ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2025, t. 22, nr 2**

Philosophical Aspects of Origin s. 17–390

https://doi.org/10.53763/fag.2025.22.2.256

ARTYKUŁ ORYGINALNY / ORIGINAL ARTICLE

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On the Limited Validity of Falsificationism

Feyerabend's Theoretical Pluralism and its Relation to Popper, Wittgenstein and Bohm

Received: April 18, 2025. Accepted: March 10, 2026. Published online: July 13, 2026.

Abstract: This essay develops Feyerabend's early philosophy of science as intended as a criticism of Karl Popper's falsificationism and Ludwig Wittgenstein's "quietism" in "Explanation, Reduction and Empiricism" [ERE (1962)]. It illustrates how Wittgenstein and David Bohm played central roles in the genesis of ERE (1962) as Feyerabend's attempt to step out from under Popper's shadow by criticizing his normative methodology. Part 1 shows how Feyerabend's criticism of Popper was an attempted synthesis of Popper and Wittgenstein's views with respect to meaning and methodology that was also intended to mediate Popper's famous "Poker dispute" with Wittgenstein on the nature of genuinely philosophical problems. Specifically, Feyerabend argued that falsificationism cannot take testing incommensurable rival universal theories empirically into account, and so it has only a limited validity. It is limited to testing commensurable theories. Part 2 explains how this set the stage for Feyerabend's infamous slogan "Anything goes!" — according to which all methodological rules can have only a limited validity. Feyerabend's dispute with Popper's school on methodology began in 1960 on a draft of ERE (1962). Seven years later, Feyerabend announced his break with Popper's school with respect to methodology, which was sparked when Feyerabend awoke from his Popperian slumber on the 17th of December 1967, while watching Lakatos lecture falsificationism to his students. Feyerabend, who had gradually changed how he taught falsificationism over the

Keywords:

Anything goes;
David Bohm;
Falsificationism;
Incommensurability;
Karl R. Popper;
Ludwig Wittgenstein;
Meaning change;
Paul K. Feyerabend;
Theoretical pluralism

years, realized just how far from Popper's methodology he thought he had come. Falsificationism has only a limited validity and even *ad hoc* hypotheses can have a legitimate role in progress. In letters to Watkins and Lakatos, he explained that he was leaving Popper's school because of the dispute about Popper's methodology, which began with his 1960 draft of ERE (1962) according to which Popper's methodology (falsificationism) shares a dogmatic element with school philosophies so that it impedes the progress it purports to promote. Part two also explains how and why Feyerabend awoke from his Popperian slumber in methodology into a living nightmare. Popper (and Popperians) misunderstood Feyerabend's idea of incommensurability as Popperstein's baby, just as Popper misunderstood Feyerabend's arguments against his methodology as a "Lessing Effect". Moreover, Feyerabend's idea of incommensurability in ERE (1962) continues to be misrepresented by some Popperian interpretations of Feyerabend's theoretical pluralism today. Part 3 of the essay shows how Feyerabend and Bohm were working closely together and why ERE (1962) should be seen as the fruits of that collaboration. Feyerabend directed and developed his views on incommensurability as a criticism of falsificationism while discussing these issues directly with Bohm, who was also trying to criticize what he took to be falsificationism in the same way, by limiting its validity to testing within frameworks. Bohm's attempts to criticize Popper's methodology clearly anticipate Feyerabend's criticism of Popper's methodology in ERE (1962) in many further significant respects. Bohm was using the Brownian motion example to defend his hidden variables proposal, which some had dismissed as pernicious ad hoc metaphysics and Feyerabend developed his test-model to show how incommensurable theories can be compared by crucial experiments despite meaning variance based on Bohm's Brownian motion example and to support his methodological argument for theoretical pluralism: Because incommensurable alternatives can increase the empirical content of their rivals, they should always be welcome (even if they are *ad hoc*). In ERE (1962), Feyerabend developed his theoretical pluralism in contrast to and as part of a criticism of Popper's methodology following Bohm's lead and in support of Bohm's quest to explain the results of quantum theory as a second kind of Brownian motion. In this way, Feyerabend's views on methodology as set out in ERE (1962) were shaped by two formative experiences: the ad hoc dogmatic dismissal of Ehrenhaft's effects in the late 1940s, and the dogmatic dismissal of Bohm's ad hoc hidden variables proposal in the late 1950s.

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Introduction

Paul Feyerabend's philosophy of science has always been controversial. His views on science and method and methodology are notoriously complicated as is the story of how those views developed. This essay revisits Feyerabend's relation to his mentor Karl Popper and explains Feyerabend's idea of incommensurability in his *locus classicus* "Explanation, Reduction and Empiricism" (1962) as part of a criticism of falsificationism. [1] The essay illustrates Ludwig Wittgenstein and David Bohm's central roles in the genesis of ERE (1962) as an attempt to come out from under Popper's shadow by breaking free from Popper's falsificationism. Feyerabend's break with Popper should be understood in the context of Feyerabend's relationship to Bohm and Feyerabend's criticisms of both of his two philosophical idols' (Wittgenstein and Popper's) most prized achievements. In 1962, Feyerabend criticized Popper's methodology (falsificationism) and Wittgenstein's philosophy (as "quietism"), both for the same reason. They are both anti-humanitarian because they prevent progress instead of promoting it. In a double philosophical patricide, Feyerabend tried to combine the best from both, while criticizing the worst in each. Specifically, he combined the normative critical rationalist principle that striving to increase testability is the best way to promote progress with what he took to be Wittgenstein's contextual theory of meaning as a criticism of Popper's methodology limiting its validity. Feyerabend argued that Wittgenstein's dictum that "meaning is use" should be replaced by Feyerabend's own dictum that "meaning should be correct use". Moreover, Feyerabend's criticism of Popper as an attempt to synthesize Popper and Wittgenstein's views with respect to methodology and meaning was also an attempt to mediate Popper's famous "piping hot Poker dispute" with Wittgenstein on the nature of philosophical problems, which Feyerabend tried to explain in ERE (1962) as the result of antiquated onto-

[1] See Paul K. Feyerabend, "Explanation, Reduction and Empiricism", in: Herbert Feigl and Grover Maxwell (eds.), **Scientific Explanation, Space and Time**, *Minnesota Studies in the Philosophy of Science*, Vol. III, University of Minnesota Press, Minneapolis 1962, pp. 28–97. The 1981 republication in Feyerabend's collected papers was significantly revised, see Paul K. Feyerabend, **Realism, Rationalism and Scientific Method. Philosophical Papers, Volume 1,** Cambridge University Press, Cambridge 1981, pp. 44–96. See John Preston, **Feyerabend: Philosophy, Science and Society,** Polity Press, Cambridge 1997, and Matteo Collodel, "Was Feyerabend a Popperian? Methodological Issues in the History of the Philosophy of Science", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 27–56.

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logical principles that continue to haunt everyday language because we use the same terms to mean different things.

In ERE (1962), Feyerabend argued that Popper's falsificationism cannot take testing competing universal theories into account because they are incommensurable, or "deductively disjoint" due to meaning variance in the terms used to state them, so that the new theory cannot explain the successful predictions of its incommensurable rival, (nor can predictions deduced from the new theory logically contradict predictions deduced from the existing theory), as required for a falsification. This is precisely how Feyerabend's theoretical pluralism was developed as a criticism of falsificationism that limits its validity to testing commensurable theories.

In his publications, Feyerabend was a staunch supporter of falsificationism right until ERE (1962). Feyerabend's criticism of Popper's methodology was the fundamental factor in Feyerabend's deteriorating relationship with Popper in the 1960s, as Feyerabend tried to develop his own peculiar form of theoretical pluralism explicitly in contrast to Popper's falsificationism. However, Feyerabend failed to make his criticism of falsificationism clear to Popper, who initially mistook Feyerabend's attempt to criticize falsificationism as a somewhat muddled attempt to describe and promote Popper's own views, perhaps even Popper's own idea of "inkommensurabilität". [2] In the long run, Popper mistook Feyerabend's suggestion to imply that universal "framework" theories are *untestable*, when in fact Feyerabend had argued that they are testable, just not by falsificationism. John Watkins, (a major member of the Popperian school) had explicitly understood that ERE (1962) was supposed to be a criticism of falsificationism from the start, as can be seen in his correspondence with Feyerabend on an early draft of ERE (1962) in 1960. However, Watkins was also explicitly unable to understand Feyerabend's views on meaning and methodology, and explicitly made it clear that he did not understand Feyerabend's criticism of falsificationism. Contemporary philosophers continue to misunderstand Feyerabend's theoretical pluralism as an extension of falsificationism, instead of a view developed in contrast to and as part of a criticism of falsificationism. In this way, they continue to misrepresent Feyerabend's relation to Popper, his philosophy, and the Popperian school, which they

[2] As we shall see, Popper used the term "inkommensurabilität" in **Logik der Forschung**, which was translated into English in **The Logic of Scientific Discovery** as "non-comparable", which led to a number of misunderstandings.

characterize as an attempt to promote and extend falsificationism, when in fact it was indisputably an attempt to criticize it, limiting it validity to cases of cumulative progress in contrast to progress through scientific revolutions in which entire worldviews are replaced. Put more directly, the misunderstanding of Feyerabend as a proponent of Popper's falsificationism began in the early 1960s, and eventually resulted in a Popperian misunderstanding of Feyerabend's incommensurability as somehow following Popper's own "inkommensurabilität" as an extension of Popper's methodology. The result was "Popperstein's baby": [3] Popperian misunderstandings of Feyerabend's idea of incommensurability and its relation to falsificationism. This is how Feyerabend's use of "incommensurability" was born in 1960 (as a criticism of falsificationism limiting its validity to testing commensurable theories), and why seven years later in 1967, Feyerabend broke with Popper's school, having awoken from his Popperian slumber, only to find that he was lost and alone in what had become a living nightmare. Moreover, as we shall see, this is exactly how and when Feyerabend arrived as his hedonistic attitude in methodology: "Anything goes!".

Part 3 of this essay shows that Feyerabend's criticism of falsificationism in ERE (1962) was directly following Bohm's 1959 criticism of falsificationism. More specifically, Feyerabend's criticism of falsificationism was based on Bohm's understanding of the methodological implications of Einstein's prediction of the stochastic character of Brownian motion, as part of Bohm's plea for more tolerance in matters epistemological, in support of Bohm's 1954 quantum Brownian motion proposal. Bohm's 1954 proposal offered a causal, realistic alternative to the standard interpretation of quantum mechanics that by 1957 was empirically indistinguishable from it. (The same successful predictions could be deduced from both theories). Bohm's views were rejected because they seemed to be reactionary by reintroducing metaphysics into quantum theory. However, from Feyerabend's perspective, Bohm was a progressive, who was ostracized from the community unfairly because of philosophical prejudice, not for legitimate scientific reasons.

Feyerabend had been developing his ideas on the topic of how theories can be tested empirically given that *ad hoc* maneuvers can always be used to protect

[3] The name "Popperstein's baby" is intended to allude both to Frankenstein (as a scary hybrid) and Rosemary's baby as the result, for Feyerabend, was a horrifying nightmare, in which he felt lost and alone. See Roman Polański movie, **Rosemary's Baby** from 1968, which is a psychological horror movie based on the novel with the same name by Ira Levin (1967).

