misplaced; in particular, he came to realize that Niels Bohr had had very good reasons to develop his ideas in quantum physics the way he did, and was far from having been a conservative scientist. Feyerabend's realization that he had no reason to criticize quantum physics on methodological grounds led him to take a step back and as he himself claimed, this was decisive in his move towards anarchism in philosophy of science.

Despite the continued interest in Feyerabend's philosophy of science, there have only been limited attempts to examine the relevance of this work on quantum physics for the development of his ideas. The most comprehensive account seems to be the entry on Paul Feyerabend in the *Stanford Encyclopedia of Philosophy*, which contains a few paragraphs on Feyerabend's work on quantum mechanics (Preston, 2016). Oberheim, in *Feyerabend's Philosophy* (2006), writes that "Feyerabend's enormous debt to Bohm has gone largely unnoticed in the hefty secondary literature on Feyerabend's philosophy", but otherwise only offers some brief remarks on *Feyerabend's relation to Bohm*; also Preston (2016) and Collodel (2016) mention Bohm's influence on Feyerabend, but provide little detail, and do not mention Bohm's alternative approach to quantum physics.[1] In section 1, I show how Feyerabend developed his arguments for pluralism in science and how this can be placed in the context of debates on David Bohm's alternative approach to quantum physics, starting with a brief sketch of Bohm's approach to quantum physics and his position in the physics community. Section 2 deals with Feyerabend's move to anarchism which went together with a reappraisal of the work of Niels Bohr.

1. Pluralism

1.1 David Bohm's Position at the Margins of Physics

In the 1950s, a number of alternative approaches to quantum physics appeared, which challenged the consensus about the foundations of quantum physics (see Camilleri 2009, Freire 2015). Partly, these criticisms of orthodox quantum physics came from a Marxist or Soviet corner (Camilleri, 2009). These critics often objected to what they saw as the idealism or subjectivism of quantum physics and to the active role attributed to the observer, and argued against the idea that quantum mechanics should only deal with measurement outcomes and not with what happens between measurements.

Most notable among these critics was David Bohm. Regarded as a very promising young physicist in the 1940s, he got into trouble because of his links with communism. The cold war era was difficult for all US academics with communist sympathies, but especially for quantum

[1] While writing this paper I became aware of the fact that Daniel Kuby was working on a paper which also deals with Feyerabend's writings on quantum mechanics, see Kuby (forthcoming).

physicists, whose work could potentially be relevant for atomic weapons. In 1951, Bohm was suspended from his position at Princeton University because of his former links with the *Communist Party*, and realizing that there was no future for him in US academia, he left for Brazil, where he got a position at the University of São Paulo (Freire, 2015, 49).

In 1952, Bohm published an alternative account of quantum physics, which is deterministic and in which particles have a well-defined position at all times (Bohm, 1952). It is known as a 'hidden variable' theory of quantum mechanics in the sense that it adds variables to the existing theory, which make it deterministic. Bohm presented his proposal as a demonstration that an alternative theory of quantum mechanics was possible and as a starting point for reworking the foundations of quantum mechanics, rather than as a finished theory. Bohm's approach gained a couple of supporters, notably Louis De Broglie (who had made a similar proposal already in the 1920s, but had given up on it in the light of criticism he received), Jean-Pierre Vigier, and for a brief period Mario Bunge; but otherwise it did not receive the reception Bohm had hoped for (Freire, 2015). Because it did not lead to new predictions, many working physicists found it irrelevant, and it came to be labeled as philosophy. In the field of philosophy of physics, it proved to attract enduring attention and is nowadays regarded as one of the main options for the interpretation of quantum mechanics. However, Bohm had intended for his theory to be physics, rather than philosophy, and hoped that a further development of his ideas would lead to new predictions and to progress in physics.

While rethinking the foundations of quantum physics in Brazil, Bohm thus found himself not only geographically at the margins of the physics community, but also with respect to his ideas. It has been argued that Bohm's theory was unrightfully ignored (Cushing 1994, Beller 1999), and that this can be explained through the marginalization of those with links to communism during the cold war. Against these claims, Myrvold (2003) has argued that Bohm's theory did get a response from the physics community, and was met with criticisms from Einstein, Pauli and Heisenberg. Besides these criticisms on the level of physics, Bohm's proposal was also criticized on ideological grounds. Freire recounts how the Belgian physicist Léon Rosenfeld, who worked with Bohr in Copenhagen and has been called "Bohr's bulldog", did everything in his power to work against Bohm's interpretation of quantum mechanics:

> Rosenfeld mobilized colleagues wherever he could to take up the fight against the causal interpretation [Bohm's interpretation]. (…). He pushed Frédéric Joliot-Curie—a Nobel prize winner and member of the French Communist Party—to oppose French Marxist critics of complementarity; advised Pauline Yates— Secretary of the "Society for cultural relations between the peoples of the British Commonwealth and the USSR"—to withdraw her translation of a paper by Yakov Ilich Frenkel critical of complementarity from Nature; asked Nature not to publish a paper by Bohm entitled "A causal and continuous interpretation of the quantum theory;" and advised publishers not to translate one of de Broglie's books dedicated to the causal interpretation into English. (Freire, 2015, 37).

Like Bohm, Rosenfeld sympathized with Marxism; but he took himself to be defending the correct Marxist interpretation of quantum mechanics against a new generation of Marxist critics

– this shows that ideological factors in the debates on quantum mechanics could be complicated, as also Freire (2015) has pointed out.[2]

1.2 The Colston Symposium (1957)

In 1957, Bohm came to Bristol for the Ninth Symposium of the Colston Research Society, which brought together physicists and philosophers to talk about quantum physics (on this conference, see Kožnjak, 2018).[3] Feyerabend, who was then appointed in Bristol, was involved in organizing the conference. Other participants included Rosenfeld, Vigier, Ayer, and Popper (who could not attend but had his paper read by Feyerabend). Bohm gave an introduction to his hidden variable theory, which was followed by a talk by Rosenfeld, who was extremely dismissive about Bohm's proposal. Rosenfeld's talk, titled "Misunderstandings about the foundations of quantum theory", starts as follows:

> Recent criticism of the foundations of quantum theory originates from a number of physical and epistemological misconceptions. To point out, with painful explicitness, the most serious of these might be helpful to those whom this criticism seems to have caught unprepared (Rosenfeld, 1957, 41).

