Feyerabend's Reevaluation of Scientific Practice: Quantum Mechanics, Realism and Niels Bohr

Daniel Kuby

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Abstract

The aim of this paper is to give an account of the change in Feyerabend's (meta)philosophy that made him abandon methodological monism and embrace methodological pluralism. In this paper I offer an explanation in terms of a simple model of 'change of belief through evidence'. My main claim is that the evidence triggering this belief revision can be identified in Feyerabend's technical work in the interpretation of quantum mechanics (ca. 1957-1964), in particular his reevaluation of Bohr's contribution to it. This highlights an under-appreciated part of Feyerabend's early work and makes it central to an understanding of the dynamics in his overall philosophy of science.

Status

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The archival documents cited in this paper are being published (many for the first time) in Feyerabend (n.d.). I thank the editors Matteo Collodel and Eric Oberheim for making this material available to me before publication. I hope this archival treasure will soon be accessible to the general public.

1 Introduction

In this paper I offer a specific interpretation of how Feyerabend came from a Popperian critique of the Copenhagen interpretation to a detailed reevaluation of Niels Bohr's idea of complementarity. Engaging with this chapter of Feyerabend's intellectual *Werdegang* is not only an interesting exercise in Feyerabendian exegesis; an explanation of this change of mind in a very narrow domain — or so it seems — gives the backdrop for Feyerabend's thoroughgoing turn from methodological monism to methodological pluralism, for which he would became known to a wider audience with his publication of *Against Method* (Feyerabend 1975).

In his early philosophy and until the mid-1960s, Feyerabend positioned himself decidedly against the wave in philosophy of science that would eventually be labeled as its "historical turn". This is ironic in light of the fact that Feyerabend is remembered to this day as a proponent of the turn. Though his later adherence to the turn is not disputed, it is also recognized that his previous philosophical stance had a normative urgency towards the sciences that his later philosophy would lack. Indeed, I propose to recognize this normative stance as a kind of *philosophical prescriptivism*, according to which philosophy of science qua general methodology has standing to make prescriptive claims vis-a-vis the sciences. This view grew particularly strong in the early 1960s, in that *only* general methodology has standing to set up methodological rules. Still a methodological monist, Feyerabend defended specific methodological rules, most importantly the demand to interpret our best scientific theories realistically, as means to realize the core epistemic value of testability. The justification of testability as a core scientific value, however, was based in a purely axiological decision concerning the aims of science (cf. Feyerabend 1962a).

The first observation we can make is that Feyerabend's adherence to the "historical turn" coincides with an abandonment, indeed a rejection of this philosophical prescriptivism (even though his later philosophy would still have an appreciation for normative claims). A more specific question about Feyerabend's adherence to the turn can, then, be asked in terms of how Feyerabend came to abandon his prescriptivism and which factors made him abandon it. One avenue of research is to relate the dynamics of Feyerabend's philosophical views to a broader context, by noting the political and social turmoil that coincides with his changing views—most notably, the effects of desegregation around US-American universities (starting in 1954) and the Free Speech Movement at UC Berkeley (starting in 1964), where Feyerabend had been a tenured Professor since 1959. Surely no explanation of Feyerabend can be complete without putting this picture at the center stage. In this paper, however, I will put forward an explanation of Feyerabend's philosophical journey in a complementary fashion, in an attempt to resist the narrative of an "anarchic overturn" between an "early" and a "later" Feyerabend. In fact, I will offer a very standard explanation in terms of a simple model of "change of belief through evidence".

My main claim is that the evidence he was exposed to came through his engagement in (the history of) quantum mechanics, in particular with a reevaluation of Neils Bohr's contribution to it. I contend that Feyerabend's prescriptivism was first confronted with a serious problem in the specific context of his methodological arguments for realism vis-à-vis justified scientific practice in quantum mechanics. A crucial feature of Feyerabend's methodological argument for a realistic interpretation of scientific theories is its generality. The argument is universal in scope, such that the methodological demand obtains "for all scientific theories". It poses no conditions on its application on the specifics of a theory, in part because the argument applies to scientific theories as reconstructed in the statement view, which completely abstracts from the specifics of any given scientific theory. It was the universal scope of the argument that was slowly but steadily put into question. Throughout the 1960s Feyerabend recognized for himself specific instances of arguably scientific theories in which differing demands were legitimate because they 'made sense scientifically', putting a dent into Feyerabend's top-down methodological argument scheme: for a specific research situation, we arrive at contrasting demands whether we look at it from an general-methodological or from a contextual-scientific point of view. Such was the situation of Bohr's interpretation of quantum mechanics. This is what I want to call *Feyerabend's dilemma*:

(1) According to his philosophical prescriptivism, the compelling reasons for a specific scientific behavior are axiological.
(2) There's a specific class of scientific behavior that Feyerabend finds compelling, for reasons that are independent from axiology.

As long as the methodological conducts derived from (1) and (2) are compatible, no problem arises. It might not even be possible to conceptually separate (2) from (1), i.e. to be forced to recognize that the behavior in (2) is not chiefly dependent on axiological decisions. The problem arises if the methodological behaviors derived from (1) and (2) are incompatible.

This contrast became more and more strident, until Feyerabend was forced to give up the universality of the methodological argument, which initiated a cascade of consequences extending to the very core of his conception of what philosophy of science is about. He had discovered and came to acknowledge the existence of a scientifically justified, theory-specific notion of ontological interpretation which stood in contrast to a theory-independent, axiologically justified notion of ontological interpretation. I contend that the source responsible for bringing Feyerabend face-to-face with this evidence was his physical and philosophical interest in quantum mechanics.[1]

2 Feyerabend and quantum mechanics

Feyerabend is often thought of as a philosopher working in general philosophy of science—and, since his (1970), openly advocating its demise. It might therefore come as a surprise that Feyerabend started out as a philosopher of quantum mechanics. His early scholarly production deals overwhelmingly with problems in microphysics (1954a; 1956; 1957a; 1957c; 1957b; 1958b; 1958c; 1958a; 1960a; 1960b; 1960c; 1961; 1962b; 1963). In earlier as well as later papers quantum mechanics continued to surface as historical casuistry.

Feyerabend got to work on quantum mechanics since the late 1940s as a trained physicist with an interest in philosophy of physics. His earliest extant paper ([1948] 2016), written as an undergraduate student, deals with the concept of intelligibility in microphysics (cf. Kuby 2016). We don't know the exact topic of his attempted dissertation thesis, which was to deal with problem in classical electrodynamics—but we know that he abandoned it in order to work on the philosophical problem of basic statements (Feyerabend 1951). We have only scarce evidence on how Feyerabend came to concentrate on quantum mechanics. It coincides temporally with his stay stay abroad at the LSE in 1952 with Karl Popper and took off from thereon (cf. Feyerabend 1995, 92). Feyerabend came to work on a large num-

[1] I want to stress that Feyerabend's change of mind cannot be explained by his exposure to this evidence alone. He had to be receptive to this evidence in the first place. This receptiveness is rooted in specific characteristics of his overall metaphilosophical conception, in particular the demand that methodological rules should be actually realizable, which had become almost ineffective in his philosophical prescriptivism and slowly regained importance. For a reconstruction of Feyerabend's metaphilosophy as "Decision-Based Epistemology", see Kuby (in preparation).

