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Matteo Collodel  
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Feyerabend's  
Formative Years.  
Volume 2. Feyerabend  
on Logical Empiricism,  
Bohm & Kuhn

Correspondence and Unpublished  
Papers



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## Volume 10

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Matteo Collodel • Eric Oberheim  
Editors

# Feyerabend's Formative Years. Volume 2. Feyerabend on Logical Empiricism, Bohm & Kuhn

Correspondence and Unpublished Papers

 Springer

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# Abbreviations

AAAS	American Association for the Advancement of Science
CASBS	Center for Advanced Studies in the Behavioral Sciences
c	Copy or carbon copy
CH	Hempel's annotation
d	Draft
E	Editorial note
e	Envelope
EFA	<i>Europäisches Forum Alpbach</i> [European Forum Alpbach; known before 1949 as IHA]
ERE	“Explanation, Reduction and Empiricism”, see FEYERABEND 1962b
FFY1	<i>Feyerabend's Formative Years. Volume 1. Feyerabend and Popper. Correspondence and Unpublished Papers</i> , see COLLODEL–OBERHEIM (eds.) 2020
FFY3	<i>Feyerabend's Formative Years. Volume 3. Feyerabend Among Popperians. Correspondence and Unpublished Papers</i> , see COLLODEL–OBERHEIM (eds.) forthcoming
H	Handwritten (or mostly handwritten) document
<sup>HF</sup>	Feigl's annotation
HF	Herbert Feigl Papers, Archives and Special Collections, Elmer L. Andersen Library, University of Minnesota
IEUS	International Encyclopedia of Unified Science
IHA	<i>Internationale Hochschulwochen Alpbach des Österreichischen College</i> [Alpbach International Summer Seminar of the Austrian College Society; known after 1949 as EFA]
IL	Imre Lakatos Collection, <i>LSE Library Archives and Special Collections</i>
JW	John W. N. Watkins Collection, <i>LSE Library Archives and Special Collections</i>
KP	Karl Popper Papers, Hoover Institution Library & Archives, Stanford University; and <i>Karl Popper-Sammlung, Universitätsbibliothek der Alpen-Adria-Universität Klagenfurt</i>
MCPS	Minnesota Center for Philosophy of Science

MSPS	Minnesota Studies in the Philosophy of Science (book series)
NSU	<i>Nordisk Sommeruniversitet</i>
ÖC	<i>Österreichisches College</i> [Austrian College Society]
PM	Postmark
PC	Postcard
PF	<i>Sammlung Paul Feyerabend, Philosophisches Archiv der Universität Konstanz</i>
SHQP	Sources for History of Quantum Physics
SSR	<i>The Structure of Scientific Revolutions</i> , see KUHN 1962
R	<i>Folium rectum</i> (front side of a sheet of paper)
RC	Rudolf Carnap Papers, Archive of Scientific Philosophy
T	Typed (or mostly typed) document
T/H	Typed document with substantial handwritten annotations
TK	Kuhn's annotation

# Chapter 1

## Introduction

Matteo Collodel and Eric Oberheim

One of the most fascinating features of Paul Feyerabend's formative years was his direct involvement, both intellectually and personally, in several major movements in the history of twentieth century philosophy of science; most notably the rise of Karl Popper's critical rationalism, the demise of Rudolf Carnap's logical empiricism, and the 'historical turn' primarily attributed to Thomas Kuhn. The materials collected in this volume contain detailed discussions and analyses of specific arguments and assumptions underpinning these movements. They provide fresh insights into Feyerabend's personal relations to many prominent individuals and their evolving institutional roles,<sup>1</sup> as well as into Feyerabend's philosophical breakthrough at the turn of the 1960s.<sup>2</sup> The intellectual exchanges to which these materials testify jointly contributed to shaping Feyerabend's methodological argument for theoretical pluralism, initially developed in his controversial "Explanation, Reduction and Empiricism" (FEYERABEND 1962b, *abbr.* *ERE*). In this landmark essay, Feyerabend drew together his earlier work on Wittgenstein and formal logic, the foundations of quantum theory, and normative methodology, developing his idea of

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<sup>1</sup> Indeed, as we shall see, a striking feature of Feyerabend's formative years is the personal interactions Feyerabend had with prominent individuals whose ideas directly shaped specific aspects of his developing views, such as Popper, Bohr, Wittgenstein, Carnap, Hempel, Bohm, and Kuhn.

<sup>2</sup> Understanding Feyerabend's ideas and the different roles that he played in these (and other) movements has been severely hindered by a notorious lack of reliable resources. For example, Carnap and Popper made almost no mention of their acquaintance with Feyerabend in their published philosophical or autobiographical papers (cf. CARNAP 1963; POPPER 1976). Scholars attempting to clarify and compare their mutual influences and to untangle and evaluate their views have been left for the most part with Feyerabend's side of these stories. Moreover, for a variety of reasons, Feyerabend's autobiographical reflections do not always provide an ideal basis for interpreting and assessing how his work developed and for understanding his various roles in these movements. See COLLODEL 2016.

'incommensurability', which laid the foundation for his views on science, knowledge and pluralism that gradually morphed into *Against Method* (FEYERABEND 1975; 1988; 1993).

The first part of this volume contains unpublished papers, discussions and correspondences directly pertaining to the development of Feyerabend's criticism of logical empiricism, including previously unpublished papers written by Feyerabend and detailed discussions of them by several prominent logical empiricists, such as Carnap and Carl Hempel. The second part contains Feyerabend's autobiographical reflections about David Bohm, and a previously unpublished lengthy response by Bohm to Feyerabend's review of Bohm's *Causality and Chance in Modern Physics* (BOHM 1957a).<sup>3</sup> The third part collects Feyerabend's correspondence with Thomas Kuhn, including detailed comments and criticisms that he made on a draft of Kuhn's *The Structure of Scientific Revolutions* (KUHN 1962, *abbr. SSR*). Bohm and Kuhn both made significant contributions to the development of Feyerabend's 1962 criticism of contemporary empiricism.

Taken together and against the background of Feyerabend's relationship to Popper presented in the first volume, the materials collected in this volume provide first-hand depictions of significant developments within these movements as well as succinct summaries of some of Feyerabend's specific criticisms of them. Because Feyerabend often expressed himself much more clearly and directly in his correspondence than in his sometimes rather notoriously opaque academic publications, the autobiographical remarks they contain contribute to a much clearer picture of his own development than the rather unclear image that has emerged based solely on third-person reconstructions that have had to rely on his published papers. By offering crisper and more direct formulations of some of Feyerabend's complex ideas and how they developed, these materials are of great help for understanding and situating those ideas in their proper context, allowing for a deeper and more accurate view, not only of the intellectual and personal sides of Feyerabend's relations, but also of the history of twentieth century philosophy of science more generally.

To facilitate access to, and appreciation of, the materials collected, and how they shed new light on Feyerabend's formative years, the next three sub-sections introduce Feyerabend's developing relationships to logical empiricism (1.1), Bohm (1.2), and then Kuhn (1.3) emulating the structure of the collection. Section (1.4) describes the collection providing specific information about the materials, their origins, and how they have been ordered. Section (1.5) specifies the editorial policies and conventions used to edit and present the material, which is followed by our acknowledgments to the many people and institutions that helped make this project possible.

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<sup>3</sup> See FEYERABEND 1960b.