Instead of examining the content of Bohm's theory, Rosenfeld gave a general criticism of alternative interpretations of quantum mechanics, arguing against the very notion that there can be something like an 'alternative interpretation': he argues that the interpretation which can be given to a physical theory is generally unique. Rosenfeld admits that there are unsolved problems in fundamental physics; he points out that some critics, such as Bohm, think that a solution will be found through the development of a new theory at the subquantum level, but "Unfortunately for them, all the evidence points with merciless definiteness in the opposite direction" (Rosenfeld, 1957, 44). It is clear, according to Rosenfeld, that further progress will be made through an extension of the current theory of quantum mechanics, rather than through the development of a hidden variable theory such as Bohm's.

Rosenfeld's talk was not well received: it led to a discussion about the role of speculation in physics, and even relatively conservative physicists such as Fierz and Pryce, who were generally not in favor of Bohm's approach, thought that Rosenfeld was not justified in excluding its very possibility. Kožnjak (2018) argues that as a result, Bohm came out well: even if many participants were not convinced of the success of his theory, at least it was admitted by most of the participants that it was an option which could not be dogmatically excluded.[4]

Feyerabend thus witnessed at the Colston Symposium how an alternative theory of quantum mechanics was met with a dogmatic dismissal from a leading physicist, which led to a discussion on the role of speculation and the desirability of alternative approaches in physics. This event

[2] In particular, Rosenfeld regarded Bohr's notion of complementarity as a dialectical notion, which fits within the dialectical materialism of Engels. See Jacobsen (2007).

[3] The contributions to the conference including the discussions were published as Körner (ed.): Observation and Interpretation: A Symposium of Philosophers and Physicists (1957). According to the preface, "Dr. Feyerabend undertook the arduous task of supervising the recording of the discussions from tape-recorder to the printed page."

[4] According to Kožnjak (2018), this meant a turn in the reception of Bohm, whose theory was mostly criticized and ignored before this conference, and started to get some recognition afterwards; however, it was only in the 1970s and 1980s that Bohm's theory really gained popularity.

made an impact on Feyerabend: in later publications, he often quoted Rosenfeld's response to Bohm as an example of a dogmatic attitude in quantum physics (e.g. Feyerabend 1961a, 89, 94; 1962, 145, 1975).

Feyerabend's own position at the conference appears ambivalent. During the discussion he supported the idea that there is room for speculation and for alternative approaches in quantum physics, but expressed doubts about the adequacy of Bohm's proposal. In his own talk, however, Feyerabend argued that, although there are conceptual difficulties with quantum mechanics, these "can be solved within the present theory" (Feyerabend, 1957, 129). The difficulty he points out is the problem of measurement. A problematic issue in quantum mechanics is that in order to connect the wave function which is described by the Schrödinger equation with measurement outcomes, it seems that there has to be something like a wave function collapse, but it is unclear how and why this occurs exactly. Feyerabend argues that the measurement process can be understood in a way which does not require a wave function collapse: the reason why we observe a definite measurement outcome, rather than a superposition, lies in the fact that at the macroscopic level, a definite outcome is indistinguishable from a superposition. Feyerabend concludes that "Apart from leading to the rejection of part of the current interpretation of QM the result of our analysis can also be used for showing the inadequacy of various attacks against the theory itself." (Feyerabend, 1957, 129). It seems that with these inadequate attacks against quantum mechanics, Feyerabend has among others Bohm in mind (Feyerabend, 1957, 124).

Kuby (forthcoming) argues that after giving this talk, Feyerabend found out that his account of measurement was in fact close to that of Niels Bohr, and that this was the start of a reappreciation of Bohr by Feyerabend. However, as we will see, Feyerabend still engaged critically with Bohr's ideas in the following years.

After the Colston conference, Feyerabend also became more appreciative of Bohm's program. A few months after the conference, and after having spent several years in Brazil and Israel, Bohm moved to Bristol to take up a position in the physics department. Thus, Bohm and Feyerabend became colleagues, and regularly spent time discussing physics and philosophy together while they were both in Bristol (Kožnjak, 2018). In the following years, Feyerabend supported Bohm in arguing for the need to develop alternatives to the current theory of quantum mechanics.

1.3 Conservative Elements in Quantum Physics

Before the Colston conference, Feyerabend published a few texts on quantum mechanics, which are heavily influenced by Popper, and in which it is difficult to find a common line.[5] After the conference, however, Feyerabend's position becomes more clear. He identifies certain elements in quantum mechanics as conservative and as an obstacle to progress; in particular, he objects to the idea that measurement outcomes necessarily have to be formulated in terms of classical physics. Heisenberg's uncertainty principle states that particles do not simultaneously have a well-defined position and momentum; similarly, in quantum theory, light appears in certain conditions as waves and in other conditions as particles, but cannot be conceived as waves and as particles simultaneously. From this, one could conclude that the concepts of position, momentum, particle and wave have a limited applicability in the quantum domain, and

[5] These include a short paper on von Neumann's proof against hidden variables (1956) and a paper on determinism (1954). There is also an unpublished text which Feyerabend wrote as a student in Vienna in 1948, titled "The concept of intelligibility in modern physics"; see Kuby (2016).

that one should look for new concepts which describe properties which are well-defined in all circumstances. However, quantum physicists, including Bohr, Heisenberg and von Weizsäcker, argued that the development of such new concepts is impossible, if not in principle then at least in practice, and that it is necessary to use classical concepts to describe quantum phenomena, despite their limitations. Bohr's principle of complementarity expresses the idea that there are concepts and modes of description which are required to describe phenomena, but which cannot be applied simultaneously.