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them from direct refutation ever since his experience with Ehrenhaft and his unexplained experimental effects in the late 1940s. Throughout the 1950s, he was developing his ideas about incommensurability. This set the stage for his relationship to Bohm and his ideas, which led directly to the application of Feyerabend's use of the term "incommensurable" (following Einstein's use of that term) as a criticism of Popper's methodology in ERE (1962). Feyerabend met Bohm in 1957, shortly after reading Bohm's book **Causality and Chance in Modern Physics** at the infamous 1957 Colston conference that Feyerabend had helped to organize. At the conference, Feyerabend witnessed first-hand and up-close, how Bohm's proposal was unfairly dismissed, and how Bohm defended himself with his own dialectical methodology, and the Brownian motion example, in support of Bohm's plea for more tolerance in matters epistemological in defense of his and Vigier's 1954 hidden variables proposal. At the conference, Feyerabend floated his own views on methodology and meaning change, and Bohm supported these views. A few months later, Feyerabend and Bohm began directly discussing methodology and the foundations of quantum theory at the University of Bristol. The significant overlap in their views is readily apparent in their work in philosophy of science in 1959 and the early 1960s. ERE (1962) reads like a professional philosopher's rendition of Bohm's 1959 attempt to criticize falsificationism and Bohm's first foray into a professional philosophy of science journal ("On the Relationship between Methodology in Scientific Research and the Content of Scientific Knowledge") in 1961 based on Bohm's Brownian motion example used to illustrate Bohm's defense of his hidden variables proposal and Bohm's plea for more tolerance in matters epistemological. At the turn of 1959, Feyerabend was writing a lengthy critical review of Bohm's 1957 book that was published in 1960. Bohm read a draft of Feyerabend's critical review in 1959 and wrote a soon to be published, [4] 38-page response to it, developing an extensive critical comparison of his own methodological views (he called "the qualitative infinity of nature") with falsificationism. Feyerabend's criticism of falsificationism in ERE (1962) was an attempt to develop and sharpen Bohm's own criticism of falsificationism. Feyerabend was following Bohm's suggestion that falsificationism *can* only have a "lim-

[4] See Matteo Collodel and Eric Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical Empiricism, Bohm and Kuhn**, *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Berlin 2024, stands for **Feyerabend's Formative Years**, see below. The page breaks have been removes and minor mistakes have been corrected without being identified in the citations from the correspondences for the purpose of readability.

ited validity", [5] because it *cannot* take important instances of confirmation into account, by developing Bohm's Brownian motion example to show how there can be crucial experiments between incommensurable rivals, without establishing formal relations between their corresponding predictions. Bohm had not read Popper's **The Logik**, and so his criticism of Popper is understandably somewhat off-the mark in comparison to Feyerabend's criticism of Popper's methodology, which is developed by revealing a tension between Hempel's account of explanation and Popper's methodology. Feyerabend, however, was intimately familiar with Popper's methodology and so pinpointed the problem with an immanent criticism of it. Popper's methodology implies that revolutionary new theories explain the predictions deduced from their incommensurable rivals. However, formal relations cannot be established between observation statements when they are used to test rival universal incommensurable theories because of meaning variance in the terms used to state the theories that determines the meaning of the observation statements used to test them given that they must be deduced from them to be used as test of them. In this way, according to Feyerabend, falsification of a universal theory by an incommensurable rival is impossible due to meaning variance. Feyerabend develops this into a criticism of Popper's normative methodology, and an argument for theoretical pluralism according to which

[5] The expression "limited validity" is adopted and translated from Feyerabend's German "*Von der beschrankten Gültigkeit methodologischer Regeln* [On the Limited Validity of Methodological Rules]" (Paul K. Feyerabend, "Von der beschrankten Gültigkeit methodologischer Regeln", in: Rüdiger Bubner, Konrad Cramer, and Reiner Wiehl (eds.) **Dialog als Methode**, *Neue Hefte für Philosophie* 1972, Vol. 2–3, pp. 124–171), English translation Paul K. Feyerabend, "On the Limited Validity of Methodological Rules", in: Paul K. Feyerabend, **Philosophical Papers. Volume 3. Knowledge, Science and Relativism**, John Preston (ed.), Cambridge University Press, Cambridge 1999, Ch. 7. See also Paul K. Feyerabend, "Against Method: Outline of an Anarchistic Theory of Knowledge", in: Michael Radner and Stephen Winokur (eds.), **Analysis of Theories and Methods of Physics and Psychology**, *Minnesota Studies in the Philosophy of Science*, Vol. 4, University of Minnesota Press, Minneapolis 1970, pp. 17–130, which morphed into **Against Method** (Paul K. Feyerabend, **Against Method: Outline of an Anarchistic Theory of Knowledge**, New Left Books, London and Humanities Press, Atlantic Highlands 1975; and then into Paul K. Feyerabend, **Against Method. Revised Edition**, Verso, London — New York 1988; which morphed into Paul K. Feyerabend, **Against Method. Third Edition**, Verso, London — New York 1993). As we shall see in part 3, Bohm had already been using that phrase ("limited validity") to explain an essential feature of his methodology, which he called "the qualitative infinity of nature", according to which all methods *and all theories* have only a "limited validity" (even theories purporting to be universal; or as Bohm would later put it, to every explicate order, there is always an "implicate order", see David Bohm, **Wholeness and the Implicate Order**, Routledge, London 1980.

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incompatible universal theories compete and inform each other as an attempt at a realistic interpretation of experience.

In this way, this essay demonstrates Bohm's deep influence on Feyerabend and his relation to Popper and Popper's philosophy of science. ERE (1962) marks the moment that Feyerabend began to criticize Popper's methodology following Bohm in this respect. The essay also demonstrates the central roles played in this development by Bohm: Bohm and Vigier's hidden variables proposal, Bohm Brownian motion example in quantum theory and in methodology, that is, including his arguments for pluralism and tolerance based on that example, and ultimately, Bohm's ambition to develop a *new* conceptual framework for all of physics; one that could take into account what relativity theory and quantum theory get right, while being conceptually incompatible with both. [6] While the essay also tries to illustrate some potential aspects of Feyerabend's influence on Bohm, the main goal is to illustrate Bohm's influence on Feyerabend's ERE (1962) as a formative moment in Feyerabend's philosophical development that led to his criticism of Popper's methodology in ERE (1962), which set the stage for his infamous argument the there are no universally valid methodological rules, except of course, "Anything goes!". [7] On this interpretation, that formative moment, Feyerabend's one big step for methodology, was showing how incommensurability limits the validity of falsificationism. Feyerabend then generalized his conclusion: All methodological rules can have only a limited validity.

In sum, the essay has three parts. The first part explains Feyerabend's theoretical pluralism in contrast to and as part of a criticism of Popper's methodology as set out in ERE (1962). The second part explains the consequences this had for Feyerabend's relationship to Popper and his school, and the third part explains Bohm's supersized role in that development.

One last thing before we begin. It will be helpful to have a rough and ready

[6] In this essay, "quantum theory" refers to a framework within which many quantum theories have been developed. The framework consists of a background theory and a method (breaking things down into their smallest units, quanta, before they turn into something else) that implies universal (on that level) ontological principles that are used to interpret various quantum theories. Here, "interpret" means give meaning to in some context, where the context most relevantly for our purposes is the context of testing rival universal framework theories empirically.

[7] See Feyerabend "Against Method. Outline of an Anarchistic Theory of Knowledge..."; Feyerabend, **Against Method...** (1975); Feyerabend, **Against Method. Revised Edition**...; and Feyerabend, **Against Method. Third Edition...**

characterization of what Feyerabend was trying to say by calling theories "incommensurable" in ERE (1962), as incommensurability, according to Feyerabend, is what limits the validity of falsificationism. [8] This is especially important because, as we shall see, Feyerabend's idea of incommensurability was misunderstood by his contemporaries, including Popper, and it continues to be misrepresented by many today. This essay can be read as an argument for the thesis that in ERE (1962), Feyerabend just meant deductively disjoint due to meaning variance by "incommensurable", which he used to try to criticize Popper's methodology because it implies that contemporary universal theories explain the successful predictions that were deduced from, and thereby potentially explained by, antiquated rivals. Feyerabend argued that because rival universal theories are incommensurable, falsificationism has only a limited validity. It can only take testing commensurable theories (and not scientific revolutions) into account. Feyerabend had been developing the idea of incommensurability since the early 1950s. He only tried to clarify what he meant by "incommensurable" in ERE (1962) which he used to express that idea, and which he tried to clarify in many subsequent publication. [9] These clarifications offer a very good place for us to start. [10] In Feyer-

[8] For an overview, see Erick Oberheim and Paul Hoyningen-Huene, "The Incommensurability of Scientific Theories", in: Edward N. Zalta (ed.), **The Stanford Encyclopedia of Philosophy** 2024, https://tiny.pl/pgq5ch5j [05.08.2025].

[9] Feyerabend was evidently aware of the initial confusion caused by his own use of "incommensurable" in his, "Explanation, Reduction and Empiricism…" (1962), pp. 28–97, in the context of many people using the term "incommensurable" (or "*inkommensurabel*" in German) to mean different things. He simplified and summarized his views a year later without using the term in without using the term in "How to be a Good", "How to Be a Good Empiricist: A Plea for Tolerance in Matters Epistemological", in: Bernard Baumrin (ed.), **Philosophy of Science: The Delaware Seminar**, **Vol. II**, Interscience Publishers, New York 1963, pp. 3–39. See also Eric Oberheim, "Rediscovering Einstein's Legacy: How Einstein Anticipates Kuhn and Feyerabend on the Nature of Science", *Studies in History and Philosophy of Science. Part A*, 2016, Vol. 57, pp. 17–26.

[10] Note that our goal is to understand Feyerabend's early notion of incommensurability, not the later versions he developed after his awakening in December 1967, which led to Chapter 17 of Feyerabend, **Against Method…** (1975): "Now I think I have written a very good case study of a transition between incommensurable frameworks in chapter 17 of **Against Method**; it deals with the transition from the Homeric world view to the »substance universe« of some of the Presocratics and with problems such as: under what circumstances can we infer a change of perception from a change of style (in painting and sculpture), concepts and grammar i.e. when can we say that a group of people sees the world as it paints it (as it talks about it etc.) and when we cannot make such an inference. I also try to define incommensurability in this special case. I chose Homer in order to get away from the temptation to use philosophy-of-science terms and to develop more general terminology" (Feyerabend to Kuhn, 4 April 1983, in: Collodel and Oberheim, **Feyerabend's Formative**

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abend's philosophy, incommensurability is a specific form of "conceptual disparity". [11] Two theories are incommensurable: "if a new theory entails that all the concepts of the preceding theory have *zero extension* or if it introduces rules that *change the system of classes* itself". [12] For there to be incommensurability: "The situation must be rigged in such a way that the conditions of concept formation in one theory *forbid* the formation of the basic concepts of the other". [13] Or put differently: "When the meaning of their main descriptive terms depend on mutually inconsistent principles", [14] so that incommensurability means "*deductive disjointed-*

Years, Volume 2, Feyerabend, Logical..., pp. 314–315).

[11] See Paul K. Feyerabend, "Problems of Empiricism. Part II", in: Robert G. Colodny (ed.), **The Nature and Function of Scientific Theories: Essays in Contemporary Science and Philosophy**, *University of Pittsburgh Series in the Philosophy of Science*, Vol. 4, University of Pittsburgh Press, Pittsburgh 1970, p. 222 [275–353].

[12] Paul K. Feyerabend, "On the »Meaning« of Scientific Terms", *Journal of Philosophy* 1965, Vol. 62, p. 268 [266–274] [emphasis added].

[13] Feyerabend, "Problems of Empiricism, Part II...", p. 222. Compare Paul K. Feyerabend, **Science in a Free Society**, New Left Books, London — New York 1978, p. 68 and Feyerabend, **Against Method...** (1975), p. 269.