## 1.1 Feyerabend and Logical Empiricism

Feyerabend's critical interest in logical empiricism developed in the context of post-war Vienna, under the direct supervision of Viktor Kraft, who was Feyerabend's doctoral advisor and the last remaining member of the Vienna circle in Vienna after the war. Feyerabend's 1951 doctoral dissertation "On the Theory of Basic Sentences"<sup>4</sup> as well as his 1951 public lecture "The Dogmas of Logical Empiricism"<sup>5</sup> (Ch. 2) illustrate just how deeply Feyerabend was immersed in the Vienna circle tradition in the early 1950s. They also show that Feyerabend had already been influenced by both Popper and Willard Quine's contemporaneous criticisms of empiricism.<sup>6</sup> In the 1950s, Feyerabend developed ideas on meaning and methodology that would eventually become central to his criticism of contemporary empiricism in *ERE*, which sets out his methodological argument for theoretical pluralism based on the idea that theories separated by a scientific revolution are incommensurables, and concludes that a formal, 'objective' account of explanation cannot be given, because the meaning of a sentence is always relative to framework used to interpret it.<sup>7</sup>

The materials collected in Part I show how Feyerabend developed his infamous criticism of contemporary empiricism in *ERE* 1962 through a series of critical engagements with logical empiricism in the 1950s. This illustrates how Feyerabend developed some of the specific targets of his criticism, and to whom he was trying to explain 'how to be a good empiricist' (FEYERABEND 1963a). As we shall see, initially, in the 1950s, Feyerabend's focus was on Carnap, who had resumed his earlier position as logical empiricism's leading representative in the post-war period. By the early 1960's, Feyerabend's targets also included Hempel and Ernst Nagel. As he developed intellectually, Feyerabend also personally came to know many philosophers gravitating around logical empiricism, such as Béla von Juhos, Philipp Frank, Arthur Pap (for whom he worked as an assistant from 1953–54), and through him, Herbert Feigl. Feigl was director of the Minnesota Center for Philosophy of Science (*abbr.* MCPS), which was the institutional headquarters of logical empiricism in North America. The MCSP supported the work of leading logical empiricists, such as Carnap, Hempel and Nagel. Feyerabend kept in regular contact with Feigl, who was trying to help Feyerabend find an academic position. In 1955, Feigl was looking forward to hosting an extended visit by Feyerabend at the MCPS:

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<sup>4</sup>Feyerabend's 1951 dissertation, *Zur Theorie der Basisätze*, has not been translated into English. Cf. FEYERABEND 1951.

<sup>5</sup>Cf. "Die Dogmen des logischen Empirismus", in STADLER (ed.) 2010. English translation this volume, Part 1, Ch. 2.

<sup>6</sup>Popper tried to develop his views explicitly in contrast to logical empiricism and had coined the term 'Basissätze' (translated into English as 'basic statements') in contrast to the logical empiricist idea of 'observation statements' because Popper "needed a term not burdened with the connotation of a perception statement" (POPPER 1959a: 12fn\*2). Popper's influence on Feyerabend is clearly already discernible in the first title 'On the Theory of Basic Sentences', whereas the second title is clearly a play on Quine's "Two Dogmas of Empiricism" (QUINE 1951a).

<sup>7</sup>See FEYERABEND 1962b: 95.

“I have been reading some of Feyerabend’s papers and find most of them very good indeed. It will be a pleasure to have him here for some period next year. I shall write him soon”.<sup>8</sup> Feyerabend must have made a very good first impression (which also seems to have had been the case with both Popper and Wittgenstein) as just six weeks later, Feyerabend reported to Popper that “Prof. Feigl told me that there may be a possibility for me staying at his institute for another two years. Here you have all those exciting possibilities of travelling, learning, etc. etc.”.<sup>9</sup> Feyerabend even entertained the possibility of settling down in the U.S. for the long term, explaining to Popper that Feigl “is a realist and this is the most important thing in our ‘idealistic’ time. I am certain that I could stay in the U.S. for more than a year and that there is even a possibility of settling down there, if I am not a complete fool, which happens sometimes (and you know of it)”.<sup>10</sup>

Throughout this period, Feyerabend displayed a polemical attitude towards the views of the leaders of logical empiricism, both in his published papers and in his contributions to the epistolary discussion groups organized by the MCPS under Feigl’s management. Feyerabend’s personal ties with the logical empiricist network further strengthened and consolidated in the summer of 1957, after Feyerabend’s participation in a six-week conference on the philosophical foundations of physics at Feigl’s institute. This paved the way to the offer of a visiting appointment for 1958–59, which was extended to Feyerabend by the University of California, Berkeley in early 1958, and which he eagerly accepted. In October of 1959, on a leave of absence from Berkeley, Feyerabend returned to the MCPS to visit Feigl. Feyerabend found his stay very stimulating in comparison to his lack of colleagues in Berkely (he had not yet met Kuhn): “Whereas in Berkeley there was nobody at all with whom I could discuss here there are at least two people, Herbert Feigl and Grover Maxwell.”<sup>11</sup> During these early years of his American experience, Feyerabend’s frequent visits to Minneapolis continued to be a fundamental part of boosting his philosophical breakthrough.<sup>12</sup>

From the very start, Carnap’s deep and lasting positive influence on Feyerabend resulted from his understanding of Carnap’s 1930’s distinction between the occasion of the production of the sentences and its subsequent theoretical interpretation as a statement according to some theory, which is the main idea behind ‘objective’ theories of observation sentences.<sup>13</sup> Feyerabend adopted this early Carnapian distinction to try to criticize Carnap’s later account of scientific language, and to explain his own view, which he initially called a “theory of reactors” (FEYERABEND 1951). This initial presentation was gradually refined into Feyerabend’s ‘causal’ or

<sup>8</sup> Cf. Feigl to Pap, 15 February 1955, HF 03-134-33.

<sup>9</sup> COLLODEL-OBERHEIM (eds.) 2020 (*abbr. FFYI*), Feyerabend to Popper, 19 March 1955-8: 165.

<sup>10</sup> *FFYI*, Feyerabend to Popper, 19 March 1955-8: 166. As Feyerabend took up his first academic post as lecturer in the philosophy of science at Bristol in the fall of 1955, he did not visit the MCPS until 1957.

<sup>11</sup> Cf. *FFYI*, Feyerabend to Popper, 26 October 1959-12: 328.

<sup>12</sup> Cf. FEYERABEND 1966a; 1978; 1995.

<sup>13</sup> Cf. CARNAP 1929; 1936; 1937.

‘pragmatic’ theory of observation sentences complemented by his contextual theory of meaning in *ERE* 1962. This account allows one observation sentence to make two or more different statements depending on which theory is used to interpret it. This is how incommensurable theories, according to Feyerabend, give incompatible meanings to the same observation sentences. For example, according to Feyerabend, ‘The ball fell’ was understood to make at least two incompatible statements that imply two incompatible processes: that it was pushed by its inner impetus or pulled by an external force.<sup>14</sup>

After his initial engagement with Carnap in his 1951 Ph.D. thesis, a second engagement with Carnap was made possible thanks to Herbert Feigl. By circulating drafts and collecting responses, Feigl facilitated Feyerabend’s participation in lively discussions with members of the newly founded MCPS, who were engaging in internal, epistolary debates on Carnap’s 1956 criterion of empirical meaningfulness, and on Carnap’s explication of the structure of scientific theories more generally. This resulted in two distinct controversies (see *below*, Part I, Ch. 3). Feyerabend explicitly stressed the similarity between his own views and Carnap’s physicalism in the 1930s (and Popper and Neurath’s views).<sup>15</sup> In this way, Feyerabend tried to use ideas from Carnap’s earlier pre-war work to criticize the logical empiricist tradition that had grown out of it in the US after the war, which had largely abandoned realism as metaphysical speculation. As the materials collected in Part I are directly relevant to these developments, the following subsections will focus more closely on each of them in turn.

*The Kraft circle and Feyerabend’s theory of basic sentences (1951)*. As mentioned, Feyerabend’s first visible engagement with logical empiricism and Carnap’s work occurred during Feyerabend’s university studies in post-war Vienna, which can be seen in his 1951 dissertation, ‘On the Theory of Basic Sentences’. Feyerabend’s investigation reviews the pre-war Vienna Circle’s protocol sentence debate. It was developed through thorough discussions at meetings of the Kraft Circle<sup>16</sup>, also known today as “the Third Vienna Circle” (Stadler 2006). Feyerabend was the leader of this small circle of STEM students (science, technology, engineering, and mathematics), which included Rudolf Goldberger de Buda (1924–1988), Peter Schiske (1924–2012), Heinrich K. Eichhorn (1927–1999), Hans Sagan (1928–2000), and Erich Jantsch (1929–1980). They were trying to revive the spirit of the Vienna Circle, but this time under the patronage of Feyerabend’s supervisor, Victor Kraft, who was a former senior but peripheral member of the Vienna Circle, and the successor of Mach, Boltzmann, and Schlick’s Chair at the University of Vienna.<sup>17</sup> Most of the members of this study group went on to have brilliant academic careers in

<sup>14</sup> See FEYERABEND 1962b: 56–57.