Feyerabend quotes Bohr saying that "however far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms"; Feyerabend describes this as a "defeatist attitude", arguing that Bohr underestimates our capacity to develop new concepts (Feyerabend, 1958a, 152).[6] Feyerabend's attitude is close to Einstein's, who also argued for the need of developing new concepts to deal with the problems in quantum mechanics (Fine, 1986). Bohm too argued that the present situation in fundamental physics required "radically new concepts" (Bohm, 1957, 98). Bohm thought that his own proposal from 1952 did not go far enough in this respect, as the concepts it used were still too classical; during the late 1950s and 1960s, Bohm worked on developing new concepts for quantum physics, based on topological relations (Bohm, 1962).

Feyerabend makes a connection between what he sees as the conceptual conservatism of quantum mechanics and positivism: Feyerabend criticizes positivists for taking observations to be stable elements which cannot be doubted or revised, and he similarly criticizes quantum physicists for taking classical concepts to be unrevisable, despite their limited applicability (Feyerabend, 1958a). In (1958a), he takes quantum mechanics, and especially Niels Bohr's formulation of quantum mechanics, to be the ultimate example of the positivist attitude in modern science.

Feyerabend finds a further conservative element in Bohr's correspondence principle, according to which a number of laws of classical physics are absolutely valid in the quantum domain. In (1960a), Feyerabend writes about the way Bohr and Heisenberg have employed the correspondence principle: "Their main objective was not the construction of a new physical theory about a world that existed independently of measurement and observation; their main objective was rather the construction of a logical machinery for the utilisation of those parts of classical physics which could still be said to lead to correct predictions."

In several texts, Feyerabend contrasts Bohr's way of doing science with Einstein's: whereas Einstein develops a new theory by making bold new assumptions and inventing new concepts, Bohr basically builds up a new theory by holding on to the laws and concepts of the old theory as much as possible, using the correspondence principle to establish classical laws in the quantum domain, and restricting the validity of classical concepts without replacing them (Feyerabend, 1965a, 218; and see Oberheim 2016 on Einstein's influence on Feyerabend). Feyerabend emphasizes that Bohr's approach has been fruitful to some degree; however, it will necessarily lead to stagnation sooner or later: "*Only the invention of a new set of ideas which boldly oppose appearances and common belief, and which attempt to explain both in a deeper way, can then lead to further progress and to the continuation of a rational argument*" (1958b, 72).

[6] He also quotes von Weizsäcker, who writes that "Every actual experiment we know is described with the help of classical terms and we do not know how to do it differently." Feyerabend: "The obvious reply is, of course: 'Too bad; try again!'" (Feyerabend, 1962a, 155).

1.4 Interpretations versus Theories

We have seen how both Bohm and Feyerabend argued for rethinking the foundations of quantum physics and developing new concepts, which would result in a genuinely new theory of quantum physics. However, the account of quantum physics which Bohm had proposed in (1952) was framed as an alternative interpretation of the theory of quantum mechanics, rather than as an alternative theory of quantum mechanics.

This interpretation of quantum mechanics was contrasted with the 'Copenhagen interpretation'. The latter term was introduced by Heisenberg (1955), in response to the emergence of alternative interpretations of quantum mechanics such as Bohm's (Camilleri, 2009). The '*Copenhagen interpretation*', according to Heisenberg, was the interpretation for which there was a broad consensus among quantum physicists.[7]

The philosopher of science Norwood Russell Hanson argued against the idea that the issue was that of different interpretations of the same theory. Hanson argued that one cannot get rid of the puzzling features of quantum mechanics, such as wave-particle duality and Heisenberg's uncertainty relation, through a mere re-interpretation of the theory:

> One cannot maintain a quantum-theoretic position and still aspire for the day that the difficulties of the uncertainty relations will have been overcome. This would be like playing chess and yet hoping for the day when the difficulties of possessing but one king will have been overcome. (Hanson, 1958, 149).

To do without duality and the uncertainty relations would mean to do without quantum mechanics altogether and to develop a completely new theory, rather than merely a different interpretation. And to develop such a completely new theory to account for quantum phenomena would not be an easy task. Like Feyerabend, Hanson argues that what we observe is not independent of the theories we work with. *Hanson's Patterns of Discovery* (1958) is mainly known for his account of the theory-ladenness of observation: our theories influence what we observe and what kind of experiments we can do. This means that the development of a new theory goes together with the development of a new way of observing and experimenting: to develop a theory means to find a 'pattern of explanation' within which observations fit and make sense. The simultaneous changes in theoretical framework and observation which Hanson describes are close to the "paradigm shifts" which Kuhn described a few years later in his *Structure of Scientific Revolutions* (1962) – indeed, Kuhn acknowledged an indebtedness to Hanson in the introduction of this book.

Hanson's argument, that quantum mechanics cannot be modified without being changed completely, was meant as an argument against alternative accounts of quantum mechanics such as Bohm's. However, Bohm was in fact in agreement with the idea that an alternative account of quantum mechanics would require fundamental change. Bohm again and again emphasized that his (1952) paper was merely a starting point and a more thorough rethinking of the foundations of quantum physics was needed. *According to Pinch* (1977), what Bohm in fact had offered in

[7] Howard (2004) and Camilleri (2009) have argued that Heisenberg made it seem like there was more consensus than there actually was; in fact, there was a large variety in the ways in which physicists interpreted quantum mechanics. Howard (2004) argues that Feyerabend was one of the philosophers of science who contributed to the establishment of the myth of the Copenhagen interpretation. This is not right – on the contrary, Feyerabend was well aware of the diversity of interpretations among quantum physicists, and in (1962, 147) remarks that "Quite obviously, the fictitious unity conveyed by the term 'Copenhagen interpretation' must be given up."

his (1952) paper was a new interpretation of quantum mechanics plus an outline of a research program that goes beyond mere interpretation.