ber of topics: indeterminism in the microphysical domain; the limits of the Von Neumann no-go theorem; the quantum theory of measurement; quantum mechanical formalisms, in particular quantum logic; ontological interpretations of quantum mechanics; and alternative theories to quantum mechanics. Feyerabend's philosophical allegiance to Popper consolidated around that time.[2] On his return to Vienna, his first research project in 1954 included an analysis of "the role of the ergodic hypothesis within classical statistics" as part of the larger topic "The function of hypotheses in science", on which Feyerabend remarked in a letter to Popper: "the title already mirrors your influence" (Feyerabend to Popper, March 12, 1954, KP 294.16-15). At first, likely due to this intellectual bond, Feyerabend came to adopt Popper's specific criticism in the philosophy of quantum mechanics, chastising its main proponents as giving in to an unwanted and unwarranted positivist position (cf. Feyerabend [1954a] 2015, 34; Feyerabend [1954b] 2015, 12), which allegedly had been built on the scientific consensus at the Fifth Solvay Conference in 1927 and was ascribed to the Copenhagen-Göttingen school of Niels Bohr, Werner Heisenberg, Max Born and Wolfgang Pauli. Feyerabend repeatedly invoked Popper's sweeping picture of a capitulation of physics to vicious philosophy in his early papers, too:

> Today the view of physical science founded by Osiander, Cardinal Bellarmino, and Bishop Berkeley, has won the battle without another shot being fired. Without any further debate over the philosophical issue, without producing any new argument, the instrumentalist view (as I shall call it) has become an accepted dogma. It may well now be called the 'official view' of physical theory since it is accepted by most of our leading theorists of physics (although neither by Einstein nor by Schrödinger). And it has become part of the current teaching of physics (Popper 1956, 360).[3]

[2]It has been suggested to me that the term "Basissätze" (instead of "Protokollsätze") in the title of Feyerabend's dissertation (1951) is already a clear reference to a Popperian framework, thereby predating this allegiance to an earlier time. A thorough reading of the dissertation thesis, however, does not substantiate this claim.

[3]Popper's capitulation picture is important because it licensed the use of the Copernican Revolution as

But then something changed. In his (1958b), for the first time Feyerabend timidly used a footnote to exonerate the founder of the Copenhagen school from the charge of deceivingly stating the Copenhagen interpretation as a necessary consequence of the formalism of quantum mechanics.[4] In private correspondence we can predate a change of mind about Bohr already to an earlier time. In a short post scriptum to a letter to Popper, Feyerabend notes that "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 to Popper, 21 July 1957, KP 294.13-26).

What happened in 1957? Feyerabend's engagement with the original literature of the 'first quantum revolution' coincides with the Ninth Symposium of the Colston Research Society, hosted by the University of Bristol, where Feyerabend held his first appointment as lecturer in philosophy. The conference was seminal in challenging the scientific orthodoxy after World War II and helped create a climate in which philosophers of science considered foundational issues to be open questions again, creating a platform for challenges to (and defenses of) scientific orthodoxy—though these issues would be accepted back into physics only with Bell (1964; see Kožnjak 2017).

Though the evidence is sparse, discussions during the conference also alerted Feyerabend to the fact that his knowledge of Bohr's own views and arguments were deficient. In particular, he was made aware that his contribution to the conference, on the topic of quantum measurement theory, was not a counterpoint to Bohr's view, as Feyerabend framed it, but much along lines that Bohr had previously indicated.[5] This gave Feyerabend pause—not

a foil to discuss the interpretation of quantum mechanics and one can track Feyerabend's position by the way in which he handled Popper's thesis both regarding the Copenhagen interpretation and the Copernican Revolution.

[4] A charge leveled in that context against Von Neumann's presentation of the theory in von Neumann ([1932] 1955); Feyerabend (1958b, 346, fn 1) exonerates Bohr in one succinct remark without further comments: "It ought to be mentioned that Bohr himself did not commit this mistake."

[5] An account of this incident and of Feyerabend's contribution at the conference about the quantum measurement problem will be detailed in Kuby (in preparation).

in his philosophical struggle against positivism and subjectivism in quantum theory, but in associating *Bohr's* position with positivism and subjectivism. Given Bohr's key role in the development of quantum theory, Feyerabend developed a genuine interest into his ideas, which would have deep repercussions at the very core of Feyerabend's metaphilosophy. But first, this new perspective on Bohr's work ignited a series of detailed examinations of Bohr's contribution to quantum mechanics, recognizing his unique perspective (Feyerabend 1958a; Feyerabend 1961; Feyerabend 1962b; Feyerabend 1968; Feyerabend 1969).

3 The role of physical argument: Feyerabend reevaluates Bohr

Feyerabend's motivation to learn about the original development of quantum mechanics was greatly enhanced by his participation in the Colston Symposium. Access to the original development of quantum mechanics meant access to the dynamics of scientific reasoning behind its establishment: *Did complementarity earn its place in microphysics? If so, how?* Nothing less was the motivation of Feyerabend's interest in the early history of quantum mechanics. Popper had taught him, mostly on general methodological grounds, that complementarity had not earned its place. Feyerabend first followed Popper, but then came in contact with historical protagonists and the original literature and he started to think differently. We can see this progressive awareness in an almost chronological ordering of his papers:

In Feyerabend (1958a), Feyerabend tries

> to show that [Bohr's point of view] is consistent, that it has led to important results in physics and that it therefore cannot be easily dismissed. It will also turn out that this point of view is closely related to the position of positivism: the issue between the classical model of explanation and complementarity is essentially an instance of the age-old issue between positivism and realism

(Feyerabend 1958a, 80–81).[6]

Firstly, he recognized Bohr's complementarity as a proposal for a new model of scientific explanation. This model diverges from the classical model of explanation in how it treats the two groups of experimental facts that firmly established the wave-particle duality of light. Two theories can completely explain each group of facts, yet they are mutually exclusive. While the classical model of explanation regards "the existence of two non-exhaustive and complementary descriptions [...] to be an historical accident, an unsatisfactory intermediate stage of scientific development" to be hopefully solved by the "search for a new conceptual scheme", the new model accepts the duality and changes the very requirements of what a scientific explanation is. The classical model demands that "such a new theory [...] must be empirically adequate, i.e. it must contain the facts [about duality] as approximately valid under mutually exclusive conditions [...] [a]nd it must be universal, i.e. it must be of a form which allows us to say what light *is* rather than what light appears to be under various conditions" (1958a, 78). In this sense it is "closely connected with the position of realism" (1958a, 79). Bohr instead does not regard duality as "a deplorable consequence of the absence of a satisfactory theory, but a fundamental feature of the macroscopic level. For him the existence of this feature indicates that we have to revise [...] the classical *ideal of explanation*" (1958a, 79). This new ideal of explanation, expressed in the principle of complementarity, "does not consist in relating facts to a universal theory, but in their incorporation into a predictive scheme none of whose concepts is universally applicable" (1958a, 87–88). It is therefore an abdication of realism in that it not only gives up universal applicability of quantum-mechanical concepts as a condition of explanation (a), but, by replacing traditional theories with the notion of "natural generalization of classical physics", by following the correspondence rule, also of any future quantum theory (b) (1958a, 90).

Is this abdication justified? Feyerabend maintains that this new model of explanation is successful in the case of quantum mechanics and he gives a first run-down of how com-

[6] Yet Bohr's work stands in contrast to other physicists of the "Copenhagen school [...]. To them Popper's remark [about the capitulation of physics, see above] applies" [Feyerabend1958Complementarity, 80].

plementarity fits well with the physical layout of quantum theory. In this sense (a) can be said to be justified, though with important limitations. But Feyerabend argues vehemently against (b): the new model doesn't make the classical ideal of explanation which it tries to replace neither impossible nor obsolete; more importantly, its application to the very possibility of future physics would lead to a complete "stagnation" of physics (1958a, 103–4).