<sup>15</sup> See FEYERABEND 1962b: 34f.

<sup>16</sup> See FEYERABEND 1995a: p. 85.

<sup>17</sup> Against the backdrop of the forced migration of the adherents of the scientific worldview, an unlikely, transient revival of scientific philosophy occurred during the allied occupation from 1946 to 1955. Cf. KUBY 2010; SCHORNER 2010; STADLER (ed.) 2010.



North America in astronomy, physics or mathematics. Feyerabend also managed a brilliant academic career, but along a very different path.

During this period, on various occasions in Vienna and Alpbach, in events promoted by para-academic institutions such the *Österreichisches College* [Austrian College Society] or the *Institut für Wissenschaft und Kunst* [Institute for Science and Fine Arts], Feyerabend had the chance to meet and interact intensively and repeatedly with acclaimed physicists and distinguished philosophers of science such as Erwin Schrödinger (1949, 1953, 1955),<sup>18</sup> Léon Rosenfeld (1949),<sup>19</sup> Maurice Pryce (1949),<sup>20</sup> Philipp Frank (1950, 1955),<sup>21</sup> and Karl Popper (1948).<sup>22</sup> This afforded him the opportunity to establish himself as a philosopher in the foundations of quantum theory within academic circles on both sides of the Atlantic, virtually overnight in the second half of the 1950s.

In his doctoral thesis, Feyerabend examined what came to be known as the protocol sentence debate from two perspectives (formal and an empirical).<sup>23</sup> By

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<sup>18</sup>Feyerabend first met Schrödinger at the 1949 *Europäisches Forum Alpbach* (abbr. EFA), cf. *FFYI*, Feyerabend to Popper, 1 November 1948-2: 64n3. They also met at the 1953 EFA, cf. *FFYI*, Feyerabend to Popper, 10 September 1953-4: 72, and n2. On Feyerabend and Schrödinger at the EFA, see HACOHEM 2000: 314–315; KUBY 2016. In 1955, Feyerabend, seeking a recommendation, asked Popper to contact Schrödinger on his behalf. Popper replied: “I shall of course write to Schrödinger if you want me to but it is infinitely preferable that you should write to him yourself. I know that he remembers you, and it is a general rule that people prefer to be approached directly by the person most immediately concerned, rather than through an intermediary” (*FFYI*, Popper to Feyerabend, 1 April 1955-11: 173). On Feyerabend and Schrödinger, cf. *FFYI*: 5–8, 28n24; Ch. 3: 55; Feyerabend to Popper, 1 November 1948-2: 64 and 64n3; Feyerabend to Popper, 10 September 1953-4: 72, especially 72n2, 73; Feyerabend to Popper, 28 September 1953-5: 76–77; Feyerabend to Popper, 8 October 1953-6: 79n3, 80, 80n5; Feyerabend to Popper, 1953-7: 82; Feyerabend to Popper, 31 October 1953-9: 89, 89n4; Feyerabend to Popper, 2 October 1954-18: 132; 19. Feyerabend to Popper, 3 October 1954-19: 140; Feyerabend to Popper, 19 March 1955-8: 166; Feyerabend to Popper, 25 March 1955-10: 170; Feyerabend to Popper, 1 April 1955-11: 173; Feyerabend to Popper, May 1955-13: 175; Feyerabend to Popper, 27 May 1955-16: 178; Feyerabend to Popper, 28 October 1955-29: 201; Feyerabend to Popper, 1956-4: 216; Feyerabend to Popper, 11 February 1956-10: 223; Popper to Feyerabend, 11 November 1958-13: 300, 300n6; Feyerabend to Popper, 18 November 1958-14: 302; Ch. 7: 476; 478-47. See *below*, Ch. 2, Feyerabend to Feigl, 28 Juhn 1957-2: n14; Feyerabend to Kuhn, 1961-3.

<sup>19</sup>On Feyerabend and Rosenfeld, cf. *FFYI*, Ch. 2: 30; Feyerabend to Popper, 1 November 1948-2: 64n3; Feyerabend to Popper, 14 January 1957-1: 246; Feyerabend to Popper, 27 March 1957-4: 253; Feyerabend to Popper, 30 March 1957-5: 256; Feyerabend to Popper, 7 April 1957-7: 260; and see *below*, Ch. 3, 1959-3; 1961-4; Feyerabend to Peat, 7 September 1993-2; 1993-3; Feyerabend to Kuhn, 1961-3: especially n13.

<sup>20</sup>On Feyerabend and Pryce, cf. *FFYI*, Feyerabend to Popper, 1 November 1948-2: 64n3; Feyerabend to Popper, 14 October 1955-27: 196n2; especially Feyerabend to Popper, 28 October 1955-29: 199n1; Feyerabend to Popper, 6 February 1956-7: 219; Feyerabend to Popper, 11 February 1956-10: 223; Feyerabend to Popper, March 1956-14: 228n4; Feyerabend to Popper, 7 April 1957-7: 261. See *below*, Feyerabend to Peat, 7 September 1993-2.

<sup>21</sup>On Feyerabend and Frank, cf. *FFYI*, Feyerabend to Popper, 24 December 1953-15: 97n5; Feyerabend to Popper, 3 October 1954-19: 134; Feyerabend to Popper, 28 August 1955-25: 191n2.

<sup>22</sup>Cf. *FFYI*, Ch. 1: 2.

<sup>23</sup>On the protocol sentence debate, see UEBEL 2007.

drawing from research in the psychology of perception and elaborating on the development of logical empiricism in northern Europe,<sup>24</sup> the first part of the work develops a critique of ‘subjective theories of basic sentences’, that is, approaches attempting to ground basic sentences in “the [immediately] given”. Specifically, Feyerabend discusses ‘sense data’ and Schlick’s ‘*Konstatierungen*’. In the second part of his dissertation, Feyerabend groups various accounts under the heading of ‘objective theories of basic sentences’, which includes Carnap’s and Neurath’s physicalism, and which is where he situates his own proposal of a “theory of reactors” [*Theorie der Reaktoren*]. Feyerabend argues that while subjective theories correctly focus on the production of basic sentences, they give the wrong account of their epistemological function, because they ground the meaning of basic sentences in their phenomenological occurrence (in the cause or appearance of the observation). By contrast, Feyerabend takes his theory of reactors to give an entirely empirical theory of the production of basic sentences, whose meaning is only then established in a separate, subsequent act of interpretation, so that what a sentence means will depend on which theory is used to interpret it. Anticipating his realism and pluralism to come, Feyerabend also criticizes Carnap’s assertions that physicalism implies a reduction of “qualitative sciences” (*Qualitätswissenschaften*) to physics. He objects to this purported implication of physicalism by stressing the autonomous “ontological status” (*Realwert*) of each discipline’s theories.

‘*On the Interpretation of Scientific Theories*’ (1960). Feyerabend’s second engagement with Carnap took place in the mid-1950s. This time, Feyerabend published criticisms of Carnap’s more recent proposals. Specifically, between 1954 and 1958, based on the ideas developed in his doctoral thesis, Feyerabend tried to find fault with Carnap’s evolving double language model for explicating scientific knowledge. A brief and somewhat opaque critique of Carnap’s (1939) model appeared in (FEYERABEND 1954e, §4) and was reworked in more detail in a German publication (FEYERABEND 1955b).<sup>25</sup> The paper examines the function and form of ‘coordinating definitions’ understood as semantic rules in Carnap’s model, and once again, Feyerabend argues that this leads to absurd consequences. Instead, according to Feyerabend, the meaning of central terms used within a theoretical framework is holistically defined by the system itself, while observational terms used to test any such system can only be characterized causally, as utterances produced under certain conditions, which then can be variously interpreted depending on which theory is used for that purpose.

In the summer of 1955, Feyerabend’s arguments were discussed by Arthur Pap and Michael Scriven at the MCPS. Their critical remarks, highlighting misunderstandings, confusions and obscurities in Feyerabend’s paper, brought a temporary end to a controversy that in all likelihood never even reached Carnap’s attention (see *below*, Ch. 3, Sections 1–3). The following year, however, Carnap’s proposal of an updated version of his model, complemented by a novel criterion of empirical

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<sup>24</sup> Cf. MANNINEN–STADLER 2010 (eds.).