In *Causality and Chance in Modern Physics* (1957), Bohm argues that there is currently a crisis in fundamental physics: current theories predict infinite values for certain physical properties, and the so-called elementary particles are falling apart into ever more particles (Bohm, 1957, 121–22). He notes that "modern physicists feel that the present crisis in physics will be resolved by revisiting the details of the general kinds of probabilistic theories that are now current". Bohm, however, thinks that this is not enough; he argues that revolutionary changes in the theory and concepts of quantum mechanics are needed.

Feyerabend also accepted the idea that in order to deal with puzzling features of quantum mechanics such as uncertainty and complementarity, a mere re-interpretation of the theory was not enough: "*The issue of the foundations of the quantum theory can therefore be solved only by the construction of a new theory (…) it cannot be solved by alternative interpretations of the present theory*" (Feyerabend, 1962, 114).

In the early 1960s, Feyerabend increasingly distanced himself from his earlier claim that the puzzling features of quantum mechanics were the result of a positivist attitude: it was not positivist philosophy but physical reasons which led physicists to develop quantum mechanics the way they did (1962, 121, 162). In particular, Feyerabend argues that quantum uncertainty is a direct result of a few postulates, which are supported by experiment and therefore generally accepted as scientific facts, namely the quantum postulate, the duality of light and matter, and conservation of energy and momentum (1962; 1964). This means that doing away with uncertainty would require going against established experimental results. Feyerabend thus agrees with Hanson that developing an alternative theory of quantum physics would not be an easy task. However, he emphasizes that none of this shows that alternative theories are impossible or cannot be expected to be successful. He makes a comparison with objections made against Copernicus:

> …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. Any such attempt therefore amounts to introducing unsupported conjectures in the face of fact and well-supported physical laws. This is the main objection which is used today against the theories of Bohm, Vigier, de Broglie and others. It is similar to the objections which were raised, at the time of Galileo, against the idea that Copernicus should be understood realistically. (Feyerabend, 1964).

Feyerabend argues that also von Neumann's proof against hidden variables does not show that an alternative, deterministic theory of quantum mechanics is impossible. Von Neumann had shown in 1932 that given the postulates of quantum mechanics, one can prove that quantum mechanics cannot be expanded with hidden variables to make it deterministic. His proof was frequently used against hidden variable theories of quantum mechanics, including Bohm's – however, it was not easy to see where Bohm's theory went wrong, and for many years there was a confusion about whether and how exactly it could escape von Neumann's proof, and whether von Neumann's proof was generally valid (Pinch, 1977). In (1962, 167), Feyerabend argues that since the proof uses the postulates of quantum mechanics as premises, it can be circumvented by coming up with an alternative theory of quantum mechanics which does not share these postulates.[8]

Thus, Hanson, Bohm and Feyerabend were in fact in agreement that developing a satisfactory theory of quantum physics without features such as uncertainty and complementarity would require something akin to a paradigm shift – it would require the development of new concepts as well as a reinterpretation of experimental results. The main issue on which they differed regarded the feasibility and desirability of developing such a new theory.

In "*Five Cautions for the Copenhagen Interpretation's Critics*" (1959a), Hanson warns critics of the *Copenhagen interpretation*, "*particularly Bohm and Feyerabend*", that currently, giving up on the Copenhagen interpretation of quantum mechanics is not an option, because no satisfactory alternative exists at the moment. As he states in (1959b), "*There is as yet no working alternative to the Copenhagen interpretation. Ask your nearest synchrotron operator.*" He admits that quantum theory in its current form is not entirely without problems, but despite these problems, quantum mechanics is a theory which is effective, physicists can work with it, and this is not something to give up lightly: "*physicists, being reasonable men, will not abandon the imperfect tools in their hands for the ingenious and optimistic expectations in philosophers' minds*" (Hanson, 1959a).

In a response to Hanson, Feyerabend argues that Bohm's proposal is a viable option and that it should not be shoved aside just because it had not yet been developed in full detail:

> Being well versed in the history of the sciences and in the considerations which play a role in the process of discovery, Professor Hanson surely knows that elaborate theories are preceded by more or less general considerations which sometimes are inconsistent with the prevalent philosophy. But should Copernicus have abandoned the idea of Aristarch just because it was not yet worked out in as great detail as was the geocentric idea? By no means; he perceived, and justly so, that his idea was a possible one, and he had no reason to assume that the finished theory would be worse than Ptolemy's system. (Feyerabend, 1961b).

In (1962, 165), Feyerabend argues that developing alternatives takes time, and therefore it cannot be expected that someone will come up with a fully developed alternative theory of quantum physics at once. Feyerabend furthermore points out that "the point of view of Bohm and Vigier has already been developed in much greater detail than commonly supposed by most of the opponents" and therefore cannot be disregarded as an alternative (Feyerabend, 1962, 160f).

Whereas Hanson emphasizes that one should not give up a working theory without strong motivation, Feyerabend implores physicists to be open to alternatives. He stresses that "*future research need not (and should not) be intimidated by the restrictions which some high priests of complementarity want to impose upon it*" (1962, 103) and that "*Now, as ever, the future development of physics is a completely open matter*" (1962, 116).