Next, Feyerabend went into a detailed examination of the source literature in order to appreciate Bohr's interpretation not just as a philosophical preconception that happened to be physically successful, but as an outcome of scientific research, a point he argued at length in his papers "Niels Bohr's interpretation of the quantum theory" (1961) and "Problems of microphysics" (1962b) (which incorporated and expanded Feyerabend (1961)), and reaffirmed much later in his long two-part paper "On a recent critique of complementarity" (1968; 1969), prompted by Mario Bunge (1967) which Feyerabend deplored. It was in "Problems of microphysics" (1962b) that for the first time he put the (mostly qualitative) *physical reasoning* at the center stage: Bohr's "point of view can stand upon its own feet and does not need any support from philosophy" (1962b, 292). He lays out the the main aim of his paper as follows:

> I shall try to give a *purely physical explanation* of the main ideas behind the Copenhagen Interpretation. It will turn out that these ideas and the *physical arguments* leading up to them are much more plausible than the vague speculations which were later used in order to make them acceptable (1962b, 195, emphasis added).

I want to draw attention to the emergence of the notion of "physical arguments" as a crucial step in Feyerabend's reevaluation of Bohr's contribution to quantum mechanics. To sustain the Copenhagen interpretation, says Feyerabend, "much better arguments are available, arguments which are directly derived from physical practice" (1962b, 194). In contrast to scientific practice as seen through a sociological lens, which Feyerabend thought cannot deliver an evaluation of reasons, scientific practice as seen through the dynamics of physical arguments can deliver reasons for understanding and evaluating scientific decisions. And, most important of all, the class of "physical arguments" gives us an instantiation of step (2) in the challenge to axiological justification, i.e. a specific class of reasons for scientific behavior that are not dependent upon general axiology.

Without going into too much detail, we can say that Feyerabend's account of the physical grounding of complementarity works out Bohr's postulate of the *indeterminateness of state descriptions*, of which he takes complementarity to be an abstract generalization. He tracks in detail the introduction of the assumption as a "physical hypothesis" (he underlines time and again its objective character) to make the gradual interaction between two physical systems consistent with the quantum postulate: "during the interaction between two systems A and B the dynamical states of both A and B cease to be well defined so that it becomes *meaningless* (rather than *false*) to ascribe a definite energy to either of them" (1962b, 196). His point, which he makes time and again, is to bring out the objective character of this "simple and ingenious physical hypothesis", which is only based on the quantum postulate and duality (together with the individual conservation of energy and momentum, cf. Feyerabend 1962b, 204): indeterminateness is introduced by Bohr not on the basis of a verificationist theory of meaning (though he admits it has been used in this connection by many other physicists and philosophers), but on the basis of "well-known classical examples of terms which are meaningfully applied only if certain physical conditions are first satisfied and which become inapplicable and therefore meaningless as soon as the conditions cease to hold."[7]

Secondly, he calls out the misconception that Bohr's hypothesis makes reference to knowledge or observability; as a physical hypothesis, it "excludes" the *existence* of "these intermediate states themselves" (1962b, 197). Having dispelled misreadings of the indetermi-

[7]In this connection he uses the example of the term 'scratchability' (Mohs scale of mineral hardness) "which is applicable to rigid bodies only and which loses its significance as soon as the bodies start melting" (Feyerabend 1962b, 197). (This example is repeatedly used to suggest a non-philosophical reading, see 1958c, 51; 1960c, 323; 1961, 373; 1964, 294; 1969, 94, 95.)

nateness assumption *as proposed by Bohr*, he proceeds to explain how the hypothesis stood up successfully against two alternatives (Planck and Schrödinger: psi-waves as complete and well-defined states; statistical ensemble interpretation of the psi-function) to explain the physical and conceptual problems on the table (1962b, 203–7). He shows how Bohr himself tried to come up with alternatives, only to be thrown back to indeterminedness as the only viable solution. As a preliminary conclusion he points out that it is "impossible to derive Bohr's hypothesis [...] from the formalism of the wave mechanics plus the Born interpretation" (1962b, 207), and, since the "qualitative considerations" behind the hypothesis "are needed in addition to Born's interpretations if a full understanding of the theory [i.e. the formalism of wave mechanics] is to be achieved", Born's hypothesis of the indeterminateness of state descriptions is an irreducible, i.e. independent *and* necessary part of quantum mechanics (1962b, 208).

Feyerabend (1962b, 208–20) then proceeds to explain how Bohr's second hypothesis, the assumption of the *relational character of quantum-mechanical states*, was proposed as a response to EPR and how it is intimately connected to the first hypothesis insofar as it grew out of the same qualitative considerations that brought about indeterminateness. (In this sense it is not an ad hoc move to accommodate the "very surprising case discussed by EPR" (1962b, 218).) Instead of assuming, as EPR had done, that "what we determine when all interference has been eliminated is a *property* of the system investigated", Bohr maintains that "all state descriptions of quantum mechanical systems are *relations* between the systems and measuring devices in action and are therefore dependent upon the existence of other systems suitable for carrying out the measurement" (1962b, 217). This is the second hypothesis. Feyerabend goes on to show "how this second basic postulate of Bohr's point of view makes indefiniteness of state descriptions compatible with EPR. For while a property cannot be changed except by *interference* with the system that possessed that property, a relation can be changed without such interference" (1962b, 217).

Finally, Feyerabend introduces Bohr's principle of complementarity. Where the indeterminateness hypothesis referred to description in terms of classical concepts and asserted that description in terms of these concepts must be made "more liberal" if agreement with experiment is to be obtained", this principle"expresses in more general terms this restriction, forced upon by experiment, in the handling of the classical concepts" (1962b, 222). To show in which way our interpretation of Feyerabend that the complementarity principle 'had earned its place in microphysics' holds, we have to carefully disentangle Feyerabend's discussion of complementarity. The complementarity principle is not identical with the indefiniteness hypothesis, it is a philosophical extension. Empirically, it assumes (beside the conservation laws) duality and the quantum of action, but it also introduces "some further premises which are neither empirical, nor mathematical, and which may therefore be properly called 'metaphysical' " (1962b, 222). Because Feyerabend uses the rest of the paper to severely criticize these further assumptions from a methodological point of view, it may seem that he does *reject* complementarity after all. But this is not correct. We have to distinguish, firstly, Feyerabend's recognition that complementarity (i.e. including these metaphysical assumptions) has 'earned its place in microphysics' in that its application in microphysics was successful in advancing its development: the existence of quantum mechanics vindicates the abstract principle of complementarity. Feyerabend is very clear on this point when he discusses how the more "liberal attitude towards" classical concepts had been guided by the correspondence rule to obtain "rational [or natural] generalization of the classical mode of description":

> [I]i is very important to realize that a "rational generalization" [. . .] does not admit of a realistic interpretation of any of its terms. The classical terms cannot be interpreted in a realistic manner as their application is restricted to a description of experimental results. The remaining terms cannot be interpreted realistically either as they have been introduced for the explicit purpose of enabling the physicist to handle the classical terms properly. The instrumentalism of the quantum theory is therefore *not a philosophical manoeuvre that has been willfully superimposed upon a theory which would have looked much better when interpreted in a realistic fashion. It is a demand for the-*

> *ory construction which was imposed from the beginning and in accordance with which, part of the quantum theory was actually obtained* (1962b, 265, fn 62, emphasis in the original).

