INTERPRETING FEYERABEND

Critical Essays

EDITED BY

KARIM BSCHIR

University of St. Gallen, Switzerland

JAMIE SHAW

University of Toronto

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CHAPTER 7

Feyerabend's Re-evaluation of Scientific Practice
Quantum Mechanics, Realism and Niels Bohr

Daniel Kuby

7.1 Introduction

In this chapter, I offer a specific interpretation of how Feyerabend came from a Popperian critique of the Copenhagen interpretation to a detailed re-evaluation 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 1975a).[1]

In his early philosophy and until the mid-1960s, even though he used historical case studies, Feyerabend positioned himself decidedly against the wave in philosophy of science that would eventually be labelled as its 'historical turn'.[2] 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 recognised that his previous philosophical stance had a normative urgency towards the right

[1] The distinction between an 'early' and a 'later' Feyerabend has been canonised by Preston (1997a), according to whom Feyerabend's 'work can be (roughly) divided into two phases, the first stretching from the early 1950s until about 1970, the second from 1970 onwards' (Preston 1997a, p. 7). Oberheim (2006, pp. 15–16) has sensibly objected that many of Feyerabend's post-1970 ideas can (quite literally) be traced back to earlier writings throughout the 1960s. I agree with Oberheim, but I contend that there is a wall we hit in the back-dating game, around 1964–1965, such that famous articles usually classified as pertaining to the historical turn (like Feyerabend 1962a; Feyerabend 1965a) cannot and should not be assimilated to it. I also agree with Oberheim that the continuity in Feyerabend's philosophical work is much better located at the level of his metaphilosophical commitments, though I give a rather different interpretation of what these are (cf. next footnote).

[2] See e.g. the letters from Feyerabend to Kuhn on a draft of *Structure of Scientific Revolutions,* (edited in Hoyningen-Huene 2002; Hoyningen-Huene 2006) for some evidence. Hoyningen-Huene (2002, pp. 68–72) repudiates Feyerabend's criticism of Kuhn as a misunderstanding of Kuhn's position. My (2019a) agrees with Hoyningen-Huene, but gives an explanation for this misunderstanding based on Feyerabend's metaphilosophical background as developed in my (2019b).

methodology to be used in the sciences that his later philosophy would lack. Indeed, I propose to recognise 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-à-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 a consistent set of methodological rules, most importantly the demand to interpret our best scientific theories realistically, as means to realise the core scientific value of testability. I reconstruct Feyerabend's methodological argument for realism as follows:

P1: Theory-testing is a constitutive task of science. (Principle of Testability).
P2: Interpreting scientific theories realistically is more likely to maximize their testability than alternative interpretations.
P3: If scientists want to test scientific theories, they should interpret scientific theories realistically.
Conclusion: Scientists should interpret scientific theories realistically. (Realism)

The justification of testability as a core scientific value, however, was based in a purely axiological decision concerning the aims of science (see Feyerabend 1961/1981 for a statement of this view and Kuby 2019b for discussion).

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 (normative claims could be raised only contextually to a specific research situation). 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 centre stage. In this chapter, 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 the early and later Feyerabend (as has been given in Preston 1997a). In fact, I will offer a very standard explanation in terms of a simple model of change of belief through evidence.

My main claim in this chapter is that the evidence he was exposed to came through his engagement in (the history of) quantum mechanics, in particular with a re-evaluation 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 recognised specific instances of arguably scientific theories for himself, 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 a 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 Feyerabend's philosophical prescriptivism, the compelling reasons for a specific scientific behaviour are axiological.
2. There's a specific class of scientific behaviour that Feyerabend finds compelling, for reasons that are independent from axiological considerations.[3]

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), that is, to be forced to recognise that the