<sup>25</sup> Cf. CARNAP 1939.

meaningfulness (CARNAP 1956) sparked a new and lively rebuke from Feyerabend. At the turn of 1957, Feyerabend tried to expose what in his view were “obvious defects” of Carnap’s proposal in a short ‘Critical Note’ (see *below*, Ch. 3), which he sent to the prestigious journal *Mind* for publication as well as to Feigl, who circulated it among the MCPS’s affiliates. This triggered an intense debate between Feyerabend, on one side, and Carnap, Hempel and William Rozeboom, on the other, with Feigl in the middle, editing and forwarding the cross-communications to the participants in the debate. Feyerabend’s insistent critique of Carnap’s views unfolded throughout the first half of 1957 and totaled nine items including all the replies and rejoinders. Eventually, Feyerabend acknowledged his misrepresentation of Carnap’s views and withdrew his note for consideration by *Mind*, thus terminating the second controversy (see *below*, Ch. 3, Sections 4–13). Yet, shortly before admitting defeat, Feyerabend resumed the first controversy with a new ‘Critical Note’, which was intended, among other things, to clarify the opaqueness of FEYERABEND 1955b and to provide a reply to the criticisms that it had received at the MCPS two years earlier. By the late spring of 1958, Feyerabend developed this ‘Critical Note’ into his “On the Interpretation of Scientific Theories” (FEYERABEND 1960b), which was delivered to Feigl shortly after its completion, possibly as early as September 1958, and then circulated by Feigl to members of the MCPS group. This time, however, Feyerabend’s attempt to criticize Carnap’s approach was not honoured with an answer either from Carnap or from Hempel. Rather, the job of dealing with it was left to Rozeboom, who by early November 1958 offered his critical remarks in a memorandum responding to Feyerabend, unfailingly followed by Feyerabend’s reply, which finally brought the first controversy to a close (see *below*, Ch. 3, Sections 14–16).

In general, Feyerabend’s qualms revolved around the very idea of “partial interpretation”, central to Carnap’s reconstruction of scientific theories in the double language model. More specifically, Feyerabend emphasized, once again, that in Carnap’s model, any improvement or change in the means of observation that affects the empirical testing of a theory would be mirrored in the systematic reconstruction, via correspondence rules, of the relation between the partially interpreted calculus formulated in the theoretical language, on the one hand, and experience described in the fully interpreted observation language on the other. Such improvements would thereby also result in a change of the theory. In other words, according to Feyerabend, Carnap’s view allegedly implies that, for example, building better microscopes would change the meaning of the theories about the cells they are used to observe.<sup>26</sup> Such a paradoxical consequence was meant to undermine a “positivistic”, instrumentalist-leaning and conservative understanding of scientific theories, and to promote indirectly their “realistic” and progressive understanding, according to which theoretical language is meaningful independently of the casual mode of its production. For Feyerabend meanings are not fixed by experience. They change

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<sup>26</sup>Feyerabend makes the same point when he argues against formal and ‘objective’ accounts of explanation more generally in ERE. Cf. FEYERABEND 1962b: 93–95.

with experience as theories about what they describe and explain improve, which was the view that Feyerabend defended throughout his formative years.

These controversies with Carnap, who was irritated by Feyerabend's confrontational attitude were detrimental to Feyerabend's reputation among Feigl's MCPS group. As a result, before his first visit to the US in mid-August 1957, and despite Feigl's benevolent mentorship, Feyerabend's name was already notorious in logical empiricist quarters for his harshly critical boldness and seemingly opinionated arrogance. However, after Feyerabend took up a position at the University of California, Berkeley in the fall of 1958, both Carnap and Hempel had a chance to meet Feyerabend in person, at which point they re-evaluated their initial negative impressions that had resulted from their epistolary exchanges. When Carnap became personally acquainted with Feyerabend in the spring break of 1959, during a lecture tour that brought Feyerabend to southern California, Carnap immediately found Feyerabend a congenial, easy-going and stimulating discussion partner in the philosophy of physics. Carnap invited Feyerabend's critical comments on a projected work on entropy, which Carnap had drafted in 1952–54 as two separate papers (and which was only posthumously published as CARNAP 1977). Feyerabend and Carnap met again in February 1960, during a small and intense three-day restricted conference in Santa Monica sponsored by the MCPS that included Feigl, Paul Meehl and Grover Maxwell. It seems likely that on that occasion they discussed the objections that Feyerabend and Maxwell had raised against Carnap's views (and logical empiricism more generally) with Carnap himself.

*"Explanation, Reduction and Empiricism"* (1962). In the early 1960s, Feyerabend once again redeployed the basic Carnapian distinction underlying Feyerabend's earlier physicalist 'theory of reactors' (1951). This time Feyerabend developed an account of observational meaning that combines the critical rationalist maxim to try to increase testability with what he took to be insights he attributed to Wittgenstein's contextual theory of meaning. In *ERE*, Feyerabend again traces his 'pragmatic' or 'causal' theory of observation to the earlier protocol sentence debate, and in particular, to the papers in which Carnap himself, under pressure from Neurath, had come to accept the advantages of a physicalist language for describing sentences used to test theories. Feyerabend argues that later developments within logical empiricism, as exemplified by Carnap's own change in views, represent a step back, relative to the progress that the protocol sentence debate had achieved towards an 'objective' or 'causal', or 'empirical' theory of observation sentences.<sup>27</sup> In this way, Feyerabend uses early pre-war logical empiricism to criticize "modern" or "contemporary" post-war logical empiricism. Unsurprisingly, therefore, *ERE* also resumes Feyerabend's attack on Carnap's "well-known double-language model".<sup>28</sup> According to Feyerabend, Carnap's

tacit withdrawal from the pragmatic theory of observation is one of the most surprising features of modern empiricism. It is responsible for the fact that this philosophy, despite the

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<sup>27</sup> See FEYERABEND 1962b: 35.

<sup>28</sup> FEYERABEND 1962b: 41.

apparent progress that has been made since the thirties, is still in accordance with the assumption that observational meanings are invariant with respect to the process of explanation. (1962b: 42)

According to Feyerabend, Hempel's account of explanation, Nagel's account of reduction and Popper's empiricism share this same undesirable feature as Carnap's double-language model.<sup>29</sup> They all imply meaning invariance of the observation language, when in fact such meanings do and should change through scientific revolutions as better theories replace their conceptually incompatible predecessors.

Throughout the first half of the 1960s, Feyerabend continued to highlight what he took to be two erroneous underlying assumptions of contemporary empiricist accounts — the principles of deducibility and of meaning invariance — finding them historically inaccurate, as well as methodologically undesirable, because their conceptual conservatism would stultify scientific progress, not promote it. By reconstructing textbook cases of scientific progress, Feyerabend proposed his theoretical pluralism as a more descriptively apt and methodologically desirable strategy for promoting progress. In the process, he argued that the conceptual frameworks that shape experience are replaced, not corrected and explained, by a universal theory that offers a better corroborated, but conceptually incompatible interpretation of reality.<sup>30</sup>

While Feyerabend's idea of incommensurability can be seen to echo some aspects of aspects of Carnap's distinction between external and internal questions (CARNAP 1950), such similarities should be regarded as a case of independent and superficial convergence, rather than the result of historical influence. Although some ideas in Carnap's work may seem to resonate with some of Feyerabend's views, one should keep in mind that Carnap and Feyerabend had deeply different approaches to science, language, and philosophy.<sup>31</sup> Carnap's "logic of science" resulted from and is justified by his analysis of scientific language, whereas Feyerabend's approach resulted from and is justified by his analysis of scientific method. In his early philosophy, Feyerabend argued for his methodological views about science ultimately on ethical grounds. For Feyerabend, science is what we make it and what we make of it. Conversely, Feyerabend's historical turn, which begins in the mid-to-late 1960s, was certainly partly inspired by, and deeply resonates with, Neurath and Frank's "behavioristics of scholars", both in its attention to pragmatic factors affecting scientific practice and in its wariness of formalization as a privileged tool of philosophical inquiry.