[14] Feyerabend, "On the »Meaning« of Scientific Terms...", p. 227. Compare Feyerabend, **Against Method...** (1975), pp. 269–270 and p. 276. Feyerabend sometimes seems to conflate "inconsistency" with "incompatibility". For example, "To sum up: the concept impetus is not »explicable in terms of the theoretical primitives of the primary science«. And this is exactly as it should be, considering the inconsistency between some very basic principles of these two theories" (Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 57; the passage cited by Feyerabend comes from Renest Nagel's article "The Meaning of Reduction in the Natural Sciences", in: Robert C. Stauffer (ed.), **Science and Civilization**, University of Wisconsin Press, Madison 1949, p. 120 [99–145]). Or to take another example, "The conceptual apparatus will have to be eliminated because its use involves principles, such as (7) in the example above ["motion is a process arising from the continuous action of a source of motion, or a »motor«, and a »thing moving« (Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 53)], which are inconsistent with the principles of *T*; and the laws will have to be eliminated because they are inconsistent with what follows from *T* for events inside *D*'" (Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 59). To be consistent and compatible with this interpretation, Feyerabend must sometimes mean incompatible when using "inconsistent". That is sometimes he must use it loosely not formally in terms of a contradiction between statements, according to the consequence of his own account. For example, he appears to call the ontological principles that give the basic terms of rival incommensurable theories their meaning (when realistically interpreted) "inconsistent", when he seems to mean "incompatible", because he maintains the concepts used to state incommensurable theories can have no formal relations. In any case, in Feyerabend, "Explanation, Reduction and Empiricism..." (1962), Feyerabend claims that incommensurable theories (that are deductively disjoint) are based on "inconsistent" principles because they have *incompatible* rules of usage (which give meanings to its basic terms), and as we

ness, and nothing else". [15] This idea of incommensurability and the test-model Feyerabend developed to handle it as illustrated by the Brownian motion example as a crucial experiment between incommensurable rivals continued to play a central role in Feyerabend's philosophy from ERE (1962) through **Against Method**. [16] Feyerabend published an entire paper defending it from Putnam's criticisms in "Putnam on Incommensurability" as late as 1987, well after he had developed his later notions of incommensurability in the wake of his awakening. [17]

In ERE (1962), incommensurability just means "deductively disjoint". In other words, you can simply replace every instance of some form of "incommensurable" with some form of "deductively disjoint" and the content stays the same. If two theories are "incommensurable", then they are "deductively disjoint". This implies both that one theory potentially cannot be a deductive consequence of another, *and* (in contrast to Popper's falsificationism) that none of the predictions deduced from one theory can be logically consistent with predictions deduced from the other, so the new theory cannot explain the old statements which stated antiquated facts as explained by antiquated theories. It follows that none of the pre-

shall see, this is where the incompatibility between the theories lies, which leads to the abandonment of an established theory. The established theory is not abandoned because formal relations between predictions deduced from them can be used to show that the new theory can explain all the successful predictions of the existing theory, while also falsifying it with a prediction deduced from it that logically contradicts a prediction deduced from its rival. It cannot explain those predictions. The existing theory is abandoned because it is shown to be based on the wrong ontology, according to a better corroborated alternative that can explain a novel phenomenon according to an incompatible ontology.

[15] Paul K. Feyerabend, "Changing Patterns of Reconstruction", *British Journal for the Philosophy of Science* 1977, Vol. 28, No. 4, p. 365 [351–382][emphasis added].

[16] See Feyerabend, **Against Method...**(1993), pp. 27–28.

[17] See Paul K. Feyerabend, "Putnam on Incommensurability", *The British Journal for the Philosophy of Science* 1987, Vol. 38, pp. 75–81. Wolfram Eilenberger insightfully emphasizes that this epiphany led to an incredibly productive phase in Feyerabend's career, which is typical of great philosophers, in which he wrote what became **Against Method** and **Naturphilosophie** (this work was first published as Paul K. Feyerabend, **Naturphilosophie**, Helmut Heit and Eric Oberheim (eds.), Suhrkamp, Frankfurt am Main 2009; translated into English as Paul K. Feyerabend, **Philosophy of Nature**, trans. Dorothea Lotter with assistance from Andrew Cross, Helmut Heit and Eric Oberheim (eds.), Polity Press, Malden 2016). While it was an epiphany, it was not a new insight in methodology, it was an insight into how far he thought he had come from Popper's methodology. Popper would likely have pointed out that his methodology allows for the use of *ad hoc* hypotheses for the purpose of exploration, just not to save theories when testing them, and that anyway, all methodological rules are just only rules of thumb.

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dictions deduced from one can logically contradict any statements deduced from the other (so the new theory cannot falsify the existing theory by successfully predicting (by deduction) a statement that logically contradicts a prediction deduced from the existing theory), although the predictions may *prima facie* look like they do because proponents of incommensurable theories use exactly the same sentences in the same contexts to mean something different.

As we shall see, this is the spooky (Wittgensteinian) point that follows from the contextual theory of meaning that Feyerabend uses to try to criticize Popper's account of falsificationism in ERE (1962). For Feyerabend, universal theories are deductively disjoint, because the meanings of the terms used to state them are conceptually incompatible. They are conceptually incompatible because changes in the system of classes (that break Kuhn's no-overlap principle) used to state the new theory imply that the concepts of its rival have zero extension (there is no "absolute mass").

As we shall also see, according to Feyerabend's test-model, the crucial experiment shows that the existing theory is wrong, but not with respect to its predictions. Rather, it was entirely wrong, as a wrong way of explaining every empirical statement that had been deduced from it and used to test it, including every statement about anything and everything it had purportedly explained. The new theory shows the existing theory was wrong about everything, by showing that it is based on incorrect ontological principles. The new theory implies a different ontology that affects the meanings of all empirical terms deducible from it, even the everyday terms used to state ordinary facts as expressed by accepted basic sentences that can be used to test it and which it can be used to explain. In "Explanation, Reduction and Empiricism", Feyerabend illustrates "an example of a cosmological assumption (anisotropic character of space) implicit in the common idiom" with the word pair "up-down" and concludes:

> This example refutes the thesis which has been defended by some philosophers that "everyday languages" are fairly free from hypothetical elements and therefore ideally suited as observational languages. [18]

[18] Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 85. In the footnote attached, Feyerabend points out that Feigl has introduced this thesis to him: "This thesis was introduced by Professor Herbert Feigl in discussions with me. For my own position, see also Philipp Frank" (Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 85, fn. 193), citing Philipp Frank, **Relativity, a Richer Truth,** Beacon Press, Boston 1950.

For Feyerabend, revolutionary new theories give new incompatible meanings to the same sentences that the established theory was already thought to have explained, and to which they gave different, incompatible meanings. Everything including "everything" must be re-explained according to the new theory and the ontological principles used to interpret it when it is used as a new realistic interpretation of experience. According to Feyerabend, disputes about which ontological principles a theory implies (or should imply) inevitably stir up Wittgensteinian fog due to their incompatible rules of usage. However, clarifying that fog by freeing current thoughts from the antiquated principles that were used to develop our language has an important function, as it can and should lead to crucial experiments that decide between entire worldviews based on indirectly refuting them by undermining their ontology, not by formally contradicting predictions (statements deduced from the theory) with their own. Feyerabend's normative test-model in ERE (1962) is designed to show how to do exactly that. It's a recommendation for how to revolutionize the world, literally. Although his views have been widely misunderstood, Feyerabend's methodological argument for pluralism was based on his test-model in contrast to and as part of a criticism of Popper's falsificationism. Feyerabend argued that because of meaning variance in the terms used to state rival incommensurable universal theories, the statements deduced from them and used to test them cannot enter formal relations. Meaning variance renders worldviews separated by revolutions deductively disjoint, or "incommensurable". Revolutionary new universal theories cannot explain antiquated facts.

Part 2 examines the consequences ERE (1962) had for Feyerabend's relation to Popper and his school. This part shows how the interpretation developed in this essay fits both the published *and unpublished record*, in contrast to the existing popular misconception of Feyerabend as a falsificationist in the early 1960s. For this purpose, we will draw evidence from the Feyerabend — Watkins correspondence that demonstrates that John Watkins (one of the main members of the Popperian school) was well-aware that the draft of ERE (1962) that he had read was an attempt to criticize Popper's methodology, but he was unable to understand how the criticism was supposed to work. To Watkins, it did not seem like a cogent criticism that accurately targets Popper's views. However, Watkins unquestionably clearly did understand that an early draft of ERE (1962) was intended as a criticism of Popper's methodology. As we shall see, the fact that ERE

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(1962) was intended as a criticism of Popper's methodology belies several prominent accounts of Feyerabend's relation to Popper.

Roughly seven years later, Feyerabend was again trying to explain his normative criticism of Popper's methodology to Watkins, but Watkins had finally had enough of it. On the 17th of December 1967, Feyerabend sent two letters, one to Lakatos and one to Watkins. Both explain that he was breaking from the Popperian school, because of his criticism of Popper's *methodology*. He accuses Watkins of being stuck in a cloud of Popperian ideology (as Watkins was so no longer willing to let Feyerabend try to explain his criticism of Popper's methodology); and he *explicitly* explains that he is breaking from Popper's school because of his criticism of Popper's methodology, and not because of his disagreement with Popper's about his propensity interpretation of quantum theory, which Watkins was more than willing to continue to discuss. On the 30th of December 1967, Watkins replied to Feyerabend's announcement that he was breaking from the Popperian school with respect to methodology given his criticism of it as "antihumanitarian": "Popperianism »is also antihumanitarian« — balls". [19] Feyerabend had wanted to have a "heart-to-heart" on the matter, and he had suggested to Watkins that they run a seminar on Popper's methodology together the previous semester. Instead, Watkins suggested that they run a seminar together on Feyerabend's criticism of Popper's propensity interpretation. He wanted to keep Feyerabend from leaving Popper's school, wandering lost in what had become a living nightmare. He recommended that Feyerabend should try to improve Popper's methodology instead of destroying it:

> And isn't this just the attitude you should now take to Popperianism? Despite your opposition to it you still find it the "most acceptable" philosophy at present available. Then don't try to burn it or stamp it out; try to improve it. [20]

In section 2 of part 2, we will examine Popper's reaction to "Explanation, Reduction and Empiricism", and Feyerabend's rebuke of Popper's reaction, and then Popper's response to Feyerabend's rebuke of Popper's reaction. This will establish how and why Popper seems to have misinterpreted Feyerabend by initially

[19] Watkins to Feyerabend, 30 December 1967, in Matteo Collodel and Eric Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians** (forthcoming).

[20] Watkins to Feyerabend, 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

taking Feyerabend's "incommensurable" to mean his own well-defined "*inkommensurabel*" (in German), which Popper had thoroughly discussed in his **Logik der Forschung** (1935). [21] This is how "Popperstein's baby" as a "Lessing effect" was born. Popper initially seems to have thought Feyerabend had misappropriated Popper's "inkommensurabilität", ascribed to him the opposite of the views he actually holds, and then tried to criticize Popper for allegedly holding those views with Popper's own ideas. Later, he seems to have concluded that "incommensurable" implies untestable in Feyerabend's work, bolstering the pernicious myth that "incommensurable" implies *empirically incomparable*, or untestable (because incommensurable rivals have no formally comparable consequences). Popperstein's baby continues to thrive as the popular misconception that Feyerabend was trying to extend or expand on Popper's falsificationism, when in fact he was trying to *criticize* it and *limit* its validity, while proposing an alternative test-model that can take account of testing incommensurable theories empirically *in contrast* to it.

Section 3 of part 2 explains how Feyerabend's relation to Popper's philosophy of science should be reevaluated with respect to Feyerabend's philosophical development subsequent to ERE (1962) through to **Against Method**. [22] When Feyerabend finally stepped out of Popper's school, he took his new "position" with him. He made the transition from the implication of his criticism of Popper's methodology in ERE (1962) that falsificationism *can* have only a limited validity to the more general conclusion that all methodological rules *can* have only a limited validity, not just falsificationism, but even its admonition against protecting theories from refutations with *ad hoc* hypotheses. In this way, the transition to the "Anything goes!" of **Against Method** was made in a single step in December 1967, when he announced his break from the Popperian school to Watkins and Lakatos. The postscript of part 2 shows that and why Popper took Feyerabend's criticism of his methodology in **Against Method** as a Lessing effect 2.0.