[3] I claim that Feyerabend's conception of philosophy of science, even at its prescriptivist peak, is not free from a task inherited from previous philosophies of science: whatever scientific method is proposed, it has to account for the shared canon of modern science. Here Feyerabend gives probative value to the canon of past scientific achievements in the form a shared set of exemplary contributors to science (what Laudan (1989, p. 213) calls 'the Tradition'), independently from axiological considerations. As Laudan (1989) has pointed out, one of the main tasks of new positions defended in philosophy of science has been to reinterpret the Tradition to fit their view on methodology. Even if the Tradition varies in scope, prototypical scientists like Galileo, Newton and Einstein are there: [A]ny proposals about the aims [and methods] of science must allow for the retention as scientific of much of the exemplary work currently and properly regarded as such. A suggested aim [and method] for science which entailed, for instance, that nothing in Newton's *Principia* was really scientific after all would represent such a distortion of scientific practice that it would be wholly uncompelling' (Laudan 1990, p. 47).

suggested that other interpretations were possible. Second, he distinguished between 'syntax' and 'semantics' of elementary quantum theory, that is, 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 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'.[13]

This very specific organisation 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:

[13] Presumably Bohm (1953), Bohm and Vigier (1954), and possibly Bohm and Aharonov (1957); Feyerabend might also have had 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 in Feyerabend 2020, p. 259).

behaviour in (2) is not chiefly dependent on axiological decisions. The problem arises if the methodological behaviours 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.[4]

I will proceed as follows: Section 2 gives the set-up of the early Feyerabend as a philosopher chiefly preoccupied with quantum physics and its philosophy, who attacked the 'Copenhagen interpretation' as an instrumentalist interpretation of quantum mechanics along Popperian lines. I also note that we can pinpoint at first a timid change of mind with regard to Niels Bohr to 1957. In Section 3, I argue that, motivated by this change of mind, Feyerabend's early goal was to disentangle, respectively, the philosophical and the empirical support for the initial introduction of the principle of complementarity in quantum mechanics. I then distinguish between the vindication of complementarity in quantum mechanics, which Feyerabend came to appreciate on empirical grounds, and complementarity as a general requirement for future microphysical theories, which Feyerabend still rejected on philosophical grounds. In Section 4, I introduce Feyerabend's framework for distinguishing syntax, semantics and ontology of scientific theories in order to understand his original interpretation of Bohr's complementarity as an ontological interpretation grounded in physical arguments. This construal leads to a first instantiation of 'Feyerabend's dilemma': with regard to the same theory, philosophical reasons compel us to demand a realistic interpretation, while physical reasons compel us to accept an instrumentalist interpretation. In Section 5, I show that Feyerabend reduced the problematic import of the dilemma by appealing to his theoretical pluralism. Indeed, the dilemma

[4] 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 realisable, 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 (2019b).

could be used for an argument for the principle of proliferation, demanding realistically interpreted alternative theories in microphysics. In Section 6, I argue that the dilemma was only circumvented, not resolved. I argue that Feyerabend maneuvered himself into too strong an argument for realism in microphysics by showing that the general inference from testability to realism is not viable any more (while leaving the inference to proliferation intact). Section 7 concludes by arguing that Feyerabend came to this realisation, too. More than that, he viewed this failure as a refutation of his own philosophically motivated, theory-independent notion of realism, leading him to embrace the methodological constraints put in place by scientific theories and the local and fallible status of methodological rules, to which Bohr's contribution in quantum mechanics he now considered a testament.

7.2 Feyerabend and Quantum Mechanics

Feyerabend is often thought of as a philosopher working in general philosophy of science – and, since his (1970e), 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 (Feyerabend 1954a; 1956; 1957a; 1957b; 1957c; 1958a; 1958b; 1958c; 1960a; 1960b; 1960c; 1961; 1962b; 1963b). In earlier as well as later papers quantum mechanics continued to surface as historical casuistry. Feyerabend got to work on quantum mechanics from the late 1940s as a trained physicist with an interest in philosophy of physics. His earliest extant paper (Feyerabend 1948/2016), written as an undergraduate student, deals with the concept of intelligibility in microphysics (cf. Kuby 2016). We do not know the exact topic of his attempted dissertation thesis, which was to deal with classical electrodynamics – but we know that he abandoned it 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 abroad at the London School of Economics in 1952 with Karl Popper and took off from there (cf. Feyerabend 1995, p. 92). Feyerabend came to work on a large number 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. 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, 12 March 1954, in Feyerabend 2020, p. 117).[5] At first, very 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, p. 34; Feyerabend 1954b/2015, p. 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, p. 360)[6]