But complementarity, as a general principle of Bohr's Copenhagen interpretation, claims validity beyond quantum mechanics. While Feyerabend even agrees that its success may warrant complementarity as a useful heuristic principle for future development, he understands Bohr to make a much stronger claim: any future microphysical theory that will not obey complementarity

> will either be internally inconsistent, or inconsistent with some very important experimental results. [Many followers of the "orthodox" point of view] therefore not only suggest an interpretation of the known results in terms of indefinite state descriptions. They also suggest that this interpretation *be retained forever* and that it be the foundation of any future theory at the microlevel. It is at this point that we shall have to part company. I am prepared to defend the Copenhagen Interpretation as a physical hypothesis and I am also prepared to admit that it is superior to a host of alternative interpretations. […] But […] any argument that wants to establish this interpretation more firmly is doomed to failure" (1962b, 201).

Thus Feyerabend rejects the complementarity principle insofar as it implies that its success in the construction of quantum mechanics warrants its extension to any future microphysical theory, i.e. its imposition as a *necessary restriction* on the future development of physics. Additionally, he rejects complementarity on general methodological grounds, greatly expanding on his arguments concerning complementarity as a new model of explanation already discussed above (cf. 1958a, 90) and to be further discussed below.

More generally, behind this reevaluation of Bohr's arguments lies Feyerabend's consistent aim to understand Bohr's thinking as original contributions to quantum theory, not at all assimilable to other members of the Copenhagen school. In this respect we must call into question Howard's claim that Feyerabend was among "the most important enablers of the myth" (Howard 2004, 677) of a unitary Copenhagen interpretation allegedly reproducing Bohr's view; if Feyerabend was part of the group who most "contributed to the promotion of this invention for polemical or rhetorical purposes" (Howard 2004, 670), this claim should be limited to his pre-1958 papers. Since then, he was an active myth-buster.

4 Physical arguments and ontological problems

Let us dwell a little longer on the new and remarkable outcome of Feyerabend's investigation: Bohr's interpretation, in particular the principle of complementarity, is justified by physical arguments grounded in Bohr's research activity.[8] This, however, seems at odds with the contention, stemming from Feyerabend's philosophical prescriptivism, that the interpretation of quantum theory is a philosophical problem to be decided on purely methodological grounds. Has the interpretation of quantum theory suddenly become a physical question? To understand how Feyerabend understood this state of affairs, it is instructive to see how he conceptualized the interplay between philosophical and physical problems in the domain of quantum mechanics.

In a letter to Herbert Feigl from 28 June 1957, a few months after the Colston Symposium, Feyerabend sketched a framework for the discussion of quantum mechanics for an upcoming conference to be held at the Minnesota Center for the Philosophy of Science. Firstly, he drew the distinction between the "analysis of quantum mechanics in its present form and interpretation" and "suggestions as to the possible form of a future theory of microscopic phenomena" (Feyerabend to Feigl, 28 June 1957, HF 02-133-02/1). This was by no means an obvious distinction at the time. Let us remember: The completeness of quantum

[8] The interpretation may have roots in Bohr's philosophical ideas, Feyerabend is not disputing this. Feyerabend's point is that the interpretation earns its place in physics not because of Bohr's philosophical background, but due to Bohr's physical arguments.

theory was assumed; and, as Leon Rosenfeld did, the very expression "interpretation" was questioned because the term suggested that other interpretations were possible. Secondly, he distinguished between "syntax" and "semantics" of elementary quantum theory, i.e. the chosen mathematical formalism and the rules which are "necessary and sufficient for transforming the formalism into a full-fledged physical theory". Notably, questions about the proper interpretation of quantum mechanics are not semantical questions, but take place on a third level, "ontology":

> [W]hen discussing the question which is the proper interpretation of quantum mechanics, a wave-interpretation, a particle interpretation or e.g. the Copenhagen-interpretation, physicists and philosophers are not concerned with semantical problems, i.e. they are not concerned with the problem how an uninterpreted formalism ought to be connected "with reality". The question "particles or waves?" rather presupposes that the symbols of quantum mechanics have already been given a certain meaning, i.e. it presupposes that all syntactical and semantical problems have been settled in a satisfactory way. What is to be interpreted is not a formalism, but a physical theory. This is the reason why it seems to be advisable to distinguish between two different kinds of interpretation of a physical theory, between its *semantical interpretation* and its *ontological interpretation*. The Born-interpretation is a semantical interpretation of the formalism of quantum-mechanics. The Copenhagen-interpretation (or the wave-interpretation or the particle-interpretation) is an ontological interpretation of quantum theory. Problems connected with ontological interpretations I shall call ontological problems. This distinction between syntactical problems, semantic problems, ontological problems, seems to be very useful, especially in the case of quantum mechanics. (Feyerabend to Feigl, 28 June 1957, HF 02-133-02/1).

Among the ontological interpretations of quantum theory Feyerabend's lists Einstein's— "as defended by Popper"; Bohm's first (1952) interpretation; similarity between quantum-mechanics and the theory of diffusion; and Schrödinger's interpretation (Feyerabend to Feigl, p. 3). As alternative theories to quantum mechanics, with their own possible sets of ontological interpretations, Feyerabend mentions "Bohm's new papers."[9]

This very specific organization of the discussion has a number of consequences relative to how the levels are related to each other. With regard to the ontological level, Feyerabend is very clear that there can be a relation of implication between this level and the syntactic plus semantic level:

> Traditional philosophers have tried to solve ontological (or metaphysical) problems such as e.g. the problem of causality (or the narrower problem of determinism) by speculation on the basis of (sometimes very scarce) experience. The existence of very general scientific theories enables the philosopher to change the methods of ontological research. For it may turn out that a theorem of one of those theories either contradicts, or implies a statement of metaphysics. Such a theorem may be called "ontologically relevant". *And a hypothesis as to the ontologically relevant theorems of a given theory may be called an ontological interpretation of that theory.* Ontological interpretations in this sense can be *tested* by comparing their consequences with theorems of the theory so interpreted. It is not always easy to carry out such a test. This is the reason why there is still so much *argument* about the (ontological) interpretation of quantum-mechanics. On the other hand [ontological interpretations] may be introduced with the help of certain arguments which do not at all refer to theorems of the theory so interpreted and which strongly resemble the ontological arguments of traditional metaphysics. Most of Bohr's arguments are of this kind, although

[9] Presumably Bohm (1953), Bohm and Vigier (1954), and possibly Bohm and Aharonov (1957); Feyerabend might also have Bohm's Colston Symposium paper (Bohm 1957a) in mind; see also Bohm (1957b), which, though not a paper, Feyerabend was already acquainted with in April 1957 at the latest (cf. Feyerabend to Popper, 1 April 1957, KP 294.19).

> his results are shown to be correct by many theorems of the theory itself. (Feyerabend to Feigl, 28 June 1957, HF 02-133-02/1, emphasis in the original)

Note that this is the state of Feyerabend's assessment in 1957, i.e. this framework is in place *before* Feyerabend's reevaluation of Bohr. This tells us two things: Firstly, the contention that general physical theories are relevant to ontological problems that were once in the domain of 'pure metaphysics' (e.g. the issue of determinism) precedes the reevaluation of Bohr. (Indeed, this contention is one of the most pristine expressions of Feyerabend's understanding of the rapprochement of science of philosophy and it was already clearly expressed in Feyerabend ([1954b] 2015).) Secondly, he still thought that the "Copenhagen interpretation", including Bohr's complementarity principle, was not an ontological interpretation derived from the underlying physical theory, but—following Popper's assessment—was posited on the basis of a dubious philosophical presupposition. Feyerabend recognized that it fit the underlying physical theory, but it was almost as if it matched it 'by chance'. This coincides with the outline of Feyerabend (1958a) that we gave above, in particular point (a).