[8] Feyerabend had struggled with Von Neumann's proof for many years, and wavered on its validity. Already in (1954), Feyerabend argued that von Neumann's proof would not apply to a fundamentally new theory. In (1956), he claims to give a general refutation of the proof. However, Feyerabend came to believe that his refutation had been based on a mistake, and in (1957) he states that von Neumann's proof is valid and rules out hidden variable theories. Then, in (1962a, 127, 166), he credits Bohm with having refuted von Neumann's proof. Dieks (2017) points out that von Neumann himself never claimed to have shown that hidden variable theories are impossible: von Neumann pointed out that his proof relies on the formalism of quantum mechanics, so if you are prepared to change the formalism, you may circumvent the proof.

1.5 Why Alternatives?

We have seen that Feyerabend argued for the possibility and desirability of developing alternative accounts of quantum physics. What exactly was his motivation for this? As we have seen in section 1.3, Feyerabend objected to the principle of complementarity and the correspondence principle because he saw these as ways to hold on to the concepts and laws of classical mechanics instead of developing a new theory on new foundations. But are there any intrinsic reasons why such a new theory would be preferable?

Feyerabend never seems to have been very concerned about determinism: in (1954), he even argued that determinism is a philosophical prejudice.[9] He considered Bohr's answer to the Einstein-Podolsky-Rosen paradox to be satisfactory (1958b, 1962), and also the measurement problem was not a central issue for Feyerabend: as we saw in section 1.2, in 1957 he argued that measurement in quantum mechanics could be dealt with without requiring a new theory, and although in later publications he did regard measurement as problematic, it never seems to have been his main concern.

Feyerabend does often emphasize the need for a realist account of quantum mechanics, especially in his earlier publications. This means that quantum physicists should not restrict themselves to giving predictions of measurement outcomes, but should aim to provide an account of what happens between measurements. This also means that theories should be formulated in terms of concepts which are universally applicable, and not only applicable under specific experimental conditions (1958b). Feyerabend ultimately defended his preference for realism on the basis of methodological arguments: a realist approach motivates the search for theories and explanations, since it motivates attempts to go beyond observation and measurement outcomes and to entertain hypotheses about unobserved processes, and in this way it leads to progress.

In the early 1960s, Feyerabend developed further arguments for the development of alternative theories of quantum mechanics; again, these arguments were methodological. Feyerabend argued that developing alternatives to existing theories is in general crucial for the scientific enterprise. He argues that developing alternatives to an existing theory can uncover problems with the theory which would otherwise have remained undiscovered. This means that if you don't consider alternatives to an established theory, you eliminate potentially refuting facts (Feyerabend, 1963). His main example is Brownian motion, which is the random motion of particles in a fluid, which can be observed through a microscope (Feyerabend, 1993, 262). It was first described by Robert Brown in 1827, but was not given any particular significance until Einstein showed in 1905 that it is possible to account for Brownian motion through the kinetic theory of heat, and on this basis it can be shown that Brownian motion involves a violation of the second law of thermodynamics on small scales. Thus, with the help of the kinetic theory (alternative theory), the observable phenomenon of Brownian motion was reinterpreted and used to refute a result from thermodynamics (established theory).

Interestingly, Feyerabend has attributed his arguments for the importance of developing alternative theories in order to test established theories to David Bohm, along with Karl Popper, and specifically attributes the example of Brownian motion to Bohm (Feyerabend, 1993, 262). In (1965b), Feyerabend writes:

[9] Probably under influence of Popper, he defined determinism in terms of predictability. With this definition, Bohm's account does not come out as deterministic and even classical mechanics is not deterministic.

> My general outlook derives from the work of David Bohm and K. R. Popper and from my discussions with both. The idea that a theoretical pluralism should be the basis of knowledge can be found both in the dialectical philosophy of Bohm and in Popper's critical rationalism. However, it seems to me that it is only within the framework of the latter that it can be developed without undue restrictions. (Feyerabend, 1965b, 153).

There is a relation between Feyerabend's pluralism and Popper's falsificationism: one should use all possible means to relentlessly test theories, even those which are already established, and this includes trying to develop alternative accounts. In later editions of Against Method, however, Feyerabend writes that Bohm helped him to go beyond Popper's falsificationism: "*That falsification is not a solution became very clear in discussions with David Bohm who gave a Hegelian account of the relation between theories, their evidence, and their successors*" (Feyerabend, 1993, 262).

Several authors have argued that although Bohm was very influential for Feyerabend, this influence started quite late, in the late 1960s (Collodel, 2016) or early 1970s (Preston, 2016). This is based on the fact that in a review of a book by Bohm which Feyerabend published in (1960), Feyerabend was critical about what he described as a Hegelian structure of reality adopted by Bohm, which includes the idea that nature is infinitely complex, that all theories have a restricted domain of application and that no scientific result can ever be taken as more than approximately valid. It may be true that these ideas only started to play a role for Feyerabend later on; but the argument that developing alternatives to a theory is needed in order to test this theory also works independently of this broader metaphysical picture. This is an argument which Feyerabend developed in discussions with Bohm, in the context of discussions on the foundations of quantum mechanics.

The argument that a theory can be tested by developing alternatives to it can also be found in a paper by Bohm and Bub from 1966. Here, Bohm and Bub argue that "*there is a very important methodological justification for the consideration of hidden variable theories, even those which are not necessarily seriously regarded as 'right' ones*": as long as we stay within the framework of quantum mechanics, the basic postulates of quantum mechanics are unlikely to be exposed to tests, because only those certain questions can be meaningfully asked and only those experiments can be considered which do not go outside this framework. By developing hidden variable theories, it may become possible to test the basic postulates of quantum mechanics experimentally, and possibly falsify them (Bohm and Bub, 1966).