But then something changed. In his (1958a), 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.[7] 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 '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

[5] Preston (1997a) highlights the Popperian heritage of many themes in Feyerabend's early philosophy. Farrell (2000, 275) claims that even the later Feyerabend was 'in many respects, a die-hard pluralistic Popperian'. For a more nuanced view of the institutional and philosophical relationship between Feyerabend and Popper, see Collodel (2016).

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

[7] A charge levelled in that context against Von Neumann's (1932/1955) presentation of the theory; Feyerabend (1958a, p. 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.'

I did not know it well enough' (Feyerabend to Popper, 21 July 1957, in Feyerabend 2020, p. 267).

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 (April 1st–4th), 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 defences 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.[8] This gave Feyerabend pause – not 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 in 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, recognising his unique perspective (Feyerabend 1958c; 1961; 1962b; 1968a; 1969b).

7.3 The Role of Physical Argument: Feyerabend Re-evaluates 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.

[8] An account of this incident and of Feyerabend's contribution to the rise of the quantum measurement problem is in preparation.

Feyerabend first followed Popper, but then came in contact with historical protagonists, 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 (1958c), Feyerabend wants

> 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 1958c, pp. 80–81)[9]

Initially, he recognised Bohr's notion of 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' (p. 78). In this sense, it is 'closely connected with the position of realism' (p. 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*' (p. 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' (pp. 87–88). It is therefore an abdication of realism in that it not only a) gives up universal applicability of quantum-mechanical concepts as a condition of explanation, but b) by replacing traditional theories with the notion of 'natural generalization of classical physics' following the correspondence rule, also of any future quantum theory (p. 90).

9 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' (Feyerabend 1958c, p. 80).

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 rundown of how complementarity fits well with elementary 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 makes 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 (pp. 103–104).

Next, Feyerabend went into a detailed examination of the source literature 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 his (1961), and reaffirmed much later in his long two-part paper 'On a recent critique of complementarity' (1968a; 1969b), prompted by Mario Bunge (1967), which Feyerabend deplored. It was in 'Problems of microphysics' that for the first time he put the (mostly qualitative) *physical reasoning* at the centre stage: Bohr's 'point of view can stand upon its own feet and does not need any support from philosophy' (Feyerabend 1962b, p. 292). He lays out 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 to make them acceptable. (p. 195, emphasis added)

I want to draw attention to the emergence of the notion of 'physical arguments' as a crucial step in Feyerabend's re-evaluation 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' (p. 194). For him, 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 Feyerabend's dilemma, that is, a specific class of reasons for scientific behaviour 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 assumption of the *indeterminateness of state descriptions*, of which he takes complementarity to be an abstract generalisation. He tracks in detail the introduction of the assumption as a 'physical hypothesis' (he underlines time and again its objective character)[10] 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' (p. 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, p. 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.'[11]

Second, 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' (p. 197). Having dispelled misreadings of the indeterminateness assumption *as proposed by Bohr*, he proceeds to explain how the hypothesis stood up successfully against two alternatives (Planck and Schrödinger: ψ-waves as complete and well-defined states; statistical ensemble interpretation of the ψ-function) to explain the physical and conceptual problems on the table (pp. 203–207). He shows how Bohr himself tried to come up with alternatives, only to be thrown back to indeterminateness 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' (p. 207), and, since the 'qualitative considerations'

10 Cf. Feyerabend (1962c, p. 194, p. 220, emphasis added): 'Most critics interpret the two main principles of the Copenhagen Interpretation, namely, the principle of the indeterminateness of state descriptions and the principle of the relational character of quantum mechanical states not as physical assumptions which describe *objective features of physical systems*; they interpret them as the direct result of a positivistic epistemology and reject them together with the latter. [. . .] We first presented a physical hypothesis which was introduced by Bohr to explain certain features of microscopic systems (for example, their wave properties). It was pointed out that this physical hypothesis is of a *purely objective character* and that it is also needed, in addition to Born's rules, for a satisfactory interpretation of the formalism of wave mechanics.'