Feyerabend's reevaluation of Bohr's ontological interpretation as being grounded in physical argument (1962b) does not overthrow this framework in principle. Indeed, such a move is envisaged in the framework and corresponds to the possibility that "a theorem of one of those theories either contradicts, or implies a statement of metaphysics". Feyerabend's claim that Bohr's "point of view can stand upon its own feet and does not need any support from philosophy" is equipollent to the claim that it is an "ontologically relevant" consequence of the physical theory, not a philosophical argument "resembling ontological arguments of traditional metaphysics", as previously thought. And yet—behind this coherent interplay of philosophy and science lurks a possibility that Feyerabend had not readily envisaged. When Feyerabend thought of ontological problems, he thought of issues like determinism. But *the upshot of his reevaluation of Bohr is that another kind of issue turns out to be an "ontologically relevant" consequence of physical theory: the issue* *of realism itself.* This result cannot be overstated: under the assumption that realism and instrumentalism are mutually excluding positions, we have a situation in which

(1) according to general axiology, there are compelling reasons to interpret scientific theories realistically;
(2) the instrumentalist interpretation of a specific theory, namely quantum mechanics, is compelling because of physical arguments grounded in the development of the theory.

In other words, we are now confronted with an explicit case of Feyerabend's dilemma.

How did Feyerabend deal with the dilemma? Quite ingeniously, he used his theoretical pluralism to give an answer: While a given physical theory (its syntax and semantics) may indeed give stringent indications as to the right solution to an ontological problem, including the realism issue, a methodological demand can always be put forward to develop genuinely alternative theories that may imply different solutions to ontological problems. The discovery that a solution to the issue of realism can be itself part of the ontological consequences of a scientific theory was, at the beginning, not only seen as unproblematic for his methodological conception of realism; it was used by Feyerabend as a vindication of the importance of theoretical pluralism for the progress of science. If quantum mechanics forces an instrumentalist interpretation, the importance of genuine alternatives to quantum mechanics that allow for a realistic interpretation becomes a central problem for the future of microphysics.

5 The limits of quantum theory and hidden variable alternatives

At first, Feyerabend made the contextual reevaluation of complementarity fit with general methodology, in particular with his methodological argument for realism. If not only elementary quantum mechanics but also Bohr's interpretation had earned their right to stay, it was not the interpretation that was in need of being changed:

> If I am correct in this, then all those philosophers who try to solve the quantum riddle by trying to provide an alternative interpretation of the *current theory* which leaves all laws of this theory unchanged are wasting their time. Those who are not satisfied with the Copenhagen point of view must realize that only a new theory will be capable of satisfying their demands (Feyerabend 1962b, 260, fn 49).

Progress could only come from an alternative, realistically formulable theory, whose purpose was to compete with quantum mechanics in the microphysical domain and, while being in accordance with quantum mechanics to an approximation, would *contradict* quantum theory.

This point had been made already long before his reappraisal of Bohr while discussing how a future microphysical theory should look (still assuming von Neumann's no-go theorem to be unlimitedly valid).[10] The philosophical outline about how a new theory in the microphysical domain should look was a direct application of the anti-inductivist historical notion of progress of theory succession qua theory replacement. This is one of the most durable notions throughout Feyerabend's philosophical papers. Not so well known is that its genesis and argumentative use starts out in his papers on quantum mechanics, in which he consistently referred to the historical example of the intertheoretic relation between Kepler's and Newton's laws and referenced (often, but not always) a little-known paper by Karl

[10] And indeed von Neumann ([1932] 1955, 325) made the same point when he commented on his proof: "[…] we need not go any further into the mechanism of the 'hidden parameters,' since we know that the established results of quantum mechanics can never be re-derived with their help. […] The present system of quantum mechanics *would have to be objectively false*, in order that another description or the elementary processes than the statistical one may be possible" (emphasis added)—Feyerabend 'simply' added the methodological justification to pursue this goal.

Popper (1949).[11] The first use of the Kepler-Newton transition happens while discussing whether Bohm's first attempt (1952) at a hidden variables interpretation could bring back determinism in the realm of quantum theory.[12] Feyerabend's conclusion is that it cannot in its current form, but the reason lies in the fact that "Bohm takes up the task to construct an interpretation that does *not* contradict quantum theory":

> Physicists and philosophers who defend the idea that a causal interpretation of the formulas of quantum mechanics is possible are always very concerned that this interpretation does not contradict quantum theory. That is why von Neumann's proof seemed, for them, to represent an obstacle that could not be overcome. As a consequence, they overlook the fact that comprehensive theories, which unify a series of less comprehensive theories, almost invariably contradict them: Kepler's laws contradict Newton's theory, as they can be derived from it only approximately. As a consequence, as long as the contradiction between quantum theory and its allegedly causal interpretation falls under the threshold of measurement, its existence cannot be used as an argument against the interpretation (Feyerabend [1954a] 2015, 39–40; cf. Feyerabend 1954a, 104).

The historical point is repeated time and again from Feyerabend (1954b) [470-1][13] to Feyerabend (1965, 236, fn 44); the argumentative move can be found in Feyerabend (1958b).[14]

[11] This paper is of some historical significance for Feyerabend scholarship. It is the paper Popper gave at the *Internationalen Hochschulwochen* at Alpbach in 1948, when Feyerabend first met Popper; see Kuby (2010) for details. The paper appeared in English translation in Popper ([1963] 2002). Popper repeated the point in his (1983), 140, which is now the *locus classicus*.

[12] Bohm regarded his 1952 proposal as a proof-of-concept to show the limit of von Neumann's no-go theorem and thus the *possibility* of an hidden variables approach, not as an alternative physical theory.

[13] "The movement of the elements is very well described by Kepler's laws. However, these laws contradict Newton's theory (for they are valid only for an infinitely heavy Sun, and for the planets with negligible masses)" (Feyerabend [1954b] 2015, 17; cf. Feyerabend 1954b, 470–1).

[14] "[…] even if (a) and (b) were theorems of QM von Neumann's proof could not show, as has sometimes

And this point was not only made by Feyerabend on behalf of "quantum dissidents", but was made by the dissidents themselves. Already at the Colston Symposium Bohm is recorded as saying:

> I agree with Professor Rosenfeld that our theory cannot be entirely equivalent to quantum mechanics, but I also believe that every new theory must contradict the old theory in some respects. Quantum mechanics contradicts classical mechanics in very important respects [...] and nevertheless approaches classical mechanics as an approximation. [...] I believe that eventually we will come to a point where we contradict quantum mechanics and get consequences which simply are not consistent with the quantum of action (Körner 1957, 46).[15]

As we can see, Feyerabend's appreciation for quantum theory and Bohr's interpretation, on the one side, and his interest in alternative microphysical theories, on the other side, was not in contradiction; it was part of one and the same research problem: the question how a real alternative to quantum mechanics looks could be answered by studying the limits of quantum theory (Feyerabend 1965, 251, fn 125).

6 The problem of competing methodological rules

Is Feyerabend's way of disengaging from the dilemma appealing? In part, it is: As this notion of progress was built independently of the dilemma, developing it further to dissolve the dilemma doesn't seem an *ad hoc* move to save general methodology as a justification for axiological arguments for realism. Instead, it can be used as a further reason in an

been assumed, that determinism has been eliminated once and forever. For new theories of atomic phenomena will have to be more general; they will contain the present theory as an approximation; which means that, strictly speaking, they will contradict the present theory. Hence, they need no longer allow for the derivation of von Neumann's theorem" (Feyerabend 1958b, 345).

[15] The point that not every future microphysical theory will need to accommodate Planck's constant "in an essential way" is repeated in Feyerabend (1962b), 227.

argument about the progress of science. This argument was the development of theoretical pluralism as a methodological proposal, which had been in the making for some time.