It is possible that Feyerabend and Carnap met only once after the 1962 conference in Santa Monica, at the 1964 European Forum Alpbach, together with Feigl, among others. In any case, they remained on friendly terms until Carnap's death, as evidenced by Carnap's letter of recommendation in December 1966, which was instrumental to Feyerabend's appointment at the Free University of Berlin in 1968

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<sup>29</sup>On Feyerabend's criticism of Popper's empiricism, see OBERHEIM 2024, OBERHEIM and HOYNINGEN-HUENE 2024.

<sup>30</sup>See FEYERABEND 1962b: 92–95

<sup>31</sup>See especially OBERDAN 1990.

(see *below*, Ch. 2, Section 11). Feyerabend's developing views, in contrast to Carnap's, can be exemplified by his re-appraisal of Carnap's 'Principle of Tolerance'. While denouncing it as sterile in one context, Feyerabend would also group Carnap among the "splendid exceptions" to the "much more rigid attitude" of most "meta-scientists" and would judge Carnap's 'Principle of Tolerance' as being "pretty close to my own anarchism" (FEYERABEND 1977: 362). Though a careful reader of Carnap, it seems that Feyerabend, like many of his contemporaries, had not initially properly understood Carnap's project of linguistic engineering centred around proposals of formally improved conceptual tools that could eventually prove fruitful for progress in scientific research. An alternative, more radical reading suggests that Feyerabend's critique of rational reconstruction was based on the belief that the Carnapian formal approach wouldn't prove fruitful for promoting progress because of meaning variance, and because it was too far-removed from the messiness and informal nature of scientific concept-building. In any case, Feyerabend seems initially to have missed Carnap's emphasis on the possibility of producing wholly new concepts, rather than just refining inherited old ones.

In sum, despite Feyerabend's close personal proximity to leading logical empiricists, Feyerabend was highly critical of logical empiricism's main tenets. Carnap's double language model of logical reconstruction of scientific theories was his main targets in the 1950s,<sup>32</sup> to which he added Hempel's theory of explanation, Nagel's theory of reduction and Popper's empiricism, which were subjected to repeated attacks in the 1960s.<sup>33</sup> The climax of Feyerabend's repeated assaults on logical empiricism was reached with the idea of incommensurability as set out in *ERE*, which is considered by some as "[t]he most influential critique of Logical Positivism's analysis of scientific observation".<sup>34</sup> Notwithstanding a consolidated tradition of studies in the origins and development of logical empiricism, the peculiar role of Feyerabend as both a sort of insider and as a critic of the movement has so far failed to be satisfactorily analysed in all of its historical and theoretical details.<sup>35</sup> Whether this might be partly due to the radical theses Feyerabend appeared to be championing or to the bad reputation his increasingly anti-academic personality gave him, it seems clear that the lack of better documented evidence as to Feyerabend's relations to logical empiricism has constrained the understanding of a crucial period of twentieth century philosophy of science.

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<sup>32</sup> Cf. FEYERABEND 1954e; 1955b; 1958a, CARNAP 1936; 1937; 1938; 1939; 1956.

<sup>33</sup> Cf. FEYERABEND 1962b; 1963a; 1965a; 1965b; 1965c; HEMPEL-OPPENHEIM 1948; NAGEL 1949; 1961; OBERHEIM 2024.

<sup>34</sup> OBERDAN 1990: 25.

<sup>35</sup> Noteworthy, though rather dated, exceptions on the theoretical side are HARRÉ 1959; BUTTS 1966; TOWNSEND 1971; and OBERDAN 1990. STADLER-FISCHER (eds.) 2006 mark a turning point on the historical side.



## 1.2 Feyerabend, Bohm and the Foundations of Quantum Theory

David Bohm (1917–1992) was an American-British quantum physicist and philosopher of Jewish descent.<sup>36</sup> He was born in Wilkes-Barre, Pennsylvania and attended Pennsylvania State University, graduating with a B.S. in 1939. He began his graduate work in physics at the California Institute of Technology, then transferred to the University of California at Berkeley, to work with J. Robert Oppenheimer.

Although Oppenheimer, who was Bohm's mentor and doctoral supervisor at the time, wanted Bohm and his work on scattering to be part of the Manhattan project at Los Alamos, because Bohm was deemed a security risk (a close colleague was potentially a spy), he was initially denied security clearance. His research on scattering toward his thesis was promptly classified, and so he lost access to, and was forbidden to discuss, his own work. Oppenheimer arranged for conferral of his degree without an official submission and defence in 1943. Bohm did eventually contribute directly to the Manhattan project. His calculations at the Y-12 facility in Oak Ridge, Tennessee were used to enrich the uranium for the bomb dropped on Hiroshima in 1945.

In 1947, Bohm became Assistant Professor at Princeton University, where he worked closely with Albert Einstein who became his new mentor. In 1951, Bohm was suspended for refusing to collaborate with the House Un-American Activities Committee. He was arrested and charged for having invoked his fifth amendment right against self-incrimination, based on a McCarthy-era law that was later to be declared unconstitutional by the Supreme Court. Oppenheimer advised Bohm to leave the country. With letters of recommendation from Oppenheimer and Einstein, Bohm joined the University of São Paulo, Brazil (1951–55), and then moved to the Technion in Haifa, Israel (1955–57) before becoming Research Associate at the University of Bristol (1957–61), where he and Feyerabend briefly became close colleagues in 1957, before Feyerabend left for Berkeley in 1958. Bohm then became Professor of Theoretical Physics at Birkbeck College, University of London (1961–83) and Feyerabend left for Berkeley in 1962.<sup>37</sup>

Bohm made a wide range of significant contributions to physics. He established the foundations of plasma theory. He made major advances in quantum theory and relativity theory, and he tried to develop a revolutionary new conceptual framework based on what they have in common. Bohm's contributions to philosophy are also of the highest order and span an equally impressive range.<sup>38</sup> While he is most widely known for his notorious 1952 (deterministic) hidden-variables proposal, his general

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<sup>36</sup>For biographical information on Bohm, cf. PEAT 1997.

<sup>37</sup>On Feyerabend, Kuhn and Berkeley, see *below*, section 1.3.

<sup>38</sup>The wide range of interconnected philosophical proposals that Bohm considered has often been emphasized: "His philosophical reflections ranged from the role of causality and chance in modern physics to the role of creativity and dialogue to enhance human sociability on our planet and included reflections on consciousness" (FREIRE JUNIOR 2019: vii).

methodology and its development in the 1950s and early 1960s, has received considerably far less attention.<sup>39</sup> His philosophy of mind<sup>40</sup> and his general philosophy promoted by the Dalai Lama (which includes political philosophy and practical philosophy, as for example, his views on conflict resolution and the potential to improve the human condition through what has been dubbed ‘Bohm dialogues’)<sup>41</sup> have taken on somewhat of a cult status in some circles. All his diverse ideas in and about science and philosophy were developed as part of an attempt to establish a revolutionarily new world based on a more humane worldview. In Bohm’s terms, these ideas were an attempt to form an undivided wholeness characteristic of the qualitative infinity of nature itself.<sup>42</sup>

In the late 1940s, under Popper’s influence,<sup>43</sup> Feyerabend began to develop his interest in the foundations of quantum theory. To this end, from 1949 to 1952, Feyerabend visited either Sweden or Denmark every year. These trips gave him the opportunity to study trends in scientific philosophy in those countries, and he “got

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<sup>39</sup>For Bohm’s methodological views in the 1950s, based on the idea of infinite levels, cf. BOHM 1957a. For Bohm’s methodological views in the early 1960s, based on the qualitative infinity of nature, cf. *below*, Ch. 8; and BOHM 1961; 1962; 1963a; 1964. For Bohm’s methodological views in the late 1960s, cf. BOHM 1966: Ch. 25 “The Falsification of Theories”: 94–99; BOHM 1968 (on science and art); BOHM 1969 (on science as perception). On Bohm’s methodological views in the early 1970s and his search for a new order in physics, cf. BOHM 1971a; 1971b; 1972; 1974. On Bohm’s methodological views in the late 1970s and the 1980s, see Bohm on wholeness, fragmentation and the implicate order in BOHM 1976. Cf. BOHM 1977; 1978; 1980; 1985; 1986a; 1986b; 1986c; 1987a; Bohm 1988; BOHM-HILEY 1985; BOHM-HILEY 1993; KRISHNAMURTI–BOHM 1985.

<sup>40</sup>For Bohm’s philosophy of mind, cf. BOHM 1976; 1986; 1987a; 1990.