11 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 1962c, p. 197). (This example is repeatedly used to suggest a non-philosophical reading, see 1958b, p. 51; 1960c, p. 323; 1961, p. 373; 1964, p. 294; 1969b, pp. 94–95).

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 (p. 208).

Feyerabend (pp. 208–220) 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 Einstein, Podolsky and Rosen (1935) 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' (Feyerabend 1962b p. 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' (p. 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' (p. 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' (p. 222). To show in which way our interpretation of Feyerabend's view 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"' (p. 222). Because Feyerabend uses the rest of the paper to severely criticise these further assumptions from a methodological point of view, it may seem that he does *reject* complementarity after all. But this is not correct. First, we have to distinguish 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]t is very important to realise 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 theory construction which was imposed from the beginning and in accordance with which, part of the quantum theory was actually obtained.* (p. 265 fn. 62)

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. (p. 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, that is, 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. 1958c, p. 90) and to be further discussed below.

More generally, behind this re-evaluation 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 Don Howard's claim that Feyerabend was among 'the most important enablers of the myth' (Howard 2004, p. 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' (p. 670), this claim should be limited to his pre-1958 papers. After this, he was an active myth-buster.

7.4 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.[12] 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 conceptualised 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 Centre for the Philosophy of Science. First, 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 theory was assumed; and, as Leon Rosenfeld did, the very expression 'interpretation' was questioned because the term

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

> 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 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, that is, this framework is in place *before* Feyerabend's re-evaluation of Bohr. This tells us two things: first, 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 re-evaluation of Bohr. (Indeed, this contention is one of the most pristine expressions of Feyerabend's understanding of the rapprochement of science and philosophy and it was already clearly expressed in Feyerabend (1954b/2015)).

Second, 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 recognised 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 (1958c) that we gave above, in particular point (a).

Feyerabend's re-evaluation 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. 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.

7.5 The Limits of Quantum Theory and Hidden Variable Alternatives

At first, Feyerabend made the contextual re-evaluation of complementarity fit with general methodology, in particular with his methodological argument for realism presented in the introductory section. 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 realise that only a new theory will be capable of satisfying their demands. (Feyerabend 1962b, p. 260 fn. 29)

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 like (still assuming von Neumann's no-go theorem to be unlimitedly valid).[14] The philosophical outline about how a new theory in the microphysical domain should look like was a direct application of the anti-incrementalist notion of historical 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 inter-theoretic relation between Kepler's and Newton's laws and referenced (often, but not always) a little-known paper by Karl Popper (1949).[15] The first use of the Kepler–Newton transition happens while discussing the question whether Bohm's first attempt (1952) at a hidden variables interpretation could bring back determinism in the realm of quantum theory.[16] 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

[14] And indeed von Neumann (1932/1955, p. 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.

[15] This chapter 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). Popper repeated the point in his (1983, p. 140), which is now the *locus classicus*.

[16] 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 a hidden variables approach, not as an alternative physical theory.

> 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, pp. 39–40; cf. Feyerabend 1954a, 104)

The historical point is repeated time and again from Feyerabend (1954b, pp. 470–471)[17] to Feyerabend (1965, p. 236 fn. 44); the argumentative move can be found in Feyerabend (1958a).[18] 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, p. 46)[19]

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 1965a, p. 251 fn. 125)

7.6 The Problem of Competing Methodological Rules

Is Feyerabend's way of disengaging from the dilemma appealing? In part, it is: as the notion of progress through theoretical pluralism was built

[17] '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, p. 17; cf. Feyerabend 1954b, pp. 470–471).