This will bring us to part 3 and Bohm's direct role in the genesis of ERE (1962) and the criticism of Popper's methodology within it, which lead to Feyerabend's

[21] See Karl R. Popper, **Logik der Forschung. Zur Erkenntnistheorie der modernen Naturwissenschaft,** Wien 1935, englisch translation Karl R. Popper, **The Logic of Scientific Discovery,** trans. Julius Freed and Lan Freed, Springer, Vienna 2002.

[22] See Feyerabend, "Against Method. Outline of an Anarchistic Theory of Knowledge…".

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subsequent break with Popper and his school with respect to methodology in 1967.

Part 1. "Explanation, Reduction and Empiricism" Explained

ERE (1962) represents a landmark in Feyerabend's formative philosophical development for at least two related reasons. First, in this essay Feyerabend initially launched his "methodological argument for theoretical pluralism" on the basis of the idea of incommensurability as illustrated by the Brownian motion example, which became the backbone of Feyerabend's philosophy of science all the way through to **Against Method**.[23] As we shall see, in this way, ERE (1962) set the stage for **Against Method**.[24] Second, in ERE (1962), Feyerabend used his methodological argument for theoretical pluralism to criticize Popper's falsificationism with an idea that would later become infamous. Feyerabend argued that because universal theories are "incommensurable", falsificationism fails as a universal methodology. In other words, because falsificationism cannot take testing incommensurable theories empirically into account, it has only a limited validity. With ERE (1962), Feyerabend was trying to break free from his mentor on methodology by criticizing his signature accomplishment in the philosophy of science (falsificationism) by hitting it where he knew it would hurt, by trying to show that it has become an anti-progressive, dogmatic school philosophy because it implies meaning invariance in the terms used in basic statements.

The purpose of this part of the paper is to show that, and exactly how, ERE (1962) was explicitly intended as a criticism of Popper's falsificationism that de-

[23] On Feyerabend's incommensurability and his methodological argument for theoretical pluralism, see Eric Oberheim, **Feyerabend's Philosophy**, De Gruyter, Berlin — New York 2006, Chapter 8, especially 8.3, pp. 245ff. Almost all of Feyerabend's major publications in the 1960s contain some version of his methodological argument for theoretical pluralism (precursors can also be seen in earlier publications), and it continues to play a central role in **Against Method**. For a long list of references to Feyerabend's methodological argument for theoretical pluralism, see Oberheim, **Feyerabend's Philosophy...**, p. 246, fn. 338.

[24] See and compare Feyerabend on the "manufactured character of experimental evidence" and the methodological implications of "the indeterminateness of facts", Feyerabend, "Explanation, Reduction and Empiricism..." (1962), pp. 48–52 with Feyerabend, **Against Method...** (1993).

veloped a methodological argument for theoretical pluralism in contrast to Popper's argument for pluralism. Popper argues that established theories are protected from direct refutations, unless there is a new theory that can explain them, falsifying the new theory. Feyerabend develops his argument for pluralism and the example illustrating his test-model in contrast to falsificationism as part of a criticism of it. This is why ERE (1962) was such a significant step in Feyerabend's break with Popper's school. Feyerabend's criticism of Popper's methodology in ERE (1962) is what led to Feyerabend's awakening in December 1967, as their dispute about methodology became the major factor contributing to the deterioration of their relationship, and what led directly to the infamous views Feyerabend set out in **Against Method**.

To explain ERE (1962), we will begin by considering Feyerabend's starting point (1.1 A problem with falsificationism), and his background assumption (1.2 Critical rationalism). We will proceed to the position (1.3 Kant on wheels realism) and the question: how can empirical evidence be used to test universal theories if they condition all experience? This illustrates Einstein's influence on Feyerabend's developing views. The fourth section develops the framework (1.4 Synthesizing Wittgenstein and Popper). The fifth section sets the stage for the arguments in ERE (1962) by emphasizing how it is a performance that uses a two-pronged argumentative strategy that takes ethics as the basis of methodological decisions (1.5 The performance). The sixth section develops Feyerabend's test-model (1.6 Conjectures and novel corroborations) based on the Brownian motion example. [25] This will finally put us in position to establish the main targets of Fey-

[25] "Brownian motion" has been used to mean different things. "Brownian motion" can be understood to mean a stochastic motion caused by atoms randomly bombarding a small particle from all sides. Directly before Einstein, however, it was used to mean and unexplained *thermal* phenomena. Before 1888 and Gouy's definitive measurements of "concomitant variations" between temperature and the rate of motion of the Brownian particle, even this was unclear, and it was just an unexplained phenomena, see Daniel Sirtes and Eric Oberheim, "Einstein, Entropy and Anomalies", in: J.-M. Alimi and A. Füzfa (eds.), **Albert Einstein Century International Conference**, *AIP Conference Proceedings* 2006, Vol. 861, No. 1, p. 1148 [1147–1154]. The existing empirical evidence prior to Perrin's confirmation of Einstein's prediction was inconclusive for the community at large, as Henri Poincaré concluded his talk at the Congress of Arts and Sciences in St. Louis in 1904, presenting Boltzmann's ideas and the results of the Gouy's experiments of on concomitant variation as follows: "I hasten to say we are not yet there, and as yet nothing proves that the principles [of classical thermodynamics] will not come forth from the combat victorious and intact" (see Sirtes and Oberheim, "Einstein, Entropy and Anomalies...", p. 1153). Gouy's concomitant variation experiments had convinced *some* scientists that Brownian motion posed a serious threat to classical thermodynamics,

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erabend's criticism (1.7 The targets and the purpose) and its main purpose: To disinfect empiricism to promote progress and thereby prevent the spread of pernicious ideologies. ERE (1962) was explicitly intended by Feyerabend as just such a criticism of Popper's falsificationism.

ERE (1962) should be read as explaining why falsificationism, posing as a one size fits all methodological rule of thumb, is infected to its empirical core (basic statements) with a pernicious ideology. It cannot take meaning variance into account and so it needs to be disinfected. This will put us in a position to establish the main thesis of Part 1 in the sections that follow. According to Feyerabend in ERE (1962), falsificationism cannot take testing incommensurable theories into account (section 1.8 The argument), and so it has only a limited validity (section 1.9 The implications). As we shall see, this interpretation of Feyerabend's criticism of Popper's methodology also explains some of Feyerabend's other controversial ideas that do not make sense from the perspective of Popper's falsificationism, specifically Feyerabend's contrastive account of empirical content and his ever-expanding ocean of *incompatible* alternatives account of scientific progress. In this section, we will also try to develop some of the implications of Feyerabend's criticism of Popper's methodology in ERE (1962) for Feyerabend's form of theoretical pluralism and why Feyerabend concluded that there are irreducibly subjective and pragmatic elements to methodological decisions, while embracing ontological relativism and the non-autonomy of facts. Before turning to part 2, the Postscript to part 1 revisits Feyerabend's criticism of Wittgenstein in light of Feyerabend's test-model and his conception of the role scientific philosophy can play in science.

1.1 The Starting Point and Motive Force: A Problem With Explanation in Falsificationism

As Feyerabend points out in the introduction of the essay, Popper's philosophy of science is explicitly both the *starting point* and the *motive force* of Feyerabend's investigation:

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however, the empirical evidence remained decidedly *indecisive* prior to Perrin's confirmation of Einstein's prediction deduced from the kinetic theory, after which the question was closed.

> Popper's admirable **Logic of Scientific Discovery** and his paper "The Aim of Science" have been both the starting point and the motive force of the investigations to follow. [26]

In the summary and conclusion, Feyerabend is even more specific, claiming that it was Popper's discussion of the condition that the predictions deduced from the new theory should be "inconsistent with", i.e. logically contradict, the predictions deduced from the existing theory "in the domain in which they overlap", which "has been discussed with great clarity in ["The Aim of Science"]," which was the "starting point of the present analysis of *the problem of explanation*," *adding* "(as well as dissatisfaction with "The Meaning of Reduction in the Natural Sciences")". [27] Thus, the starting point of, and motive force for, Feyerabend's critical

[26] Feyerabend, "Explanation, Reduction and Empiricism…" (1962), pp. 31–32. Feyerabend was intimately familiar with his mentor's writings, citing Popper, originally in German as Karl R. Popper, "The Aim of Science", *Ratio* 1957, Vol. 1, No. 1, pp. 24–35, while carefully noting that the basic idea, can already be found in Karl R. Popper, "Naturgesetze und Theoretische Systeme", in: Simon Moser (ed.), **Gesetz und Wirklichkeit: internationale Hochschulwochen des Österreichischen College, Alpbach-Tirol 21. August bis 9. September 1948**, Tyrolia Verlag, Innsbruck 1948, pp. 65–84, which is the initial German version. Popper thought Feyerabend put too much emphasis on concepts: "I doubt whether it is permissible to base as much as you do on concepts" (Popper to Feyerabend, 5 May 1958, Paul K. Feyerabend, "The Feyerabend–Popper Correspondence 1948–1967)", in: Matteo Collodel and Eric Oberheim (eds), **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper,** *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Venice — Berlin 2020, p. 281 [59–422]). **Feyerabend's Formative Years, Volume 1** is the first volume of **Feyerabend's Formative Years**, which is a three-volume collection of papers and correspondences still in progress. **Feyerabend's Formative Years Volume 2. Feyerabend, Logical Empiricism, Bohm and Kuhn** was published after this paper was initially drfated, but before it was finalized, with a third volume **Feyerabend among Popperians** in preparation. Henceforth, cited as Collodel and Oberheim, **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper…**, Collodel and Oberheim, **Feyerabend's Formative Years Volume 2, Feyerabend, Logical…**; Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians…**; respectively.

[27] See Feyerabend, "Explanation, Reduction and Empiricism…" (1962), pp. 91–92, main text, and p. 92, fn. 113. In a summary of his early views, including his ERE (1962), Feyerabend specified that his "general outlook" in the early 1960s was "derive[d] from the work of K. R. Popper (London) and David Bohm (London) and from [his] discussions with both. *It was severely tested*" in discussions with his "colleague, T. S. Kuhn (Berkeley). It was the latter's skillful defense of a scientific conservatism which triggered two papers". However, while he specifies that one of the papers he means is the current one ("How to be a Good Empiricist. A Plea for Tolerance in Matters Epistemological" (Feyerabend, "How to Be a Good Empiricist…"), Feyerabend does not explicitly specify which of his other papers he means. Given the overlap in themes, he could be referring to his motivation for writing "Knowledge without Foundations" (1961), although that interpretation would make this an awkward way for Feyerabend to refer to a paper that was delivered speaking freely with no prepared manuscript

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investigation was a problem with explanation in Popper's falsificationism, more specifically a problem with Popper's discussion of the condition that to falsify the existing theory, the new theory should have to be able to explain the successful predictions of the existing theory in the domain in which their predictions overlap, *as well as* dissatisfaction with Nagel's account of reduction. [28]

As we shall see, Feyerabend criticized and rejected recommending *precisely* this Popperian requirement of falsificationism when testing rival incommensurable theories, on the grounds that meaning variance renders them "deductively disjoint" so that predictions deduced from one theory cannot be explained by being formally consistent with corresponding predications deduced from the other. For this reason, falsificationism cannot take testing incommensurable theories into account, and so recommending it as *the* logic of scientific justification inhibits the kind of revolutionary progress that it purports to promote. In this way, Feyerabend clearly implies that (in addition to dissatisfaction with Nagel's account) his motivation for writing ERE (1962) was a problem with Popper's account of explanation. As we shall see, Feyerabend explicitly set out to "develop and to defend the outlines of [...] a disinfected empiricism" [29] that avoids this problem of explanation in Popper's account. He tries to develop a new model for testing rival in-

(the published text is based on a slightly revised transcription by the author of recordings of the lecture). On the other hand, Feyerabend may have been claiming that part of his motivation for writing ERE (1962) was to combat Kuhn's "skillful defense conservativism" (Feyerabend, "How to Be a Good Empiricist...", p. 8). This seems to make more sense given that "How to be a good Empiricist" is explicitly a summary of ERE (1962) and given that Feyerabend also criticized Popper for promoting a pernicious dogmatism in ERE (1962), that is, contemporaneously with his (unpublished, at the time) accusation that Kuhn was promoting a pernicious dogmatism with his "skillful defense" of conservativism.