And yet—and here I introduce the problematic kernel—divorcing the future progress of physics and the development of quantum mechanics (which after all was the future of physics at some point in time!) obscures the incompatibility of two opposite methodological rules applying to one and the same situation. Assume we take complementarity to be a methodological principle about how to handle statements involving classical concepts; then this principle, which tells scientists to restrict the validity of these statements, directly contradicts the methodological rule following from the principle of testability, according to which scientists ought to force the universal validity of these statements. And, in the specific instance of the development of quantum mechanics, Feyerabend is ready to admit that complementarity trumps a realistic interpretation, i.e. an instrumentalist interpretation is the 'right scientific move', the justified behavioral guideline as a mean to realize the principle of testability. Following our analysis of two levels of complementarity, Feyerabend circumvents the problem described because he avoids a methodological reading of complementarity grounded in physical argument on the one side, and rejects complementarity when viewed as a generalization justified on philosophical grounds on the other side. The strong emphasis on the physical grounding of complementarity has a double argumentative function: since, following his philosophical prescriptivism, physical reasons cannot justify general methodological rules and only axiological decisions can, as long as complementarity is treated as physically justified, it cannot have the status of a methodological rule—this avoids having to describe the situation of quantum mechanics in a way in which two general methodological rules, both justified on quite different grounds, are in conflict; and, where it is extended by further philosophical reasons to become a methodological rule, the philosophical reasons adduced can be thoroughly criticized methodologically and are shown to be "neither correct nor reasonable" (1962b, 195).

This leads to a very interesting if unintended result: Feyerabend's construal and appreciation of complementarity as a 'mere' heuristic move grounded in a specific research situation is actually the first instance of what he would later call a "rule of thumb": in contrast to methodological rules, there is no general justification for its application, it is only contextually valid in the scientific situation in which it shows its worth, and its future success cannot be inferred from its past success. Feyerabend's refusal to elevate complementarity to a general methodological rule (or to interpret indeterminateness as an application of this methodology) provides the template for his later negation of the existence of general methodological rules.

There is, furthermore, an even more obvious candidate for a methodological rule, the correspondence rule, which, as Feyerabend himself reports, 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" (1962b, 265, fn 62, emphasis added)). Feyerabend disengages the threat by limiting its reach, for it did not bring about the 'other part' of quantum theory: wave mechanics. Wave mechanics as the completion of quantum mechanics was, instead, constructed following a realistic demand "that was completely opposed to the philosophical point of view of Niels Bohr and his disciples" (1962b, 265, fn 62) including the correspondence rule. That wave mechanics turned out to be "just that complete rational generalization of the classical theory that Bohr, Heisenberg and their collaborators had been looking for" (1962b, 265, fn 62) is thus a lucky coincidence, not a result attributable to the correspondence rule. Similarly to the complementarity case, this handling of correspondence is a preview of a later concept, the notion of the limited validity of methodological rules.

Both cases show *in nuce* the difficulties that eventually would motivate Feyerabend to drop the universal justifiability of methodological rules. However, it does not need an incompatible methodological rule to provide a counterinstance to a given methodology. Feyerabend already admits that complementarity earned its place because a realistic interpretation didn't work out notwithstanding many attempts in this direction (also by Bohr, contrary to his philosophical inclinations):

> the [preceding] arguments [...] should have shown that there exist weighty

> *physical* reasons why at the present moment a realistic interpretation of the wave mechanics does not seem to be feasible [...]. A philosophical crusade for realism alone will not be able to eliminate these arguments. At best, it can ignore them. What is needed is a new theory. Nothing less will do (Feyerabend 1962b, 260, fn 49).

This negative result of achieving a realistic interpretation directly impinges on the *realizability* of Feyerabend's methodological proposal. But, as is well known, a counterinstance does not a falsification make and Feyerabend is adamant that the 'failure' of his methodological rule in a specific instance does not prove that it cannot be successful in the future (Feyerabend's papers abound with syntactical double negation constructions in this regard), which amounts to the assertion that an alternative to quantum theory allowing for a realistic interpretation has not been shown to be impossible. Feyerabend continues the preceding quotation:

> I have to admit, however, [...] that philosophical arguments for realism, though not sufficient, are therefore not unnecessary. It has been shown that given the laws of wave mechanics, it is impossible to construct a realistic interpretation of this very same theory. That is, it has been shown that the usual philosophical arguments in favor of a realistic interpretations of theoretical terms do not work in the case of quantum mechanics. [T]here 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, 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 1962b, 260, fn 49).

This sounds like all is well on the philosophical battlefield, but in fact this is a retreat.

Feyerabend moves the goal post from a methodological assurance that a realistic theory is not only desirable but realizable to the claim that such a theory has not been shown to be impossible or that the attempt to find one will be unsuccessful. We want to draw attention to this shift because there's a lesson to be learned from his reevaluation of Bohr: *the realizability of a realistic interpretation is not a given*.

Going back to Feyerabend's methodological arguments for realism and proliferation, we discover a further (necessary but unstated) premise, that scientific theories are in principle amenable to a realistic interpretation. This premise turns out to be false. The premise is quite innocent under the assumption that a realistic interpretation depends only on a decision about how to handle scientific statements, a decision independent from physical results and the specifics of the theory we want give a realistic interpretation of. But now it turns out that the specifics of actual research can pose constraints on this handling. *This is the moment in which justified actual scientific practice comes in contact—one may say: comes in the way of—Feyerabend's conception of methodology as conceived in his philosophical prescriptivism*.

Feyerabend found himself in a tough spot: he welcomed cases in which philosophical notions get in contact with experience; at the same time he needed the philosophical notion of realism to be a consequence of volitional decisions. His response was ambivalent: he recognized the result, but he did not accept the consequences, making several attempts not to give up the methodological argument for a realistic interpretation of alternative theories by bringing the principle of testability to its argumentative limits. The best example is his paper "Realism and instrumentalism: Comments of the logic of factual support" (1964), which, notably, is devoted to flatly arguing "that realism is preferable to instrumentalism" (Feyerabend 1964, 280): He further strengthened the argument for proliferation by claiming that alternative theories are not only more likely to maximize the testability of established theories, but that there exist situations in which realistically interpreted alternative theories are necessary in principle in order to test the established theory. But this argument chokes in light of his reevaluation of Bohr. The methodological arguments for realism work only as long as we disregard the (admittedly surprising) discovery that the issue of ontological interpretation can be an ontologically relevant consequence of physical theory. For it is now possible that no future theory may admit of a realistic interpretation on scientific grounds. His methodological argument for a realistic interpretation of scientific theories has become unsuccessful.[16]

The argument's failure is not a black box, we can pinpoint the exact source of the problem; it lies in the fact that *the principle of testability cannot warrant an inference to realism anymore, which is the very core of all Feyerabend's methodological arguments for realism*. We can also speculate as to why Feyerabend did not immediately recognize this problem: the contextualization of an argument for realism in a broader argument for theoretical pluralism put the development of alternative theories at the center of attention: this was an independent mean to realizing the testability principle; the realistic interpretation of these alternatives has become an additional step towards testability. And Feyerabend is right to push his argument insofar as the argument for theory proliferation (as distinct from their realistic interpretation) still works, it remains unaffected by the discovery that the issue of interpretation can be amongst the ontological consequences of a scientific theory. But Feyerabend wanted more. As late as the date of the paper under scrutiny, Feyerabend thought that also the demand of a realistic interpretation of those empirically (still) unconfirmed alternatives was a "plausible demand which immediately follows from the principle of testability (1964, 308). But this further inference is now unwarranted. As he lays down his argument Feyerabend even distinguishes the two points:

> [1:] 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

[16] Barring, that is, the discovery of a principle applicable to theory construction that can guarantee a realistic interpretation on physical grounds in addition to all other requirements that a successor theory has to fulfill; or a realizable condition that can guarantee the exclusion that the issue of interpretation is among the relevant ontologically consequences of the theory so constructed. None of these options have been explored by Feyerabend, as far as I am aware.