<sup>41</sup>Cf. BOHM-FACTOR-GARRETT 1991; BOHM 1996.

<sup>42</sup>According to Bohm, “What is very probably needed instead is a qualitatively new theory, from which both relativity and quantum theory are to be derived as abstractions, approximations and limiting cases. The basic notions of this new theory evidently cannot be found by beginning with those features in which relativity and quantum theory stand in direct contradiction. The best place to begin is with what they have basically in common. This is undivided wholeness. Though each comes to such wholeness in a different way, it is clear that it is this to which they are both fundamentally pointing. To begin with undivided wholeness means, however, that we must drop the mechanistic order” (BOHM 1980: 223).

<sup>43</sup>Feyerabend met Popper in 1948 at the EFA and they quickly became close, with Popper taking on the role of mentor, and even father figure. See especially COLLODEL 2016. On Feyerabend and the EFA, see *FFYI*, Ch. 1: 25–26n22. Before 1949, the EFA was known as the *Internationale Hochschulwochen* (abbr. IHA) *des Österreichischen College* (abbr. ÖC) [Alpbach International Summer Seminar of the Austrian College Society]; cf. *FFYI*, Ch. 1: 25–26n22. The IHA was an interdisciplinary, international conference that has been held annually in the Tyrolean village of Alpbach since 1945. The event usually lasted approximately three weeks and took place in the late summer. The first International Summer Seminar was organized in Alpbach under the sponsorship of the French occupying forces by Simon Moser and Otto Molden. Its unforeseen success encouraged Otto and Fritz Molden to establish the ÖC. On Feyerabend and the IHA and the ÖC, see *FFYI*, Ch. 1: 25–26n22 and 26–27n23; Feyerabend to Popper 8 October 1953–6: 78n1 and 80n5. Feyerabend participated in the IHA/EFA as secretary and shorthand writer in 1948 and 1949, and probably also from 1950–52. He also later co-led seminars at the event in 1956 see *FFYI*: Ch.1: 26–27n23.



several valuable ideas for [his] own subject".<sup>44</sup> More specifically, in 1949, Feyerabend travelled to Denmark on a scholarship from the Danish Ministry of Education. A year later, he travelled to Sweden on a scholarship from the Swedish Student's Association, and then in 1951, he returned to Denmark through an invitation to the first "Nordisk Sommeruniversitet" (Scandinavian Summer University) in Askov, from the 11–26 of August on the general theme 'The causality problem'. This provided him with the opportunity to interact directly with Bohr and his collaborators.<sup>45</sup> In this way, as he had done earlier with Wittgenstein, Feyerabend went straight to the source of the issues in which he was interested. This led to his participation in an event that shaped his work in the foundations of quantum theory for the better part of the next two decades:<sup>46</sup> Bohr and the Bohrians' initial reaction to Bohm's 1952 "hidden variables" proposal.<sup>47</sup>

As Feyerabend recounts it, he had approached Bohr directly after Bohr had given a talk, to ask for details:

Bohr repeated his old explanations [...], but with renewed vigour, for he had just heard of David Bohm's apostasy.<sup>48</sup> 'Can you understand that?' he asked with a puzzled look on his face. Unfortunately, he was soon dragged away to another meeting. (FEYERABEND 1995: 78)

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<sup>44</sup> *FFYI*; Ch. 3, §352. Feyerabend may be referring to his ideas on meaning change, cf. FEYERABEND 1995:78. Feyerabend also actively attended the second *Nordisk Sommeruniversitet* [Scandinavian Summer University (*abbr.* NSU)] (general theme: "*Mennske og miljø*" [People and the Environment]), which took place in the Norwegian village of Ustaoset in the summer of 1952. Cf. *FFYI*, Ch. 3, §4: p. 54. The NSU is an ongoing institution that was founded in August 1950 with the aim of organizing a cultural event modelled after the EFA. See Hermansen 2000, <<http://www.nsuweb.net/wb/>>. On Feyerabend and the NSU, see *FFYI*: Ch. 2, n37; and *below*, Part 2, Ch. 6; Peat 1997: 129.

<sup>45</sup> *Nordisk Sommeruniversitet*.

<sup>46</sup> Cf. FEYERABEND 1961c; 1962a; 1968;1969.

<sup>47</sup> See BOHM 1952a; 1952b. Feyerabend first met Niels Bohr at the 1951 NSU, which was in Askov (Denmark), 11–26 August 1951, on the general theme 'The causality problem'. For background and details, see *FFYI*: Ch. 2: 30n37. On Feyerabend and Bohm, see *FFYI*, Ch. 2: 32n41.

<sup>48</sup> On Feyerabend and Bohm, see *FFYI*, Ch. 2: 32n41; Feyerabend to Popper, 16 May 1954-13: 124; 2 October 1954-18: 133n.4; 19 March 1955-8: 165; 7 April 1957-7: 260; 15 December 1957-13: 271-272; 1958-2: 275; 28 January 1958-3: 277; 12 March 1958-4: 279; 1 May 1958-5: 280; 7 June 1958-8: 286; 22 March 1959-5: 314; 26 October 1959-12: 328–239; 29 April 1961-6: 361; 17 August 1961-9: 369n18; 4 October 1967-2: 419-420n4; Ch. 7 "Philosophy and the Physicists (1955)": 479; Ch. 8 "Observationally Complete Theories: Some Observations on Quantum Theory (1958)": 490; and see *below*; Feyerabend to Feigl 1957-2; June 1957; "Report on the conference on philosophical foundations of physics", September 27, 1957; "Research Project on the Logic of Measurement, Probability and Confirmation, with Applications in Atomic and Quantum Theory"; Feyerabend to Hempel, Rec. 10-25-61; Part 2, Ch. 7 and 8; Part 3, Feyerabend to Kuhn 1961-2; Feyerabend to Kuhn 1961-3. Cf. *FFY3*, Feyerabend to Agassi 1956-5: 1957-9; 12 March 1958-5.(3): 1958-7.(5); 2 April 1958-8; 1958-9.(5); 1958-12; 22 June 1958-13; 2 November 1958-17; 1967-4; 25 November 1967-15; 30 November 1967-16; 20 July 1968-16; Feyerabend to Watkins 6 September 1960-2; 17 December 1967-9; 7 January 1968-2; 17 April 1968-8; 1971-1; Feyerabend to Lakatos 9 March 1967-4; Feyerabend to Musgrave, 25 July 1967, IL 14/10 (2 ss., H) in *FFY3*, Feyerabend to Lakatos 13 July 1967-17. Cf. FEYERABEND 1995: 78, 88, 92.

To Feyerabend, it “seemed that, for [Bohr], the sky was falling down”.<sup>49</sup> According to Feyerabend, Bohr was “neither dismissive, nor shaken. He was amazed”.<sup>50</sup> After Bohr left the room, the discussion continued. Some of those present argued that the objections to Bohm’s theory were not at all conclusive. As Feyerabend put it:

The Bohrians for a while tried to close the holes in the Bohrian fashion. When they did not succeed they said: ‘But von Neumann has proved ...’ – and that ended the discussion. Nobody said another word though I had the impression that none of the Bohrians would have been able to explain what it was that von Neumann had proved and how he had achieved it.<sup>51</sup> It seems that Bohr himself never used von Neumann as a crutch. (Ch. 7, Feyerabend to Peat, 30 August 1993–4)

From the moment that he learned of it from Bohr, Feyerabend became keenly interested in Bohm’s attempts to develop a ‘hidden variables’ interpretation of quantum theory in the face of von Neumann’s *alleged* proof that no deterministic completion by ‘hidden variables’ can be consistent with the principles of quantum mechanics.<sup>52</sup>

In November 1951, directly after obtaining his Ph.D. in philosophy in Vienna, Feyerabend applied for and was eventually awarded a British Council scholarship to study the foundations of quantum theory in London for the academic years 1952–53, under Popper’s supervision.<sup>53</sup> Von Neumann’s proof and the corresponding measurement problem became major topics of discussion between Feyerabend and Popper throughout the 1950s and early 1960s, and a main focus of Feyerabend’s publications on the foundations of quantum theory through to the late 1960s. In the early 1950s, prominent physicists began publishing responses to Bohm’s 1952 proposal. Pauli went first, promptly responding to Bohm in 1952, rehearsing his earlier

<sup>49</sup> See *below*, Feyerabend to Peat, 1993, note that ‘falling down’ was changed to ‘falling in’ by Peat.