In (1963, 80), Feyerabend proposes a "*positive methodology*" based on pluralism in the sciences. Feyerabend puts the need for developing alternatives in strong terms: he argues that if no alternatives to a theory are considered, the success of the theory will be entirely due to the exclusion of alternatives, and the theory is therefore not much better than a myth. This would imply that the defense of the Copenhagen interpretation as the only possible account of quantum mechanics has the potential of reducing quantum mechanics to a myth. Feyerabend draws this conclusion in a letter to Kuhn from 1961, in which he comments on a draft of Kuhn's Structure of Scientific Revolutions:

> Your insistence upon faithfulness to one and only one paradigm is bound to result in the elimination of otherwise very important tests and it is bound in this way to

> reduce the empirical content of the paradigm you want to be accepted. It may well be – and Bohm and Vigier are definitely of this opinion – that the situation is the same in the present quantum theory. The 'orthodox' refuse considering alternatives and their argument is that the present point of view has not yet encountered anomalies which would necessitate reconsideration of it in its entirety. Bohm points out that the limitations of the present point of view will become evident only if one has first introduced an alternative and shown that it is preferable. Hence if the absence of limitations is taken as a reason for not considering alternatives, then trouble will never be discovered, simply because it could be discovered only with the help of alternatives. This, then, would make the present quantum theory a wonderful metaphysics. (Feyerabend, in Hoyningen-Huene, 1995, 365).

The arguments which Feyerabend gives for the development of alternative theories of quantum physics, such as the one proposed by Bohm, seem to be essentially methodological: in all areas of science, developing alternatives is essential for testing established theories, and this also holds for quantum mechanics. However, Feyerabend's arguments still seem to be motivated by the idea that there is something wrong with quantum mechanics specifically, and that quantum physicists are particularly dogmatic in excluding alternatives to their theories.

Thus, to a considerable degree, Feyerabend developed his arguments for pluralism in science in the context of debates on quantum mechanics, in dialogue with Bohm and motivated by what he perceived as the dogmatism of quantum physicists and their negative reactions to Bohm's alternative account of quantum mechanics. Feyerabend then applied his arguments for pluralism to science as a whole; the fact that he developed these arguments within this specific context may be a reason to re-evaluate how far this pluralism can be extended to other scientific disciplines.

2. Anarchism

In the mid-1960s, Feyerabend changed his views on quantum mechanics; as he recalled later, this was due to a discussion he had with Carl Friedrich von Weizsäcker in 1965. As he recalls in his autobiography:

> In the late sixties or early seventies I gave a public talk in Hamburg, with von Weizsäcker in the chair. In the seminar that followed I repeated my reasons for basing research on sets of conflicting theories. Both confirmation and content, I said, depend on a confrontation with alternatives (hidden variable theories in the case of quantum mechanics). Von Weizsäcker responded with a detailed account of the problems that had arisen; he showed how these problems had been attacked and solved and to what extent the new predictions had been confirmed. 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).

He brought up the same anecdote in an interview he gave in 1994, two weeks before his death:

> You see, I was once [in 1965] in Hamburg with Carl Friedrich von Weizsäcker, who invited me to give a talk there. At that time I was still a methodology freak. At that time I believed that it made sense to argue for certain procedures in science. And my arguments were excellent. But von Weizsäcker gave a historical account of the rise of quantum theory and this was much richer and more rewarding and I realized that what I was talking about was just a dream. Just as Ceaucescu wanted to have order in his country, so he tore down the little houses and built up his concrete monsters. When von Weizsäcker started describing the development of quantum theory he was just pointing out the little houses, because there were so many little steps being made. Niels Bohr said: 'When you do research you cannot be tied down by any rule, not even the rule of noncontradiction. One must have complete freedom'. So, as he explained that to me, I recognized that my arguments were excellent but that excellent arguments don't count when you want to deal with something which is as rich as nature, or other human beings. (Feyerabend, in Jung 2000, 162).

The anecdote also appears in the 1993 edition of Against Method, where he adds: "*Thus Professor von Weizsäcker has prime responsibility for my change to 'anarchism' – though he was not at all pleased when I told him so in 1977*" (Feyerabend, 1993, 262).

Feyerabend's anarchism, which he first expressed in his essay '*Against Method: Outline of an Anarchistic Theory of Knowledge*' (1970) and a few years later in a book format in *Against Method* (1975), has been interpreted in different ways, but most commentators agree that the point is not that science has no methodology at all: rather than rejecting rules and standards altogether, Feyerabend emphasizes that all rules and standards have their limits. There are no universal methodological rules: for each methodological principle in science there are circumstances in which it may be broken. "*...there is only one principle that can be defended under all circumstances and in all stages of human development. It is the principle: anything goes*" (Feyerabend, 1975, 28). Secondly, Feyerabend emphasizes that methodology should not be imposed on science: scientists do not need '*help*' from philosophers of science who prescribe them a methodology. There is still normativity in Against Method: scientists should not be too strict in their methodologies and should be open to breaking the rules, and Feyerabend still seems to think that considering alternatives is generally fruitful, but this is not made into a hard methodological rule.[10]

Feyerabend's move towards anarchism has often been understood as a break with Popper's critical rationalism (see e.g. Preston 1997, 79, Collodel 2016). Feyerabend had been strongly influenced by Popper, who had been his supervisor at the London School of Economics in 1952–53. Feyerabend's earlier pluralism can be understood as Popperian in so far as it includes a strong normative emphasis on testing: Feyerabend emphasized that developing alternatives to established theories is essential for testing these theories. But during the 1960s Feyerabend gradually broke with Popper. Collodel (2016) describes the personal factors which played a role in this break: it involved hurt feelings and resentment on both sides, and Feyerabend's philosophical disagreements with Popper were often fueled by personal factors.

However, Collodel also notes that the break cannot be fully explained through personal factors, and argues that partly through his studies in the history of science, Feyerabend gradually became convinced that actual science was too complex to satisfy Popper's general methodological rules. Also Preston (1977, p. 178) writes that Feyerabend's move towards anarchism took

[10] On ways to understand Feyerabend's anarchism, see Shaw (2017).

place "*probably as the result of studies in the history of science.*" Oberheim (2006) also notes that during the late 1960s and 1970s, Feyerabend became increasingly critical of Popper and attributed increasing significance to the history of science, but argues against a discontinuous break, emphasizing the continuities in Feyerabend's thought.