> whatever theory he possesses [2:] and it is also demanded that these further theories be developed in their strongest possible form, i.e. as descriptions of reality rather than as mere instruments of successful prediction (1964, 306).

In this passage we can pinpoint where Feyerabend's principle of testability as a cogent argument for realism chokes in light of his reevaluation of Bohr: (1) still works, but (2) does not. In other words, Feyerabend did distinguish the two points, but did not distinguish their different warrants.[17]

7 Conclusion

The strong methodological argument for realism fails and I claim that Feyerabend came to this realization, too.[18] The conceptual problem behind the argumentative failure lies in the equation of "in their strongest form" and "descriptions of reality" to mean "realism". This very equality has been shown to be wrong by his reevaluation of Bohr. The discovery that the issue of realism itself can be an 'ontologically relevant' consequence of a physical theory is not only potentially disruptive vis-à-vis axiological arguments for realism, it leads by itself to an almost paradoxical situation in the case of quantum mechanics:

[17] His review of Ernest Nagel's *Structure of Science* (Feyerabend 1966) is the last published appearance of the argument that "strong reasons" against a realistic interpretation of the quantum theory "can be removed only by arguments showing that it is desirable to introduce theories which contradict already existing laws" and he shows "that such arguments can be provided" (Feyerabend 1966, 248). All later references to an a strong methodological argument for theoretical pluralism only concern the proliferation principle proper, the realistic interpretation of alternatives is now omitted.

[18] His review of Ernest Nagel's *Structure of Science* (Feyerabend 1966) is the last published appearance of the argument that "strong reasons" against a realistic interpretation of the quantum theory "can be removed only by arguments showing that it is desirable to introduce theories which contradict already existing laws" and he shows "that such arguments can be provided" (Feyerabend 1966, 248). All later references to an a strong methodological argument for theoretical pluralism only concern the proliferation principle proper, the realistic interpretation of alternatives is now omitted.

(1) Realism exhorts scientists to take the ontological consequences of their physical theories at face value (to develop them “in their strongest possible form”)

(2) Taking the ontological consequences of quantum mechanics at face value (to develop them “in their strongest possible form”) results in an instrumentalist interpretation of the theory.

If the expression ‘to take the ontological consequences of a physical theory seriously’ was used by Feyerabend synonymously with a realistic interpretation of the theory, in the specific case of quantum mechanics it leads to the opposite interpretation in that it forces us to accept the limited validity of central concepts of the theory, as Bohr had argued. This is not an instance of Feyerabend’s dilemma (which concerned competing sources of justification of how to interpret scientific theories), but shows a problem with Feyerabend’s realism, i.e. with the concept of ‘ontological interpretation’ of a scientific theory itself, which escaped him at first, probably because of his thinking in Popperian terms of “positivism” vs. “realism”. Feyerabend’s tendency to describe the interpretative situation of quantum mechanics in a (grammatically and evaluative) negative way, i.e. as the “impossibility” of a realistic interpretation, forbade him to appreciate Bohr’s interpretative outcome as a full-fledged ontological interpretation, i.e. instrumentalism as possible “description of reality”. The discovery, in the end, amounts to a refutation of Feyerabend’s philosophical concept of realism in its general application to science, i.e. it shows the inadequateness of hidden philosophical premises in Feyerabend’s realistic conception.

Feyerabend came not only to recognize this point, he embraced it. In his introduction to the publication of his Collected Papers, Volume I, he commented on two reissued papers (including the paper discussed at length in this section) by admitting that, because of the specific arguments found in his reevaluation of Bohr, these turn out to be “somewhat misleading” (1985, 15):

> Producing philosophical arguments for a point of view whose applicability has to be decided by concrete scientific research, they suggest that scientific

realism is the only reasonable position to take, come what may, and inject a dogmatic element into scientific discussion […]. Of course, philosophical arguments should not be avoided; but they have to pass the test of scientific practice. *They are welcome if they help the practice*; they must be withdrawn if they hinder it, or deflect it in undesirable directions" (1985, 15–16).

> The issue between realism and instrumentalism gives rise to similar observations. Do electrons exist or are they merely fictitious ideas for the ordering of observations (sense data, classical events)? It would seem that the question has to be decided by research. […] Modern professional realists do not see matters in this way. For them the interpretation of theories can be decided on purely methodological grounds and independently of scientific research. Small wonder that their notion of reality and that of the scientists have hardly anything in common (Feyerabend 1978, 39).

A consequence of this view applied to realism is first presented in his paper "On a recent critique of complementarity" (1968; 1969). Prompted by widely-received critiques of the Copenhagen interpretation by Mario Bunge and Karl Popper in Bunge (1967), Feyerabend reissued once more his arguments about the physical grounding of Bohr's point of view, but this time he did not attempt to limit its ontological consequences on methodological grounds. A methodological argument for realism was nowhere to be found. Instead, he exposed "the myth of Bohr's dogmatism" (1969, 85, fn 61), pointed out Bunge's ignorance "of Bohr and the actual development of ideas within the"Copenhagen Circle" (1969, 92, fn 81) and explained how Bohr's interpretation had arisen from a process of "refutations and discoveries", not of "philosophical dogmatism" (1969, 92). Feyerabend's conclusion now was "Back to Bohr!" (1969, 103).

References

Bell, John S. 1964. "On the Einstein Podolsky Rosen Paradox." *Physics* 1 (3): 195–290.

Bohm, David. 1952. "A Suggested Interpretation of the Quantum Theory in Terms of "Hidden" Variables. I." *Physical Review* 85 (2): 166–79, 180–93.

———. 1953. "Proof That Probability Density Approaches $|\psi|^2$ in Causal Interpretation of the Quantum Theory." *Physical Review* 89 (2): 458–66.

———. 1957a. "A Proposed Explanation of Quantum Theory in Terms of Hidden Variables at a Sub-Quantum-Mechanical Level." In *Observation and Interpretation: A Symposium of Philosophers and Physicists, Proceedings of the Ninth Symposium of the Colston Research Society*, edited by S. Körner and M. H. L. Pryce, 33–40. London and New York: Butterworths Scientific Pub.

———. 1957b. *Causality and Chance in Modern Physics*. London: Routledge & Kegan Paul.

Bohm, David, and Y. Aharonov. 1957. "Discussion of Experimental Proof for the Paradox of Einstein, Rosen, and Podolsky." *Physical Review* 108 (4): 1070–6. doi:10.1103/PhysRev.108.1070.

Bohm, David, and J. P. Vigier. 1954. "Model of the Causal Interpretation of Quantum Theory in Terms of a Fluid with Irregular Fluctuations." *Physical Review* 96 (1): 208–16.

Bunge, Mario, ed. 1967. *Quantum Theory and Reality*. Studies in the Foundations, Methodology and Philosophy of Science 2. Berlin: Springer.

Feyerabend, Paul K. 1951. "Zur Theorie der Basissätze." Dissertation zur Erlangung der Doktorgrades an der philosophischen Fakultät der Universität Wien, Wien: Universität Wien.

———. 1954a. "Determinismus und Quantenmechanik." *Wiener Zeitschrift für Philosophie, Psychologie, Pädagogik* 5 (2): 89–111.

———. 1954b. “Physik und Ontologie.” *Wissenschaft und Weltbild: Monatsschrift für alle Gebiete der Forschung* 7 (11/12): 464–80.