[28] As is made clear in a footnote to ERE (1962), Feyerabend did not initially target Hempel's account with the criticism he had initially developed to target Nagel and Popper's accounts by developing a problem of explanation in Popper's account. Feyerabend acknowledges that it was Watkins who pointed out to him that "'every logically consistent observation report' be "logically compatible with the class of all the hypotheses which it confirms,' and more especially, he has emphasized that observation reports do "not confirm any hypotheses which contradict each other" (Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 44 and fn. 36, citing Carl G. Hempel, "Studies in the Logic of Confirmation", *Mind* 1945, Vol. 54, No. 213, pp. 1–26), so that according to Feyerabend, Hempel's account implies that "only such theories are admissible (for explanation and prediction) in a given domain which either contain the theories already used in this domain, or are at least consistent with them" (Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 44). This is how and why Hempel's account became one of Feyerabend's three main targets in ERE (1962).

[29] Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 31.

commensurable theories, in contrast to falsificationism, that illustrates how incommensurable theories can be tested despite their being deductively disjoint. In any case, it should be clear from the start that ERE (1962) was explicitly intended to handle a problem with Popper's methodology with respect to explanation. In other words, a comparison of the introduction to the conclusion of the essay clearly indicates that his motivation for writing the paper was *a problem* in Popper's methodology. In this way, Feyerabend indicates that his starting point (falsificationism) is not his ending point.

1.2 The Background Assumptions: Critical Rationalism as a Philosophy

With this starting point, Feyerabend proceeded to develop his critical investigation, including a new methodological argument for theoretical pluralism (in contrast to Popper's), *within a "critical rationalist" framework*, according to which theoretical pluralism, by increasing testability, increases progress. [30] "Critical rationalism" is sometimes misleadingly used synonymously with Popper's *normative* methodology (falsificationism), but this makes it impossible to understand Feyerabend's views, because in ERE (1962) Feyerabend criticizes one while endorsing the other. [31] For this reason, there is an important distinction between critical rationalism and falsificationism, which is necessary for understanding ERE (1962). In ERE (1962), Feyerabend, explicitly following Popper, presents critical rationalism as a *tradition* or *philosophy* that traces back to the Ionians, who took testability and *fallibilism* (not falsificationism, which Popper developed much later) as the hallmark of science. [32] For Feyerabend, critical rationalism is a philo-

[30] Feyerabend acknowledges that Popper, Kuhn, Bohm and Putnam are all theoretical pluralists in that they acknowledge a role for alternatives in the process of refutation. This implies that he is going to develop a form of theoretical pluralism that is different from these other forms. The difference, he thinks, is that his argument shows that alternatives are sometimes *necessary* for discovering the fact that leads to the abandonment of the established theory without falsifying it.

[31] See, for example, Collodel, "Was Feyerabend a Popperian…", p. 5, fn. 5, for more discussion, see part 2 on Popperstein's baby below.

[32] As we shall see, in ERE (1962), Feyerabend explicitly claims that Popper's view is that falsificationism is the empirical method both inside and outside of the natural sciences for replacing and established theory with an alternative based on empirical evidence.

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sophical tradition, or philosophy, that Feyerabend endorsed over other competing philosophies, most notably, what he took to be Wittgenstein's bastard child "linguistic philosophy" or "OLP" (short for ordinary language philosophy, which Feyerabend liked because of how awkward it sounds) and what he took to be Bohm's Hegelian dialectical philosophy.

This distinction between critical rationalism as a philosophy based on fallibilism and falsificationism is necessary for understanding that and how Feyerabend criticized Popper's views. Specifically, Feyerabend criticized Popper's normative methodology (falsificationism) from within a critical rationalist tradition (or as part of a critical rationalist philosophy). Put simply, Feyerabend endorses critical rationalism, while criticizing falsificationism. [33]

For this reason, the distinction between critical rationalism and falsificationism is fundamental for understanding Feyerabend's relation to Popper and his views. Feyerabend agreed with Popper that a critical rationalist philosophical framework that recommends trying to increase empirical testing and testability to promote progress is preferable to other philosophies such as OLP or Bohm and Kuhn's Hegelian dialectical philosophies (up until, and sometimes even after he broke with Popper and his school with respect to methodology in 1967). [34] However, for Popper, increasing testability is just the same thing as increasing falsifiability, while for Feyerabend, as we shall see, it is not. Feyerabend argues that falsificationism, not critical rationalism, cannot take testing incommensurable theories into account, limiting its validity to testing commensurable theories, and he

[33] Feyerabend also contrasts Popper's methodology from Popper's propensity interpretation in the foundations of quantum theory.

[34] As we shall see in part 2, when Feyerabend announced his awakening to Watkins of the 17th of December 1967, he made this explicitly clear: "Considering all this »Popperianism« seems now to me like a nightmare, not at all better than Hegelian philosophy of Nature. What now? In addition to being unrealistic the Popperian philosophy of science is also antihumanitarian", while in follow up letters he admits that Popper's philosophy is still the most preferable to him compared to the others: "Popperianism", I say, is the most reasonable philosophy in existence. »So don't try to stamp it out« you say, »try to improve it«. Well, that is just what I am not inclined to do [and now I am again not capable of giving arguments, just a mood — but there is nothing wrong with this if one admits it]. Why not? I really don't know. Besides, if I am correct in what I said in my lectures and in some of my arguments, then the improved method just says: »Anything goes except those things which are inconsistent with hedonism«" (Feyerabend to Watkins, January 1968, in Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

develops a rival test-model to falsificationism to show how there can still be crucial experiment between incommensurable theories despite meaning variance in the terms used to state them that renders them deductively disjoint so that formal relations cannot be established between the corresponding predictions deduced from them.

By the time Feyerabend wrote the first draft of ERE (1962) in early 1960, Popper had been mentoring him for over a decade, and Feyerabend had become familiar with Popperian ideas even before that from his Ph.D. supervisor, Viktor Kraft, who was a friend of Popper's and fellow anti-inductivist scientific philosopher. [35] Popper and Feyerabend had taken to each other immediately, with Popper quickly assuming the role of Feyerabend's mentor, when they first met in 1948 in Alpbach, which was the occasion that Popper delivered his lecture "The Aim of Science". [36] As fate would have it, as we have seen, this is precisely the "starting point" of Feyerabend's criticism of Popper's methodology in ERE (1962), which is also exactly the source of the dispute that eventually led to Feyerabend to announce his break with Popper and his school in December 1967. In this way, Feyerabend came full circle with respect to falsificationism.

According to Popper's methodology, progress should be by falsification of one theory by another, such as in the transition from Kepler's laws (and Galileo's theory of free fall) to Newton's theory, and then again in the transition from Newton's theory to Einstein's theory. For Popper, these two transitions illustrate falsifications, as according to his view, all the successful predictions deduced from the earlier theories (or laws in Kepler's case) could be explained by the new theory, while the new theory also successfully predicted statements that logically contradict statements deduced from the established theory, falsifying it.

For Feyerabend, while this may work in the Kepler case because they are commensurable, in the latter transition, there was no falsification, because Einstein's

[35] Kraft was the last remaining member of the notorious "Vienna Circle" still in Vienna after the war. See Daniel Kuby, "Paul Feyerabend in Wien 1946–1955: Das Österreichische College und der Kraft-Kreis", in: Michael Benedikt, Reinhold Knoll, Franz Schwediauer, and Cornelius Zehetner (eds.), **Auf der Suche nach authentischem Philosophieren. Philosophie in Österreich 1951–2000. Verdrängter Humanismus-verzögerte Aufklärung**, *Verdrängter Humanismus — verzögerte Aufklärung*, Bd. 6, WUV Wien 2010, pp. 1041–1056.

[36] See Collodel, "Was Feyerabend a Popperian…", pp. 27–56 and Collodel and Oberheim, **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper…**

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theory cannot explain the predictions from its rival, because meaning variance in the terms used to state them renders them deductively disjoint. In this way, with ERE (1962), Feyerabend used critical rationalism (fallibilism and the maxim to increase testability) to criticize precisely what had drawn him to Popper in the first place: His normative methodology as set out in "The Aim of Science" (1948), also known as "falsificationism".

By December 1967, he came completely full circle in an attempt at a philosophical patricide, in the sense of a student trying to break free from their mentor by finding a problem with their views, making space for their own alternative view on the subject that should replace their mentor's views. As we shall see, in this case, in ERE (1962), Feyerabend tried to break free from Popper's falsificationism by developing a new test-model, based on the idea of *incommensurability* instead of the idea of *falsifiability*; with his own methodological argument for theoretical pluralism (in contrast to Popper's methodological argument for theoretical pluralism); with his own contrastive account of empirical content (in contrast to Popper's account of empirical content); and with his own ever-expanding ocean conception of progress (in contrast to Popper's account of progress). [37]

In ERE (1962), Feyerabend explicitly emphasizes that Popper promotes theoretical pluralism and the essential role that alternatives play in the falsification of a theory. Feyerabend is also very careful to precisely highlight "[o]ne most important point of agreement [...]," between himself and Popper (and Kuhn, too), directly before beginning his critical investigation. The point of agreement "is the need, in the process of the refutation of a theory, for at least another theory". [38] Feyerabend explicitly emphasized that "this point has been made previously by K. R. Popper in his lectures on scientific method which [Feyerabend] attended in 1948 and 1952". [39] The papers Feyerabend wrote in the early 1960s are all indisputably Popperian in this specific sense. They support theoretical pluralism.

In ERE (1962), Feyerabend argues that "comprehensive theories cannot be eliminated by a direct confrontation with »the facts«". [40] For both Popper and Fey-

[37] Normative methodology and progress are always closely related, because a methodology explains how progress should be made.

[38] Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 32.

[39] Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 32.

[40] Feyerabend, "Explanation, Reduction and Empiricism..." (1962), p. 70.

erabend, direct refutation of a theory by predictions deduced from it is impossible, because it can always be protected by *ad hoc* maneuvers, unless there is an alternative that can explain the refuting instance (see Duhem). [41] Feyerabend had learned this lesson firsthand through his involvement in setting up Ehrenhat's effects, which were dismissed as *Dreckeffekt*. As it will turn out, the difference between Feyerabend and Popper is that for Feyerabend, the facts explained by the new theory are not the same facts that the established theory purportedly explains after a scientific revolution, which falsificationism, he argues, would seem to imply.

By the late 1960s, Feyerabend began to denigrate what he called his "series of Popperian papers" on empiricism and normative methodology as his dramatic reversal from chief supporter to chief critic of Popper became clearer. Feyerabend first refers to his "awakening" from his Popperian "dogmatic slumber" in letters to Lakatos and Watkins on 17 December 1967: "Listening to your [Lakatos's] lecture on Popperianism meant the end of my dogmatic slumber. Now I am awake" (Feyerabend to Lakatos 17 December 1967), [42] while announcing his break with the Popperian school. [43] Later, he reflected, "there were years and years when I was practically dormant. During this period, I wrote a series of Popperian papers". [44] By "Popperian", Feyerabend meant critical rationalist, not falsificationist. Feyerabend never really abandons critical rationalism. He just eventually finds a new

[41] See Pierre Duhem, **The Aim and Structure of Physical Theory**, trans. Philip P. Wiener, first edition 1906, Princeton University Press, Princeton 1954.