However, when Feyerabend recalls the development of his own thought correctly, it was a discussion with Von Weizsäcker about the history of quantum mechanics which pushed him towards anarchism. How to understand this claim?

I argue that Feyerabend's move to anarchism can indeed be understood as an outcome of his reflections on quantum physics. But although Feyerabend presents his discussion with von Weizsäcker as a turning point in his thought, it was actually part of a development which already started earlier, and the change in Feyerabend's thought was thus more gradual.[11] From the early 1960s, Feyerabend's criticisms of quantum physics gradually became milder.

Whereas Feyerabend argued in (1958a) that quantum mechanics was based on an ill-founded positivism, in the early 1960s, he argued that this type of criticism could not be maintained, and that there were good physical arguments for why quantum mechanics had developed the way it did. Therefore, criticizing quantum mechanics on purely philosophical grounds does not suffice. In (1962), Feyerabend thus defended orthodox quantum mechanics against what he saw as superficial philosophical criticisms. However, he then went on to argue that however strong the physical arguments for the present theory of quantum mechanics were, they could not exclude the possibility of developing an alternative theory of quantum physics on new foundations. His main target here was Popper, who, according to Feyerabend, gave unjustified criticisms of quantum mechanics from a purely philosophical perspective. Thus, Feyerabend's break with Popper was directly connected to the development of his ideas on quantum physics.

The change in Feyerabend's thought on quantum mechanics can be seen most clearly in his writings on Niels Bohr. Already from the early 1950s, Feyerabend had been particularly interested in Bohr's account of quantum mechanics: he thought that Bohr gave the strongest account of what could count as 'orthodox' quantum mechanics, and for this reason chose Bohr as the focus of his criticism. Whereas in his earlier critiques of positivism (e.g. 1958a), Feyerabend took Bohr as a core example of a positivist scientist, and regarded Bohr's account of quantum mechanics as the product of a misguided positivist philosophy, from the early 1960s Feyerabend distanced himself from these views. Until the mid-1960s, Feyerabend still objected to what he saw as conservative elements in Bohr's thought; in particular, he regarded Bohr's correspondence principle as a means to construct a new theory by holding on to an older theory as much as possible, rather than developing new foundations. He thought that quantum physicists, including Bohr, had not looked hard enough for alternative accounts and new foundations for quantum physics.

However, in "*On a Recent Critique of Complementarity*" (published in two parts in 1968 and 1969),[12] Feyerabend seems to take back all his former criticisms of Bohr and even praises Bohr as a creative physicist, and the correspondence principle as a creative methodological step. A footnote at the beginning of the paper states: "*This paper is a belated aftereffect of a discussion with Professor C. F. von Weizsaecker in autumn 1965.*" The main aim of the paper is to defend Bohr against criticisms by Popper, who had criticized quantum mechanics for being positivistic, and

[11] See also Kuby (forthcoming), who argues that Feyerabend's reappreciation of Bohr already started in 1957.

[12] Collodel (2016) mentions that Feyerabend already finished a version of the paper in 1967, which was rejected for publication.

objected to the fact that quantum mechanics only offers predictions of measurement outcomes and no description of the actual processes that take place at the quantum level.[13] Feyerabend argues that Popper's criticism is naive and uninformed about the history of quantum mechanics. Popper does not take account of the fact that Bohr tried to give an account of atomic processes which goes beyond measurement, but was refuted at every attempt:

> Every paper of Bohr's emphasizes that so far an instrument of prediction is all one can have and that this shortcoming is due to the absence of unrefuted hypotheses about the nature of atomic processes. (...) It is true that Bohr eventually arrived at the position that "the whole purpose of the formalism of the quantum theory is to derive expectations for observations obtained under given experimental conditions" – but this was the result of a series of refutations and discoveries which seemed to show that considerations of "nature" (and the word here does not indicate essentialistic aberrations!) had been removed very far indeed. Now it is of course Professor Popper's privilege to disregard such refutations and to continue believing in the correctness of his own microphilosophy. But it is somewhat unjust to describe those who took the refutations seriously as philosophical dogmatists who never realized that there was an issue, and it is also somewhat optimistic, under such circumstances, to think that one can teach them a lesson. (Feyerabend, 1969, 91–92).

We can now see how Feyerabend's engagement with quantum physics pushed him in the direction of anarchism. First, steps in the development of a scientific theory which seem methodologically objectionable, such as Bohr's employment of the correspondence principle and Bohr's violation of the methodological principle that scientists should try to go beyond mere observation and give a realist account of phenomena, can also be seen as creative methodological steps. This supports the idea that there are no universal methodological rules, and that flexibility in methodology is beneficial for scientific progress. On the very first page of the first chapter of Against Method, Feyerabend praises Bohr for his creative methodology. Feyerabend writes that for any methodological rule in science, there are circumstances in which it is advisable to violate it, and adds in a footnote:

> One of the few thinkers to understand this feature of the development of knowledge was Niels Bohr: "...he would never try to outline any finished picture, but would patiently go through all the phases of the development of a problem, starting from some apparent paradox, and gradually leading to its elucidation. In fact, he never regarded achieved results in any other light than as starting points for further exploration. In speculating about the prospects of some line of investigation, he would dismiss the usual consideration of simplicity, elegance or even consistency..." (Feyerabend, 1975, 24).[14]

[13] In particular, Popper argued that certain puzzling features of quantum physics, including uncertainty and complementarity, rest on a misinterpretation of the concept of probability. To deal with these issues he proposed a propensity interpretation of probability. Feyerabend argues that although Popper attacks Bohr's views on quantum mechanics, Popper's account of probability is actually very similar or even equal to that of Bohr, and argues that this account of probability in itself does not suffice to do away with the puzzling features of quantum mechanics.