<sup>50</sup> Feyerabend was responding to Peat’s suggestions, see PEAT 1997:129. See *below*, Part 2, Ch. 7.

<sup>51</sup> Feyerabend is referring to the purported proof of the impossibility of hidden variable interpretations of quantum mechanics that was proposed by the Hungarian-American mathematician and polymath John von Neumann (1903–1957) at the beginning of the 1930s. See NEUMANN 1932: 2–3 [ix–x], pp. 109 [209–210], Ch. 4, § 1–2. Although the proof was shown to be flawed by the German mathematician and philosopher Grete Hermann (1901–1984) in 1935 (see HERMANN 1935), it was largely—though not unanimously—considered sound for the following three decades, until its flaw was rediscovered by the British physicist John Stewart Bell (1928–1990) in the mid-1960s (see BELL 1966). On von Neumann’s proof, see JAMMER 1974: Ch. 7; CARUANA 1995; GATTEI-AGASSI 2016. On Feyerabend and Neumann’s proof, see *FFYI*, Ch. 2, 30–31n37; Feyerabend to Popper, 28 October 1955–29: 201; 1955–33: 206–207; 23 December 1955–34: 208; 18 January 1956–2: 213; 18 January 1956–3: 214; 30 January 1956–5: 217; 4 February 1956–6: 218; 8 February 1956–9: 221; 11 February 1956–10: 225–226; 5 April 1956–15: 230; 14 April 1956–16: 231; Popper to Feyerabend, 16 April 1956–17: 232; 18 April 1956–18: 234; Feyerabend to Popper, 9 May 1956–19: 235; 14 January 1957–1: 244–245; 21 January 1957–2: 248–250; 28 January 1957–3: 252; 27 March 1957–4: 253–254; 30 March 1957–5: 257; Popper to Feyerabend, 1 April 1958–6: 259; Feyerabend to Popper, 1965–4: 412; FEYERABEND 1995: 92–93, 106, 108.

<sup>52</sup> Cf. BOHM 1952a; 1952b; NEUMANN 1932.

<sup>53</sup> See COLLODEL 2016.

objections against Louis de Broglie's earlier pilot wave proposal.<sup>54</sup> Einstein published a criticism of Bohm's theory a year later, which he regarded to be essentially the same as Pauli's earlier criticism of de Broglie,<sup>55</sup> and Heisenberg published papers explaining his reasons for being unconvinced by Bohm's proposal in 1955, and then again in 1958.<sup>56</sup>

Once in London, Feyerabend attended Popper's seminar and he concentrated on two topics: foundations of quantum theory (von Neumann's proof and its relation to Bohm's proposal), and Wittgenstein's *Investigations*.<sup>57</sup> According to Feyerabend's recollections, "Von Neumann was not easy; I worked my way through his book, page by page, and eventually wrote a critical review".<sup>58</sup> At that time, he did the same with Wittgenstein's *Investigations*.<sup>59</sup>

Popper, like Bohm, was also a vocal critic of Bohr's 'official view'.<sup>60</sup> Popper was lecturing that: "the official view – backed by a proof of von Neumann's – was that the theory was complete in the sense that any attempt to introduce so-called 'hidden variables' – for example, values of the momentum and position which are more precise than the principle of indeterminacy permits – must lead to contradictions".<sup>61</sup> As Popper saw it: "David Bohm has recently published some papers which, it appears, refute the official view in this point. These papers were not very well received".<sup>62</sup> Popper was very clear that he thought that Bohm had provided: "A calculation which in [his] opinion is a striking refutation of the complementarity view".<sup>63</sup> At least since the late 1940s, Popper had been defending "Indeterminacy in Quantum-Mechanics and in Classical Physics",<sup>64</sup> and was in the early stages of

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<sup>54</sup> Cf. DE BROGLIE 1927; 1928. On early criticisms of Bohm's theory, see MYRVOLD 2003. Bohm had been *unaware* of de Broglie's pilot-wave proposal when independently 'rediscovered' in 1952. De Broglie's pilot-wave model led to Schrödinger's formulation of wave mechanics, after which the pilot-wave model was abandoned in favour of the quantum formalism.

<sup>55</sup> See Einstein 1953. Basically, Einstein criticized Bohm's proposal because it was inelegant (lacking certain symmetries), to which Bohm (who accepted Einstein's criticism) responded that it was just supposed to be a pilot pilot-wave theory, in the sense that it was not proposed as a final theory, but just as a proof of concept, implying that a more satisfactorily developed pilot-wave theory would recover those symmetries. Cf. MYRVOLD 2003.

<sup>56</sup> Cf. HEISENBERG 1955; 1958.

<sup>57</sup> Cf. WITTGENSTEIN (1953); FEYERABEND 1956: [46–48]; FEYERABEND 1954a; 1954b; 1955a. Feyerabend had a copy of the manuscript that became the *Investigations* from Elizabeth Anscombe, see COLLODEL 2016.

<sup>58</sup> FEYERABEND 1995: 92–93. Feyerabend is referring to his review of von Neumann's book *Mathematical Foundations of Quantum Mechanics* (NEUMANN 1932), see FEYERABEND 1958d: [294–298].

<sup>59</sup> See FEYERABEND 1954a; 1954b; 1955a.

<sup>60</sup> For Popper on Bohm, see *FFYI*, Ch. 6 "A Defence of Free Thinking in Quantum Theory (1953)": 459, 472n33, n34, n47; 474n26E; Popper to Feyerabend, 5 May 1958-6: 281.

<sup>61</sup> Popper in *FFYI*, Ch. 6 "A Defence of Free Thinking in Quantum Theory (1953)": 459.

<sup>62</sup> Popper in *FFYI*, Ch. 6 "A Defence of Free Thinking in Quantum Theory (1953)": 459.

<sup>63</sup> Cf. Popper in *FFYI*, Ch. 6 "A Defence of Free Thinking in Quantum Theory (1953)": 472n33.

<sup>64</sup> Cf. POPPER 1950a; 1950b.

developing his own realistic, indeterministic propensity interpretation of quantum theory.<sup>65</sup> Popper was praising Bohm:

*most of the younger physicists have been brought up to believe that [...] all that matters us (is) the formalism and its application. An exception is David Bohm who wrote an elementary textbook of the theory which was unorthodox in its unparalleled attempt to think out matters in detail in physical terms. (It is hardly an accident that he left the camp of orthodoxy not very long after the publication of this enchanting book.) (Popper in FFYI, Ch. 6 "A Defence of Free Thinking in Quantum Theory (1953)": 468)*

In this way in 1953, Popper seems to have been contributing to the myth that von Neumann's proof had purportedly demonstrated that any hidden variables account is impossible until Bohm had shown otherwise.<sup>66</sup> However, as Feyerabend points out in his first publication on quantum theory published a year later in 1954, von Neumann's proof is addressed to a specific class of hidden variable theories of which Bohm's was not one.<sup>67</sup> On this, both von Neumann and Bohm agreed.<sup>68</sup> De Broglie had confessed that he did not initially realize the limited domain of validity of von Neumann's theorem and had overstated its generality.<sup>69</sup> In any case, this seems to have contributed to the general misunderstanding, Popper's included. Feyerabend clearly seems to have thought so: "In this way von Neumann's book has created a good deal of confusion among physicists (cf. e.g., de Broglie's statement in the Introduction to *La Physique quantique restera-t-elle indéterministe?*) and, naturally, among philosophers,"<sup>70</sup> as it seems likely that this last remark was meant to include Popper.<sup>71</sup>

In this period, Feyerabend was trying to develop his own criticism of Bohm's 1952 'hidden variables' interpretation. By 1954, Feyerabend was circulating drafts of his own criticism of Bohm's proposal: "I am sending two articles I wrote. [...]. You [Popper] will find the presentation and criticism of Bohm's interpretation on the last three pages".<sup>72</sup> Feyerabend was encouraged by Margenau, who thought Feyerabend's criticism of Bohm's 1952 'hidden variables' proposal was similar to Pauli's objection: "He [Margenau] had got hold of my article on Quantum Mechanics

<sup>65</sup> Cf. Popper's contribution to the 1957 Colston conference, POPPER 1957.