[42] Imre Lakatos and Paul K. Feyerabend, **For and Against Method: Including Lakatos's Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence**, Matteo Motterlini (ed.), University of Chicago Press, Chicago 1999, pp. 120–125.

[43] As we shall see, Feyerabend sent a letter to Watkins explaining his break from him and the Popperian school on the same day, see part 2.

[44] Letter to Bechler 1 January 1976, Paul K. Feyerabend, "Curricula Vitae", in: Matteo Collodel and Eric Oberheim (eds), **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper**, *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Venice — Berlin 2024, p. 32 [45–55]. Feyerabend's "Popperian papers" include ERE (1962); "How to Be a Good Empiricist…"; Paul K. Feyerabend, "Realism and Instrumentalism: Comments on the Logic of Factual Support", in: Mario Bunge (ed.), **The Critical Approach to Science and Philosophy: In Honor of Karl R. Popper**, The Free Press of Glencoe, London — New York 1964, pp. 280–308; Paul K. Feyerabend, "Problems of Empiricism", in: Robert Garland Colodny (ed.), **Beyond the Edge of Certainty: Essays in Contemporary Science and Philosophy**, *University of Pittsburgh Series in the Philosophy of Science,* Vol. 2, Prentice-Hall, Englewood Cliffs 1965, pp. 145–260.

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form of it in John Stuart Mill's work. [45]

The salient point is that Feyerabend's 1960s papers and his early 1970s work including **Against Method** are all indisputably critical rationalist in an important sense — Feyerabend's own later attempts to distance himself from Popper and his philosophy notwithstanding. [46] They are critical rationalist in that Feyerabend assumes fallibilism, that progress is made by increasing testability, and that progress is good. In fact, when Feyerabend argues that incommensurable proposals are preferable to commensurable new proposals because they can increase the testability of the existing point of view, he is using critical rationalism (which includes the maxim to increase testability) to criticize falsificationism, which is limited to testing commensurable theories, as part of an imminent criticism of it (see section 1.5). Feyerabend turns Popper's maxim to increase testability against Popper's falsificationist methodology.

In other words, in ERE (1962), while Feyerabend enthusiastically agreed with Popper that theoretical pluralism best promotes progress, at the same time, he also criticized Popper's account of how theoretical pluralism should best promote progress, rejecting Popper's account of falsificationism as *the* logic of scientific justification, and as *the* empirical method of the sciences (or any other discipline). Feyerabend was well aware that Popper had not intended to suggest that falsificationism is *the* scientific method, but just a rule of thumb. However, in Feyerabend's view the Popperians had hardened into a pernicious school philosophy by elevating falsificationism to the status of a universally valid methodological rule of thumb (the best, or even only, way to secure a justification based on experience for replacing an established theory by a rival:

> One will *not* gloss over refutations — nor should one take them overly seriously [. . .] The only argument that could be raised is that agreement with experience is more important than the looks of a theory. But then I would like to hear why this should be so. Some people will say: "Because experience is the foundation [the testing ground] of our knowledge". This for me means being too sure of experimental statements, more

[45] See Collodel, "Was Feyerabend a Popperian...", pp. 27–56.

[46] See Feyerabend, **Against Method...** (1975), Feyerabend, **Against Method...** (1988), and Feyerabend, **Against Method...** (1993). For a detailed presentation of evidence showing that Feyerabend was a critical rationalist *philosopher* in this period, see Collodel, "Was Feyerabend a Popperian...", pp. 27–56.

> sure than a Popperian should be. Question: is it true as I suspect that Popperians at LSE are more sure of experience than Popperians should be? [47]

According to Feyerabend, the problem with Popper's methodology is that it has only one test-model for empirically comparing theories which cannot take comparing incommensurable rivals empirically into account. Theories are compared empirically by deducing testable predictions from them and then establishing formal relations between them. Feyerabend argues this account cannot work for testing rival incommensurable universal theories, and he develops a test-model that shows how there can be crucial experiment between incommensurable theories that are deductively disjoint.

This is how ERE (1962) set the stage for Feyerabend's break with Popper in December 1967, after Feyerabend had begun a new phase in 1965, what he called his "skeptical". [48] In this phase, he started to have doubts about his normative approach to methodology (it seemed too detached from concrete problems of research), [49] and he began to shift focus to a more historical approach often associated with Kuhn. [50] However:

> Actually, it was not Kuhn who made me [Feyerabend] change, but an event at the Institute of Professor von Waizsäcker then in Hamburg. In Hamburg (1965, I believe) I gave a lecture where I emphasized the need for proliferation. A physicist, I said, who has a highly confirmed theory in his hands must invent and develop alternatives to

[47] Feyerabend to Watkins, January 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians…**

[48] Collodel has done an excellent job identifying how best to refer to this development. See his astute account of it in: Collodel, "Was Feyerabend a Popperian…", pp. 27–56.

[49] This phase leads to Feyerabend's argument that because it was so detached from actual science, philosophy of science had become "a little-known form of insanity", see Paul K. Feyerabend, "Die Wissenschaftstheorie — eine bisher unbekannte Form des Irrsinns?", in: Kurt Hubner and Albert Menne (eds.), **Natur und Geschichte: X. Kongress fur Philosophie, Kiel 8.–12. Oktober 1972**, Felix Meiner Verlag, Hamburg 1973, pp. 88–124.

[50] Feyerabend's increased emphasis on history of science can be seen as he gradually abandons his quest for a purely normative methodology. Compare his "Realism and Instrumentalism" (which clearly emphasizes his early normative apporach) with Feyerabend's more historically-oriented work after he left the Popperian school, such as Feyerabend, **Naturphilosophie…**, for an English translation, see Feyerabend, **Philosophy of Nature…**, which he began writing in the late 1960s as a companion to Feyerabend, **Against Method…** (1975), and the variously revised and expanded later versions of Feyerabend, **Against Method…** (1975). Compare Feyerabend, **Against Method…** (1988) and Feyerabend, **Against Method…** (1993).

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> see whether what he has and values is worth something. Professor Waizsäcker then started discussing some very concrete problems that faced physicists in the early quantum theory. Compared with these problems the general injunction "proliferate", even though supported by the most beautiful arguments [he means his own methodological argument for theoretical pluralism based on his test-model in contrast to Popper's falsificationism] *seemed to be ever so much moonshine*. I suddenly felt quite ridiculous — and this was the beginning of what one might call "my skeptical phase". [51]

Feyerabend's adamant rejection of Popper's philosophy of science on humanitarian grounds becomes much more explicitly emphasized in Feyerabend's published work after he finally broke with the Popperian school in 1967, starting immediately in 1968 with "Science, Freedom, and the Good Life". [52] However, while Feyerabend criticized Popper's methodology, and normative methodology more generally in ever more explicit terms, he continued to develop his own critical rationalist *critical* views on normative methodology all the way through to **Against Method** (1993). In this specific sense, he continued to promote the critical rationalist principle that in science, one should (at least for the most part), try to increase the testability of theories, even after he had made a major transition in his perspective in the late 1970s. [53] In sum, Feyerabend was working within a critical

[51] Feyerabend to Bechler, 1 January 1976, in: Feyerabend, "Autobiographfical Remarks 1976...", p. 33 [emphasis added].

[52] See Paul K. Feyerabend, "Science, Freedom, and the Good Life", edited by Joseph Agassi and Michael Chiariello from a public talk delivered to The Boston Colloquium for the Philosophy of Science, *The Philosophical Forum 1968*, Vol. 1, No. 2, pp. 127–135. Again, see Collodel, "Was Feyerabend a Popperian...", pp. 27–56, who does an excellent job of highlighting this point. See also Kidd's extremely insightful accounts: Ian Kidd, "»What's So Great about Science?« Feyerabend on Science, Ideology, and the Cold War", in: Elena Aronova and Simone Turchetti (eds.), **Science Studies during the Cold War and Beyond: Paradigms Defected**, *Palgrave Studies in the History of Science and Technology*, Palgrave Macmillan 2016, pp. 55–76; Ian Kidd, "Reawakening to Wonder Wittgenstein, Feyerabend and Scientism", in: Jonathan Beale and Ian James Kidd (eds.), **Wittgenstein and Scientism**, Routledge, London and New York 2017, pp. 101–115; Ian Kidd, "Feyerabend, Science and Scientism", in: Karim Bschir and Jamie Shaw (eds.), **Interpreting Feyerabend: Critical Essays**, Cambridge University Press, Cambridge 2021, pp. 172–190.

[53] By the early 1980s, Feyerabend also relegated the role that he recommended science should play with respect to how one should decide what to believe to be real, increasingly acknowledging the pernicious scientism implicit in his early approach that insists that common sense needs "to be corrected by science. After this reversal (due to insights belatedly learned from Wittgenstein's work, in a move toward Wittgenstein's philosophy), Feyerabend argued that common sense needs to be protected from science, not corrected by it. For a discussion of this reversal, see Jamie Shaw, "Feyerabend Never Was an Eliminative Materialist: Feyerabend's Meta-Philosophy and the Mind–Body Pro-

rationalist framework that takes the maxim to increase testability as the hallmark of science, when in ERE (1962), he criticized Popper's philosophy of science (his normative methodology also known as falsificationism). Moreover, even after he broke with Popper's school in 1967, he continued to promote pluralism and critical rationalist ideas, if even just to use them to criticize methodological views promoted by others.

1.3 The Position and The Question: Einstein's Influence on Feyerabend's Theoretical Pluralism

In the introduction of ERE (1962), Feyerabend explains that his "position" consists of "two ideas": The first idea is Kantian: universal theories condition all experience. They are all-pervasive:

> The first idea [of his position] is that [...] scientific theories are ways of looking at the world; and their adoption affects our general beliefs and expectations, and thereby also our experiences and our conception of reality. We may even say that what is regarded as "nature" at a particular time is our own product in the sense that all the features ascribed to it have first been invented by us and then used for bringing order into our surroundings. As is well known, it was Kant who most forcefully stated and investigated this *all-pervasive character of theoretical assumptions*. [54]

This is part of Feyerabend's normative conception of realism, [55] which can be understood as the claim that universal theories do and should play the role of "*relativized a priori*" principles that can be replaced through scientific revolutions. [56]

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blem", in: Karim Bschir and Jamie Shaw, **Interpreting Feyerabend. Critical Essays**, Cambridge University Press, Cambridge 2021, pp. 125–131 [114–131].

[54] Feyerabend, "Explanation, Reduction and Empiricism…" (1962), p. 29 [emphasis added].

[55] That Feyerabend treats realism as a normative, and not a factual, issue, is made most clearly in Feyerabend, "Realism and Instrumentalism…", pp. 280–308. There is an extensive discussion of Feyerabend's realism in Oberheim, **Feyerabend's Philosophy…**, Ch. 6.

[56] On relativized *a priori* principles, see Michael Friedman, **Reconsidering Logical Positivism**, Cambridge University Press, Cambridge 1999; Michael Friedman, **A Parting of the Ways: Carnap, Cassirer, and Heidegger**, The Open Court, Chicago 2000; Michael Friedman, **The Dynamics of Reason**, CLSI Publications, Stanford 2001 and Allan W. Richardson, "Narrating the History of Reason Itself: Friedman, Kuhn, and a Constitutive A Priori for the Twenty-First Century", *Perspectives on Science* 2002, Vol. 10, No. 3, pp. 253–274, https://doi.org/10.1162/106361402321899050.

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A priori principles determine the conditions for the possibility of experience within that framework. In contrast to Kant's account according to which they are fixed once and for all, providing the one and only framework for stating facts (the classical worldview), *a priori* principles are relativized to the worldviews that they make possible. New universal theories provide new *a priori* principles relative to the new framework and the worldview it supports. In this way, Feyerabend's view is Kantian, but dynamic, or "Kant-*on-wheels*". The idea is that when a revolutionary new universal theory replaces an established universal theory our entire worldview changes. The new theory provides new *a priori* principles that redefine the entire framework, including all the everyday facts that can be stated within it and were thought to have been explained by it.