[14] See also (1993, 129): "I favor Niels Bohr's 'this is not crazy enough'."

Ironically, the quote is from Léon Rosenfeld, Bohm's most aggressive and dogmatic opponent.

Secondly, Feyerabend gradually came to believe that his earlier criticisms of Bohr and of quantum mechanics in general had been ill-founded. He already recognized in (1962) that one cannot simply criticize quantum mechanics on philosophical grounds without engaging with the physical details; now he recognized that, in light of the complex historical development of quantum mechanics, it is too easy to blame physicists for not having looked hard enough for alternative theories with which some of the puzzling features of quantum mechanics could have been avoided.

The fact that Feyerabend's increasing awareness of the complex history of quantum mechanics made him retract some of his earlier normative pronouncements points to a '*historical turn*' in Feyerabend's thought. Feyerabend is usually counted, along with Kuhn and Lakatos, among the philosophers responsible for a historical turn in philosophy of science, as they gave serious attention to history (Bird 2008, Nickles 2017). The relevance Feyerabend attributed to the history of science can be seen in particular from the central role played by his studies of Galileo in Against Method. However, Kuby (forthcoming) argues that Feyerabend was initially critical of this '*historical turn*', as he thought that philosophy of science should essentially be normative, and it was in the context of quantum physics that Feyerabend first became aware of the pertinence of history. Also this development in Feyerabend's thought can thus be linked to his work on quantum physics.

We can still ask how these '*anarchist*' views relate to Feyerabend's earlier arguments for pluralism in quantum physics. Feyerabend always remained a proponent of pluralism in the sciences, and one could say that whereas in the 1960s he mainly argued for a plurality of theories and theoretical approaches, with Against Method he extended his pluralism to methodology, arguing for many scientific methodologies rather than one. So one would expect that he would still be in favor of alternative accounts of quantum physics. But he no longer seems to argue that there is something specifically wrong with quantum physics which would make the development of alternatives particularly urgent in this case. If there is anything wrong with current quantum physics, it is that the younger generation of physicists tends to defend quantum mechanics with ill-founded positivist arguments, and that Bohr's originals ideas are often misunderstood: "*many contemporary physicists of the younger generation take complementarity for granted without examining it and perhaps even without understanding it*" (Feyerabend, 1969, 103). Feyerabend argues that the remedy for this is to go back to Bohr, and see exactly how Bohr has been led to his idea of complementarity.

The question whether Feyerabend still thinks that quantum physicists should try to develop alternatives to the existing theory of quantum physics is answered in the abstract of his (1968/69) paper:

> Considering that [Bohr's] views are more detailed, better adapted to the facts of the microdomain than any existing alternative it follows that fundamental discussion must first return to them. Their uniqueness is not asserted, however. Here the author still maintains that a hundred shabby flowers are preferable to a single blossom, however exquisite. But a hundred shabby flowers plus an exquisite blossom are more desirable still. (Feyerabend, 1968).

This indicates that while developing alternative theories of quantum physics is still worthwhile, it would be better to return to Bohr's original account of quantum mechanics and start from there than to start from completely new foundations. This is a break with Feyerabend's previous writings on quantum physics, in which he emphasized that progress in quantum physics may be made by developing new foundations.

However, it seems that Feyerabend did not fully give up on his support for Bohm: he ends the paper by stating that "*the first step in our attempt to achieve progress in microphysics will have to be a return to Bohr*", and adds in a footnote: "*This first step has been made by Bohr, Vigier, and others who are taking into account the features which Popper, Bunge, and others neglect*" (Feyerabend, 1969). It seems that this is a typo and should have been "*BOHM, Vigier, and others*". This would indicate that he now thought of Bohm's work as involving a return to Bohr, rather than as an alternative based on altogether new foundations.[15]

It is important to note that this paper on Bohr was Feyerabend's last major publication on the foundations of quantum mechanics. He does discuss Bohr's views in some of his later writings, but after the late 1960s, Feyerabend no longer seems to have had the ambition to contribute to the field of foundations of quantum physics. After having worked on the foundations of quantum mechanics for many years, Feyerabend no longer felt he could contribute anything to the field, as he no longer thought that methodological criticism of physics was fruitful. In essence, it was exactly this development which led to Against Method; the end of Feyerabend's career as a philosopher of physics thus meant the start of his big success in general philosophy of science.

Conclusions

Feyerabend's writings on quantum physics reveal how he developed his notions of pluralism and anarchism in the context of debates on quantum mechanics, especially concerning the feasibility and desirability of alternative approaches to quantum mechanics. His earlier writings (until the mid-1960s) on the role of alternative theories in science were motivated by what he saw as the dogmatism of the quantum physics community, and called for openness to alternative accounts of quantum mechanics, especially the one developed by Bohm. However, Feyerabend's criticisms of quantum physics gradually became milder. Initially, he objected to certain puzzling features of quantum mechanics, such as the fact that it is not possible to describe what happens between measurement and the use of complementary pairs of concepts (e.g. in wave-particle duality), and he motivated these objections through methodological concerns. But as he learned more about the physical details and historical complexities of quantum mechanics, he gradually became more modest in his criticisms, and in the end withdrew them altogether. This was crucial in his move towards anarchism, which includes the idea that methodology cannot be imposed on science. With some simplification, the ultimate outcome of Feyerabend's extensive writings on quantum physics may be formulated as follows: if even a theory as puzzling as quantum mechanics is methodologically unobjectionable, then anything goes.

[15] It has to be noted that by the late 1960s, Bohm had abandoned the interpretation of quantum mechanics he had proposed in 1952, and was working on a different approach; he only returned to his original theory much later (Freire, 2015). Moreover, Bohm's attitude towards Bohr was a complicated one: Bohm was critical of some aspects of Bohr's account of quantum mechanics but highly appreciative of others.

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