———. 1956. “Eine Bemerkung zum Neumannschen Beweis.” *Zeitschrift für Physik* 145 (4): 421–23.

———. 1957a. “On the Quantum-Theory of Measurement.” In *Observation and Interpretation: A Symposium of Philosophers and Physicists, Proceedings of the Ninth Symposium of the Colston Research Society*, edited by S. Körner and M. H. L. Pryce, 121–30. London and New York: Butterworths Scientific Pub.

———. 1957b. “Review of Foundations of Quantum-Mechanics: A Study in Continuity and Symmetry, Alfred Landé.” *The British Journal for the Philosophy of Science* 7 (28): 354–57.

———. 1957c. “Zur Quantentheorie der Messung.” *Zeitschrift für Physik* 148 (5): 551–59.

———. 1958a. “Complementarity.” *Proceedings of the Aristotelian Society, Supplementary Volumes* 32 (January): 75–122.

———. 1958b. “Mathematical Foundations of Quantum Mechanics, R. T. Beyer (Trans.), Princeton University Press: Princeton (NJ) 1955.” *The British Journal for the Philosophy of Science* 8 (32): 343–47.

———. 1958c. “Reichenbach’s Interpretation of Quantum-Mechanics.” *Philosophical Studies* 9 (4): 49–59. doi:10.2307/4318304.

———. 1960a. “On the Interpretation of Scientific Theories.” In *Proceedings of the 12th International Congress of Philosophy, Venice, 12-18 September 1958*, 151–59. Logic, Theory of Knowledge, Philosophy of Science, Philosophy of Language 5. Florence: Sansoni.

———. 1960b. “Professor Landé on the Reduction of the Wave Packet.” *American Journal of Physics* 28 (5): 507. doi:10.1119/1.1935853.

———. 1960c. “Professor Bohm’s Philosophy of Nature.” *The British Journal for the Philosophy of Science* 10 (40): 321–38.

———. 1961. “Niels Bohr’s Interpretation of the Quantum Theory.” In *Current Issues in the Philosophy of Science: Symposia of Scientists and Philosophers*, edited by Herbert Feigl and Grover Maxwell, 371–90. Proceedings of Section L of the American Association for the Advancement of Science. New York: Holt, Rinehart and Winston.

———. 1962a. *Knowledge Without Foundations: Two Lectures Delivered on the Nellie Heldt Lecture Fund*. Oberlin, OH: Oberlin College.

———. 1962b. “Problems of Microphysics.” In *Frontiers of Science and Philosophy*, edited by Robert G. Colodny, 189–283. University of Pittsburgh Series in the Philosophy of Science 1. Pittsburgh: University of Pittsburgh Press.

———. 1963. “Über Konservative Züge in Den Wissenschaften Und Insbesondere in Der Quantentheorie Und Ihre Beseitigung.” *Club Voltaire. Jahrbuch Für Kritische Aufklärung.* 1: 280–93.

———. 1964. “Realism and Instrumentalism: Comments of the Logic of Factual Support.” In *The Critical Approach to Science and Philosophy: In Honor of Karl R. Popper*, edited by Mario Bunge, 280–308. London and New York: The Free Press of Glencoe.

———. 1965. “Problems of Empiricism.” In *Beyond the Edge of Certainty: Essays in Contemporary Science and Philosophy*, edited by Robert G. Colodny, 2:145–260. University of Pittsburgh Series in the Philosophy of Science. Englewood Cliffs (NJ): Prentice-Hall.

———. 1966. “The Structure of Science.” *The British Journal for the Philosophy of Science* 17 (3): 237–49. doi:10.2307/686761.

———. 1968. “On a Recent Critique of Complementarity: Part I.” *Philosophy of Science* 35 (4): 309–31. doi:10.2307/186251.

———. 1969. “On a Recent Critique of Complementarity: Part II.” *Philosophy of Science* 36 (1): 82–105. doi:10.2307/186564.

———. 1970. “Philosophy of Science: A Subject with a Great Past.” *Historical and Philosophical Perspectives of Science* 5: 173.

———. 1975. *Against Method: Outline of an Anarchistic Theory of Knowledge*. London: New Left Books.

———. 1978. *Science in a Free Society*. London: New Left Books.

———. 1985. “Introduction: Scientific Realism and Philosophical Realism.” In *Realism, Rationalism and Scientific Method: Volume 1: Philosophical Papers*, edited by John Preston. Cambridge University Press.

———. 1995. *Killing Time: The Autobiography of Paul Feyerabend*. Chicago: University of Chicago Press.

———. (1954a) 2015. “Determinism and Quantum Mechanics (1954).” In *Physics and Philosophy*, edited by Stefano Gattei and Joseph Agassi, 25–45. Cambridge, UK; New York, NY: Cambridge University Press.

———. (1954b) 2015. “Physics and Ontology (1954).” In *Physics and Philosophy*, edited by Stefano Gattei and Joseph Agassi, 9–24. Cambridge, UK; New York, NY: Cambridge University Press.

———. (1948) 2016. “The Concept of Intelligibility in Modern Physics (1948).” Translated by Daniel Kuby and Eric Oberheim. *Studies in History and Philosophy of Science Part A* 57: 64–66. doi:10.1016/j.shpsa.2015.11.016.

———. n.d. *Feyerabend’s Formative Years: Correspondences and Unpublished Papers*. Edited by Matteo Collodel and Eric Oberheim. 2 vols. Chicago, IL: University of Chicago Press.

Howard, Don. 2004. “Who Invented the ‘Copenhagen Interpretation’? A Study in Mythology.” *Philosophy of Science* 71 (5): 669–82. doi:10.1086/425941.

Kožnjak, Boris. 2017. “The Missing History of Bohm’s Hidden Variables Theory: The Ninth Symposium of the Colston Research Society, Bristol, 1957.” *Studies in History and*

Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics, July. doi:10.1016/j.shpsb.2017.06.003.

Körner, S., ed. 1957. *Observation and Interpretation: A Symposium of Philosophers and Physicists, Proceedings of the Ninth Symposium of the Colston Research Society*. London and New York: Butterworths Scientific Pub.

Kuby, Daniel. 2010. "Paul Feyerabend in Wien 1946–1955: Das Österreichische College und der Kraft-Kreis." In *Auf der Suche nach authentischem Philosophieren. Philosophie in Österreich 1951–2000. Verdrängter Humanismus-verzögerte Aufklärung*, edited by M. Benedikt, R. Knoll, F. Schwediauer, and C. Zehetner, 1041–56. Verdrängter Humanismus - verzögerte Aufklärung: Philosophie in Österreich von 1400 bis heute 6. Wien: WUV. https://www.facultas.at/list/9783708904467.

———. 2016. "Feyerabend's 'the Concept of Intelligibility in Modern Physics' (1948)." *Studies in History and Philosophy of Science Part A* 57 (June): 57–63. doi:10.1016/j.shpsa.2015.11.004.

Popper, Karl R. 1949. "Naturgesetze und theoretische Systeme." In *Gesetz und Wirklichkeit. Internationale Hochschulwochen des Österreichischen College Alpbach, Tirol, 21.8.-9.9.1948*, edited by S. Moser, 2:43–60. Innsbruck: Tyrolia-Verlag.

———. 1956. "Three Views Concerning Human Knowledge." In *Contemporary British Philosophy*, edited by Hywel David Lewis, 357–88. Third Series. London: Allen and Unwin.

———. 1983. *Realism and the Aim of Science*. London: Hutchinson.

———. (1963) 2002. *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge.

von Neumann, John. (1932) 1955. *Mathematical Foundations of Quantum Mechanics*. Translated by Robert T. Beyer. Princeton, NJ: Princeton University Press.