According to the new theory, what were thought to be secure empirical facts turn out not to have been secure nor facts, as new facts that are part of a new conceptually incompatible worldview should replace them. According to Feyerabend, when one universal theory replaces another, every empirical statement about something should have a new meaning, including all everyday observations, even when exactly the same sentences are used in the exact same context in exactly the same way, so that one and the same sentence will make two incompatible statements, if it is interpreted according to rival incommensurable theories. In Feyerabend's example, "it fell" meant either that it was pushed down from within by its impetus or that it was pulled down from outside by gravity, when it was used to test rival incommensurable theories with the same experiments. [57]

This sort of dynamic Kantian position became popular in wake of the revolutions brought on by Einstein's special theory of relativity and quantum theory. For example, Feyerabend's form of Kant-on-wheels realism is very similar to Kuhn's, and they were both following Einstein, who had himself endorsed a dynamic Kantian realism. In fact, to pinpoint Feyerabend's Kant-on-wheels realism, it helps to contrast it to its closet sibling, Kuhn's Kant-on-wheels realism.

Kuhn was emphasizing what, from Feyerabend's perspective, might be called "mini-revolutions" in natural science. Kuhn's scientific revolutions are transitions to new subject specific paradigms that involve local conceptual breaks (for example in chemistry and then in different branches of chemistry as they develop, in bi-

[57] As we shall see, Feyerabend took this to be a significant insight with respect to his development of incommensurability.

ology and then in different branches of biology, just as in physics and branches of physics). The kind of meaning change Kuhn is talking about is specialized and and local so that even changes to the meanings of lower-level laws and theories result in incommensurability for Kuhn. Feyerabend, by contrast, was primarily interested in revolutions involving universal theories in which entire worldviews are at stake. In this sense, Feyerabend's Kantianism runs deeper than Kuhn's. For Kuhn, revolutions involve a specific form of meaning change (breaking the no-overlap principle) of the terms used to state the relations between *kinds of things*. The kinds of things may be any kinds of subject specific things, not just the kinds implied by universal theories that affect everything by changing the meaning of thinghood itself. In this way, for Kuhn there are many "minor" mini-revolutions (in comparison to the relatively few major transitions between entire worldviews). Moreover, after a revolution, for Kuhn, a scientist will experience a new world made up of *new kinds of things*, when looking at the same individual things as before the revolution. In contrast, Feyerabend is talking about variance in the meaning of "thinghood" itself, claiming that revolutionary worldviews change even the meaning of what it means to be something at all. Feyerabend makes it clear that he was using "incommensurable" in a more narrow sense in his early work than in **Against Method**, and that he was using the term in a more narrow sense than Kuhn in a letter to Kuhn in 1983:

> Your notion of incommensurability is of course much wider [than my earlier idea as set out in "An Attempt at a Realistic Interpretation of Experience] without using the term incommensurable"]. It is not tied to the statement view, as Stegmüller calls the idea that science is the same as a collection of statements and it takes non-articulated tendencies into account. But I don't think that you will find faults with either the Homer case [transition between incommensurable frameworks in chapter 17 of **Against Method**] or the earlier and more narrow version. [58]

Sometimes in ERE (1962) Feyerabend seems to suggest that falsificationism cannot take any cases of incommensurable theories into account, and not just that it cannot take cases of comparing universal incommensurable theories into account. If there is meaning variance that renders two or more theories deductively disjoint, then predictions deduced from one cannot logically contradict predictions deduced from its rivals, nor are predictions deduced from one logically con-

[58] See Feyerabend to Kuhn, 4 April 1983, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, pp. 314–315.

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sistent with the predictions deduced from the other *even when they are expressed by exactly the same sentence as the result of the same measurement procedure*. This is why Feyerabend does not think Kuhn will mind his narrow use in his early work. [59] This is also what Feyerabend understood Wittgenstein to be showing in the **Investigations** as a complimentary second half of the **Tractatus** (as Wittgenstein had wanted to see it published). The same sentence will mean something radically different when it is interpreted by an incommensurable rival theory (like a Gestalt switch), because observational meanings are determined by the meanings of the terms used to state the theories that explain them. Whenever that happens, one sentence makes two incompatible statements. Kuhn's account as developed in the 1980s converges with and compliments Feyerabend's account, as Feyerabend's account can be clarified by using Kuhn's no-overlap principle to explain how and why a change to the system of classes itself results in meaning variance that renders two theories deductively disjoint, while implying that the terms used to state its rival all had zero extension. [60]

For example, according to this position, after the relativity revolution, there is no such thing as "mass" understood according to Newton's theory, and before that revolution there was no such thing as "relative mass" that changes with change in velocity. Now there is this new kind of mass (relative mass) in contrast to the earlier mistaken view that there is mass that does not change relative to change in

[59] By 1983, Kuhn, who did not initially apply "incommensurable" to theories, had narrowed his focus to taxonomic incommensurability between "theories", (untangling not abandoning his early views), so in this sense they moved in opposite directions: Feyerabend broadened his view to include Kuhn's initial points and Kuhn narrowed his focus to where Feyerabend initially began. That is why Kuhn's no overlap principle can be used so well to explicate Feyerabend's early idea of incommensurability, and why Feyerabend thinks that Kuhn will not "find fault" with either idea, see the last block quote above. See Eric Oberheim, "Scientific Progress and Incommensurability", in: Yafeng Shan (ed.), **New Philosophical Perspectives on Scientific Progress**, Routledge, New York 2020, pp. 293–313 and Eric Oberheim, "Incommensurability and Metaincommensurability. Kind Change, World Change and Indirect Refutation", in: Leandro Giri, Pablo Melogno, and Hernán Miguel (eds.), **Perspectives on Kuhn. Contemporary Approaches to the Philosophy of Thomas Kuhn**, *The Western Ontario Series in Philosophy of Science 84*, Springer, Cham 2023.

[60] See Oberheim, "Scientific Progress and Incommensurability…". In Feyerabend's examples in ERE (1962), the transitions from one universal theory to another broke the no-overlap principle with respect to kinds of motion according to Aristotelian theory and kinds of motion according to Newton's theory, and then again from Newton's theory to Einstein's redescription of kinds of motion.

velocity. As Feyerabend sees it, within a framework (classical mechanics) provided by an antiquated universal theory that continues to be used and researched, [61] what was thought to be "mass" is now called "absolute mass", which, according to the new theory (relativity theory) does not exist given a realistic interpretation. The issue of realism was always of paramount interest to Feyerabend, even before he met Popper. Indeed, it seems likely that Popper's realism may have been part of what drew Feyerabend to Popper in the first place. [62] In ERE (1962), Feyerabend effusively praises Popper for instigating this "decisive turn" toward (or rather, return to) realism and away from the phenomenalism of logical empiricism. Feyerabend cast this Popperian return to realism as a return to the earlier realism of Neurath and Carnap's physicalism back in early 1930s. [63]

Following Popper and critical rationalism, the second idea implicit in the position Feyerabend defends demands that "our theories be testable and that they be abandoned", [64] and replaced, if they fail those tests, because science should always "proceed to better and better theories". [65] So, the position results from the equation: Kant plus critical rationalism equals Feyerabend's Kant-on-wheels realism. As we are about to see, this raises the *leading question* of ERE (1962), which ultimately results in Feyerabend's "plea for tolerance in matters epistemological", [66] to promote progress in science, in order to prevent entrenchment of pernicious

[61] See Paul K. Feyerabend, "In Defence of Classical Physics", *Studies in History and Philosophy of Science* 1970, Vol. 1, No. 1, pp. 59–85, https://doi.org/10.1016/0039-3681(70)90025-7.

[62] See Collodel, "Was Feyerabend a Popperian…", pp. 27–56 and Collodel and Oberheim, **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper…** Similarly, Feyerabend was sometimes very explicit that it was Herbert Feigl's realism that is what drew Feyerabend to Feigl and the Minnesota Center for Philosophy of Science, which is what brought him in direct contact with leading logical empiricists, such as Hempel and Carnap, see Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical…** As we shall see, the footnotes in ERE (1962) emphasize the significant roles Feigl played in setting the stage for Feyerabend's criticism of Popper's methodology in ERE (1962).

[63] See ERE (1962), p. 35. Feyerabend's 1951 doctoral thesis (which uses Popper's technical term "Basissätze" in the title) had been on the infamous "Protocol-sentence" debates. See Paul K. Feyerabend, **Zur Theorie der Basissätze** (doctoral dissertation), Universität Wien, Vienna 1951 and Rudolf Carnap, "Über Protokollsätze", *Erkenntnis* 1933, Vol. 3, pp. 215–228. See also Kuby's excellent work: "Paul Feyerabend in Wien 1946–1955…", pp. 1041–1056 and Daniel Kuby, "Carnap, Feyerabend, and the Pragmatic Theory of Observation", *HOPOS: The Journal of the International Society for the History of Philosophy of Science* 2018, Vol. 8, pp. 432–470.

[64] Feyerabend, ERE (1962), p. 29.

[65] Feyerabend, ERE (1962), p. 30.

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ideologies that can quickly degenerate into violence. [67] Feyerabend builds up to his leading question by pointing to the tension between these two ideas (which his own suggestion, his test-model, is going to resolve), explaining that:

> *some philosophers* [...] started out by reacting against the claim that scientific theories are nothing but predictive devices [instrumentalism]; they recognized that their influence goes much deeper [realism]; however, they then doubted that *it would be possible ever to get outside any such theory*. [68]

Note that Feyerabend does not initially specify who exactly he means by "some philosophers". This sets up his first reveal, as it will turn out later in ERE (1962) that he reveals that he meant Wittgenstein (and some Wittgensteinians). Note too that Popper's falsificationism is supposed to resolve exactly this sort of issue by showing how it is possible to test such theories and make progress (by falsification). However, as we shall see, Feyerabend argues that falsificationism fails in exactly this respect when applied to rival universal theories so that it does not relieve this tension, because it cannot take testing universal theories into account, which is why Feyerabend tries to develop a test-model that can. After his awakening, he indicated that he was trying to dissolve Popper's solution to this question:

> Popperian problems at least are only 30 years old. And their solution even younger. And the dissolution of the latter (by that well-known gentleman philosopher PKF) still younger. [69]

To resolve this tension, according to Feyerabend, as a staunch defender of realism:

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[66] Title of Feyerabend, "How to Be a Good Empiricist...".

[67] Feyerabend, "How to Be a Good Empiricist..." is explicitly supposed to be a simplified summary of the views presented in ERE (1962) and Paul K. Feyerabend, "Problems of Microphysics", in: Robert G. Colodny (ed.), Frontiers of Science and Philosophy, University of Pittsburgh Series in the Philosophy of Science, Vol. 1, University of Pittsburgh Press, Pittsburgh 1962, pp. 189–283. It is more than conspicuous that the term incommensurable is nowhere to be found in this summary. Feyerabend uses the term "irreducible" instead. Perhaps Feyerabend avoided the term because, as we shall see, it was causing so much confusion.

[68] Feyerabend, ERE (1962), p. 30 [emphasis added].