[18] '[E]ven if (a) and (b) were theorems of [quantum mechanics] von Neumann's proof could not show, as has sometimes 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 1958a, p. 345).

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

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 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, that is an instrumentalist interpretation is the 'right scientific move', the justified behavioural guideline as a mean to realise 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 generalisation 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 criticised on axiological grounds and are shown to be 'neither correct nor reasonable' (Feyerabend 1962b, p. 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' (p. 265 fn. 62, emphasis added). In this case, 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' (p. 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' (p. 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 counter-instance 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 the latter's own philosophical inclinations):

> [T]he [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. (p. 260 fn. 49)

This negative result of achieving a realistic interpretation directly impinges on the *realisability* of Feyerabend's methodological proposal. But, as is well known, a counter-instance does not make a falsification 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 favour 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. (p. 260 fn. 49)

This sounds like all is well on the philosophical battlefield, but in fact this is a retreat. Feyerabend moves the goalpost from a methodological assurance that a realistic theory is not only desirable but realisable 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 is a lesson to be learned from his re-evaluation of Bohr: the realisability 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 come 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 recognised 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, p. 280). He further strengthened the argument for proliferation by claiming that alternative theories are not only more likely to maximise the testability of established theories, but also that there exist situations in which realistically interpreted alternative theories are necessary in principle to test the established theory. But this argument chokes in light of his re-evaluation 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 will admit a realistic interpretation on scientific grounds. His methodological argument for a realistic interpretation of scientific theories has become unsuccessful.[20]

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 recognise this problem. The contextualisation of an argument for realism in a broader argument for theoretical pluralism put the development of alternative theories at the centre of attention: this was an independent mean to realising the testability principle; the realistic interpretation of these alternatives has become an additional step towards testability. And Feyerabend was 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

[20] 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 realisability condition that can guarantee that issues of interpretation are excluded from the relevant ontologically consequences of the theory so constructed. None of these options have been explored by Feyerabend, as far as I am aware.

alternatives was a plausible demand which immediately follows from the principle of testability (1964, p. 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 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. (p. 306)

In this passage, we can pinpoint where Feyerabend's principle of testability as a cogent argument for realism chokes in light of his re-evaluation 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.

7.7 Conclusion

The strong methodological argument for realism fails, and I claim that Feyerabend came to this realisation, too.[21] 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 re-evaluation 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:

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

[21] 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' (p. 248). All later references to a strong methodological argument for theoretical pluralism only concern the proliferation principle proper; the realistic interpretation of alternatives is now omitted.

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, that is with the concept of 'ontological interpretation' of a scientific theory itself, which escaped him at first, perhaps because of his thinking in Popperian terms of 'positivism' versus 'realism'. Feyerabend's tendency to describe the interpretative situation of quantum mechanics in a (grammatically and evaluative) negative way, that is as the 'impossibility' of a realistic interpretation, forbade him from appreciating Bohr's interpretative outcome as a fully fledged ontological interpretation, that is, 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, that is it shows the inadequateness of hidden philosophical premises in Feyerabend's realistic conception.

Feyerabend came not only to recognise this point, indeed 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 re-evaluation of Bohr, these turn out to be 'somewhat misleading' (Feyerabend 1981b, p. 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. (pp. 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 1978b, p. 39)

A consequence of this new view applied to realism is first presented in his paper 'On a Recent Critique of Complementarity' (1968a; 1969b).

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' (Feyerabend 1969b, p. 85 fn. 61), pointed out Bunge's ignorance 'of Bohr and the actual development of ideas within the 'Copenhagen Circle'' (p. 92 fn. 81) and explained how Bohr's interpretation had arisen from a process of 'refutations and discoveries', not of 'philosophical dogmatism' (p. 92). Feyerabend's conclusion now was 'back to Bohr!' (p. 103).[22]

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