<sup>66</sup> De Broglie seems to have inadvertently started this myth with respect to von Neumann's alleged proof, which he later admitted, and tried to correct. See DE BROGLIE 1953.

<sup>67</sup> See FEYERABEND 1954c: [25–45].

<sup>68</sup> Cf. MYRVOLD 2003.

<sup>69</sup> See DIEKS 2017: 30n42.

<sup>70</sup> FEYERABEND 1958d: [298], italics inserted. Cf. DE BROGLIE 1953.

<sup>71</sup> Feyerabend explicitly pointed out that "Bohr himself did not commit this mistake" (FEYERABEND 1958d: [298n]).

<sup>72</sup> FFYI, Feyerabend to Popper, 2 October 1954-18: 133. Cf. FFYI, Feyerabend to Popper, 6 May 1954: 124. The first article to which Feyerabend is referring could be one of several possibilities: Either "*Der Beobachtung Prozess in der Quantenmechanik*" [The Observation Process in Quantum Mechanics], KP 423.5 (23 ss., T/H), or a draft of FEYERABEND1954c: [25–45], or a draft of FEYERABEND 1954d. Cf. FFYI, Feyerabend to Popper, 1954-17, 129n3. The second article is "Some Remarks concerning Bohm's Quantum-Mechanics in terms of 'Hidden' Variables", KP 423.4 (10 ss., T/H). Cf. "David Bohm's 'Quantum Mechanics'", HF 08-28-04 (28 ss., T/H).

and seemed impressed because, as emerged from his letter, Pauli had proffered similar objections against Bohm”.<sup>73</sup> Unknowingly foreshadowing his own proposed solution to the measurement problem, Feyerabend wrote to Popper: “I think that Bohr’s solution [to the EPR paradox] is essentially the correct solution if we do not interpret quantum-mechanics in a conservative way.”<sup>74</sup> But Feyerabend’s attempts to criticize Bohm’s 1952 proposal were soon superseded by Bohm and Vigier’s new 1954 ‘hidden variables’ proposal in their “Model of the Causal Interpretation of Quantum Theory in Terms of a Fluid with Irregular Fluctuations” (BOHM-VIGIER 1954).<sup>75</sup> This is how Brownian motion took centre stage in discussions of the foundations of quantum theory. According to Bohm and Vigier, there are two kinds of Brownian motion. Classical Brownian motion was explained by Einstein as a stochastic process, when the predictions he deduced from the kinetic theory were confirmed by Perrin. In this way, Einstein showed that Brownian motion results from shaking atoms pushing particles that are small enough around.

According to Bohm and Vigier, the second kind of Brownian motion is the indeterminacy of the motion of electrons. Electrons behave like Brownian particles because they are bombarded stochastically from all sides by a surrounding sub-quantum ‘fluid’ in which they are suspended. In other words, just as atoms cause stochastic motions on the classical level, an underlying sub-quantum ‘fluid’ causes stochastic motion of electrons on the quantum level. In this way, Bohm and Vigier were proposing a causal theory that explains the confirmed results that had been predicted by quantum mechanics, but as the result of a lower-level process – and not as the result of some purported irreducible indeterminacy implied by quantum theory. The existing theory was supposed to be a limiting case of their proposal in the way that classical mechanics is related to relativity theory.

In October of 1955, Feyerabend began the next big step in his work on the foundations of quantum theory, when he committed to lecture on it in Bristol. He wrote to Popper, “I have announced a special lecture for next term the preliminary

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<sup>73</sup> *FFYI*, Feyerabend to Popper, 19 March 1955: 165. Cf. “I am sending an article which I wrote when I returned from Alpbach and which I would like to have published in your journal in case you think it good enough” (*FFYI*, Feyerabend to Popper 2 October 1954: 133). Feyerabend may be referring to his “David Bohm’s ‘Quantum Mechanics’”, HF 08-28-04 (28 ss., T/H). The journal he is referring to is *The British Journal for the Philosophy of Science*. Cf. *FFYI*, Popper to Feyerabend 2 January 1954-1: 99–100n2. However, there are some discrepancies to be noticed concerning the identification of this paper with the article Feyerabend is referring to here. In particular, no copy of the long paper mentioned above can be found in the Popper Collection. Moreover, the copy that Feyerabend sent to Herbert Feigl only a short time later (cf. *FFYI*, Feyerabend to Popper 3 October 1954-19: 135–135n4) is annotated with the hand-written remark: “The first article I wrote in English. The English is therefore not very good”. However, Feyerabend had already written a much shorter paper in English on the same topic, which he judged to be too long to be published in *The British Journal for the Philosophy of Science*. Cf. *FFYI*, Feyerabend to Popper, 16 May 1954-13: 124.

<sup>74</sup> *FFYI*, Feyerabend to Popper, 1954: 204.

<sup>75</sup> Cf. BOHM-VIGIER 1954.

program of which I am enclosing".<sup>76</sup> Topics to be discussed specifically included the 'Quantum-theory of measurement' and de Broglie's theory and Bohm's 1952 interpretation.<sup>77</sup> Specifically, Feyerabend was to present Bohm's response to von Neumann's proof, as well as the objections raised to von Neumann's proof by Grete Hermann, as well as Einstein's criticism of Bohm's views and Bohm's response to Einstein.<sup>78</sup> Feyerabend then began his lectures on the foundations of quantum physics in Bristol in early 1956. Physicists regularly attended including Pryce (who was soon to become Bohm's boss in the next year), with whom Feyerabend openly sparred during the lectures.<sup>79</sup> This must have felt like a heavy lift for Feyerabend, who had switched to Popper's point of view with respect to von Neumann's proof at the last minute:

I gave my first lecture to the physicists, I was very nervous (there were about 60 people), Prof. Pryce interrupted me several times, twice I gave in although I was right– but on the main point I convinced people. And this main point was that it does not follow from any presentation of the uncertainty-rel. that atomic particles cannot have sharp position + sharp velocity at the same time! For since I had left you your insistence on this point set my mind working with the result that I have changed most of what I am going to lecture in favour of your point of view. This I did about 3 days before the lectures started and I found thinking along your lines most exciting + fruitful. I discussed at length Neumann's proof. Do we conclude, so I asked, that a throw with a die gives a blurred result? The theory of Brownian movement leads again to classes which obey (1) and (2) [not sharp and pure] – nevertheless we say that Brownian particles have sharp p and q at any time of their movement. This led to a long argument between Pryce and myself by which I finally convinced him that there are no reasons whatever to believe in the queer nature of electrons. I.e. I did not actually convince him, but left him less dogmatic [...] And this is certainly desirable. (FFYI, Feyerabend to Popper, 11 February 1956-10: 223)

This seems to be the first instance that Brownian motion appears in Feyerabend's writings, clearly following Bohm and Vigier's (1954) suggestion in the foundations of quantum theory. However, Brownian motion as part of a general methodological argument for pluralism only clearly enters Feyerabend's published work after the Colston conference in 1957.

Shortly after Feyerabend switched to Popper's point of view on von Neumann's proof and the idea that there is no *special* problem of quantum indeterminacy, he published "*Eine Bemerkung zum Neumannschen Beweis*" [A Remark on von Neumann's Proof] (FEYERABEND 1956).<sup>80</sup> Popper immediately privately accused Feyerabend of plagiarism,<sup>81</sup> insisting Feyerabend write the editors and have them add a note giving himself and Agassi credit for the short paper. Feyerabend initially

<sup>76</sup>Cf. FFYI, Feyerabend to Popper, 14 October 1955-27: 196.

<sup>77</sup>Cf. FFYI, Feyerabend to Popper, 28 October 1955-29: 200–201.

<sup>78</sup>Cf. FFYI, Feyerabend to Popper, 28 October 1955-29: 200–201; 6 February 1956-7: 219.

<sup>79</sup>Pryce and other physicists regularly attended Feyerabend's lectures. Cf. FFYI, Feyerabend to Popper, 6 February 1956-7: 219; 11 February 1956-10: 223.

<sup>80</sup>Cf. FEYERABEND 1956. English translation "A Remark on von Neumann's Proof (1956)": [46–48].

<sup>81</sup>See COLLODEL 2016.