[69] Feyerabend to Watkins, January 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

> What we need is a guarantee that despite the all-pervasive character of a [universal] scientific theory as it is asserted in the first idea, it is still possible to specify facts that are inconsistent with it [as is asserted in the second idea]. [70]

In other words, put simply, the leading question the Feyerabend sets out to investigate in ERE (1962) is: If *universal theories* condition all experience, then how can experience still be used to test them? This is the leading question Feyerabend is going to try to answer with a *constructive criticism* of Popper's methodology in ERE (1962). ERE (1962) develops a *constructive* criticism because it does not just criticize other accounts (such as Popper's falsificationism), it also proposes an alternative to the accounts criticized that circumvents the problem the criticism of those accounts is based on — similar to falsificationism, which requires an alternative account to explain an anomaly as an indirect refutation of its rival, without which it can and will be protected by *ad hoc* hypotheses. Although the main elements of his destructive criticism of Popper's methodology are present in ERE (1962), Feyerabend only explicitly turns his constructive criticism of Popper's methodology in ERE (1962) it into a destructive criticism of normative methodology altogether, Popper's included, after he entered his new skeptical phase according to which *general* methodological rules with limited validity are useless if they are not directly connected to specific situations, and after his awake from his Popperian slumber and took the final step in December 1967, generalizing from his conclusion that falsificationism can have only a limited validity, and his view that the admonition against the use of *ad hoc* hypotheses can have only a limited validity to the conclusion that all methodological rules can have only a limited validity. As we shall see, initially, he wanted to fix Popper's methodology by disinfecting it with serious arguments, later he wanted to "stamp it out" without serious arguments: "I want to cheer up people, not to guide them into the rathole of »serious criticism«". [71]

To see how Feyerabend develops the leading question and the targets of his constructive criticism in ERE (1962), we can simply start with Feyerabend's *starting point,* which as we have seen is Popper's falsificationism. Taking Feyerabend's starting point into account, the leading question can be put more sharply: If uni-

[70] Feyerabend, ERE (1962), p. 30.

[71] Feyerabend to Watkins, January 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

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versal theories condition all experience, then how can experience still be used to *falsify* them? Spoiler alert: It can't. They can always be protected from direct refutation with *ad hoc* hypotheses, and falsification as an indirect refutation cannot work either. Universal theories cannot falsify their rivals by explaining their successful predictions, nor can predictions deduced from one logically contradict predictions deduced from other. They can, however, be superseded by theories that offer a more successful potential explanation of (potentially) everything that the older theory was once thought to have successfully explained and something more that the older theory could never, by itself, have explained. As we shall see, in this way, Feyerabend resolves the challenge posed by his leading question in ERE (1962) by developing a new test-model that is *not*, as is sometimes supposed, simply a repetition of Popper's views on methodology, but rather was supposed to be part of a criticism of it. Feyerabend argues that because universal theories are incommensurable ("deductively disjoint"), they are *not falsifiable* in Popper's specific sense that the new theory can explain the successful predictions deduced from the existing theory by predicting by deduction statements that are logically consistent with them, (nor can successfully confirmed predictions deduced from the new theory *logically contradict* predictions deduced from its rival). However, this does not imply, for Feyerabend, that universal theories are not empirically testable. As we shall see, that mistake (conflating testability with falsifiability) is exactly what results in Popperstein's baby and the view that incommensurability implies empirically non-comparable. By contrast, it implies for Feyerabend that falsificationism is not *the* logic of scientific justification, because it cannot take testing incommensurable universal theories (or perhaps even any incommensurable theories) into account. It implies, for Feyerabend, that universal theories are tested by a different form of indirect refutation, not by falsification, as illustrated by the Brownian motion example.

By calling two theories "incommensurable" or "deductively disjoint", Feyerabend just means that neither the theories, nor prediction deduced from them, can enter into formal logical relations with each other. Two deductively disjoint *theories* can *neither* be logically consistent (or inconsistent) with each other, *nor* can *statements* deduced from one logically contradict or be consistent with any of the statements deduced from the other. The main problem with respect to Popper's falsification is that for Feyerabend, none of the statements deduced from one universal theory can have formal logical relations with any of the statements de-

duced from its rival. They have no formally comparable consequences. In other words, Feyerabend explicitly develops a test-model *in contrast* to Popper's falsificationism to show how universal theories *are* still empirically testable by crucial experiments [72] *despite* the all-pervasive character of universal theories *and even though* meaning variance renders them deductively disjoint ("incommensurable") so that formally a falsification is impossible. In so doing, he criticizes Popper's methodology, not science:

> I thought [1962] that while the incommensurability might be a disaster for philosophical theories of explanation and reduction I did not regard it as a disaster for science and when I said that scientific progress involves "irrational" elements I only meant: "irrational" in the sense of certain philosophical theories. The criticism was of philosophy, not of science. [73]

Feyerabend's test-model is based on the methodological implications of the example of Einstein's predictions about Brownian motion, which (when later confirmed by Perrin and others) indirectly refuted a rival universal theory, providing a "crucial experiment" that catapulted us into the age of atomism and quanta, without *falsifying* Newton's theory by establishing formal relations between predictions deduced from the rival theories. [74] Confirmation of the predictions corroborated the new theory, but the rival theories are based on incompatible ontologies, as the predictions deduced from the rival theories are theory-laden with incompatible theories, so that there can be no formal relation between them. Note that Feyerabend's model for testing universal theories is a model of a "crucial experiment" in the same limited sense as Popper's falsificationism provides a test-model for a crucial experiment (new evidence could invalidate it), and Feyerabend uses the term "crucial experiment" to describe his test-model in ERE (1962) specifically in this limited way. Specifically, for both Popper and Feyerabend, neither direct refutation of a theory by evidence, nor falsification of one theory by another is ever definitive in the sense of being decisive once and for all. Because theories always go beyond the evidence (due to the quantifier "all"), they

[72] Bohm argues that because theories always go beyond the evidence in this sense, they will always turn out to have only limited validity, see part 3.

[73] Feyerabend to Kuhn, 12 February 1988, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, p. 3321.

[74] On this transition, see Albert Einstein and Leopold Infeld, **The Evolution of Physics: From Early Concepts to Relativity and Quanta**, Cambridge University Press, Cambridge 1938.

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always remain *conjectural*, and tests are never definitive. Feyerabend embraces this view. By a "crucial experiment" between incommensurable rivals, he does not mean to imply it should be seen as definitive or final, just that it should be seen as decisive for the community based on the available evidence at that time:

> refutations are final only as long as ingenious and nontrivial alternative explanations of the evidence are missing. There can be no argument to the effect that the absence of such an alternative at a particular time will make the refutation final. It is very important to point out, in this connection, that the fate of many theories strongly depends upon the belief of their defenders that it will be possible, at some future time, to incorporate into them all the apparently refuting instances. [75]

In other words, for both Popper and Feyerabend, the issue is about justification given the available evidence at some point in time. Basically, the question is: When is the evidence sufficient for justifying replacing the current empirically best supported universal theory with a new theory? Popper's answer is that we should abandon the current theory when it has been falsified by a better one. For Popper, the new theory is better because it is better tested, as all the successful tests of the current theory also count as successful tests of the new theory, and in addition the new theory should predict (by deduction) a statement that is confirmed (the falsifier) that logically contradicts a prediction deduced from the current theory. Once a theory has been falsified in this way by a better-tested alternative, we should change from the old theory to the new. For Popper, its survival of the best-tested theory: The one that has survived the most tests without being falsified. As we shall see, by contrast, there is no falsifier in Feyerabend's model. There is only a corroborator.

It cannot be emphasized enough, that Feyerabend begins by explicitly saying he is focusing his discussion on testing "universal theories" that are always everywhere applicable [76] *and not* testing lower-level generalizations directly by inspecting instances. [77] Universal theories provide *frameworks* in which lower-level laws and theories can be tested, so that:

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[75] Paul K., Feyerabend, "Comments on Grünbaum's »Law and Convention in Physical Theory«", in: Herbert Feigl and Grover Maxwell (eds.), **Current Issues in the Philosophy of Science. Symposia of Scientists and Philosophers**, **Proceedings of Section L of the American Association for the Advancement of Science, 1959,** Holt, Rinehart and Winston, New York 1961, p. 158 [155–161].

[76] In this sense, thermodynamics is a universal theory for Feyerabend, presumably because everything everywhere that is something somewhere has temperature according to the kinetic theory.

> If a certain theory is replaced by a different theory with a different ontology, then we may have to revise the interpretation of *all our measurements*, however self-evident such a particular interpretation may have become in the course of time. [78]

Feyerabend distinguishes universal theories from empirical generalizations based on their respective test-procedures. According to Feyerabend, testing universal ("non-instantial") theories empirically must always involve a two-step test-procedure in contrast to testing laws and lower-level theories. The first step is a deduction to a lower-level law or theory, and the second step is a test of that law or theory by inspection of some particular instances deduced from it. Feyerabend's distinction between *universal theories* and *lower-level generalizations* is clarified in the very first footnote, which chastises "many thinkers" for treating these two cases as if they had the same test-procedures, just because they can be put in the same form.

> One should not be misled by the fact that universal theories, too, can be (and usually are) put in the form "All A's are B's"; for, whereas, in the case of generalizations, this form reflects the test procedure in a very direct way, such an immediate relation between the form and the test procedures does not obtain in the case of theories. *Many thinkers* have been seduced by the similarity of form into thinking that the test procedures will be the same in both cases. [79]

In this way, Feyerabend begins to set up his "big reveal" in the conclusion of the essay in the very first footnote. Just exactly who are these "many thinkers"? Given the conclusion, this turns out to be an implicit criticism of Popper, who emphasizes this shared form to explain his single test-model (falsificationism), which erroneously purports to take both kinds of cases (testing rival commensurable and rival incommensurable theories) into account, so that falsificationism mistakenly recommends one and the same test-model for what, according to Feyerabend, should be considered to be two different test-procedures: One for testing commensurable theories (falsification) within a framework, and one for testing competing incommensurable framework theories (Feyerabend's test-model).

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[77] As we shall see, Feyerabend qualifies this limitation later, suggesting his account should be extended to cover all cases that involve radical concpetual change, including lower-level laws and generalizations.

[78] Feyerabend, ERE (1962), p. 37 [emphasis added].

[79] Feyerabend, ERE (1962), p. 28, fn. 1 [emphasis added].

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According to Feyerabend, testing a universal framework theory is therefore a two-step procedure. More specifically, to test a universal theory, one must first deduce a law or lower-level theory, which then can be tested like a mere generalization of the form "All A's are B's" in a second step that predicts by deduction empirical evidence (some X that can be measured). Feyerabend tells us that testing generalizations of this form ("All A's are B's") by inspecting some instance X of what appears to be an A to see if it is also a B, by contrast, can be done in a single step. If some X which seems to be an A is also a B, then the instance can count as a successful test (a corroboration) of the generalization "All A's are B's". [80] If some X that is an A is found not to be a B, then X *directly refutes* the generalization. For example, a single black swan directly refutes the generalization that all swans are white.

However, Popper emphasizes that X becomes a *direct refutation* of the generalization all A's are B's, only if it can also be shown that X is also *really* an A — perhaps X may only appears to be an A. Only once X is also shown to be an A but not a B is, then "All A's are B's directly refuted by X". But it still has not been falsified, and so will not be abandoned. A falsification is an indirect refutation in which an *alternative* theory correctly predicts a statement (potentially explaining it) that logically contradicts a prediction deduced from the established theory. To falsify the generalization, we need a generalization that predicts (and therefore potentially explains) the black swan, while still also explaining why all swans previously observed were white. In other words, according to Popper, we should not jettison a theory or a generalization that works all of the time except for some apparent anomaly that directly refutes it, unless we have an alternative theory or generalization that explains the anomaly and shows why the existing theory or generalization only works up until that point.

According to Feyerabend, a statement can only be used as an explanation if it is assumed to be true, [81] and assuming the truth of a universal theory is and should be exactly what determines our worldviews. Universal theories shape

[80] For example, "That, over there, is a swan" (basic statement). "Why is it white?". "Because all swans are white" (explanation). "How do you know?". "So far, of the many swans seen, no one has ever seen a single swan that was not white" (evidence tests based on confirmed predictions deduced from "All swans are white").

[81] "*T* explains *T*' satisfactorily only if *T* is true [...]" (Feyerabend, ERE (1962), p. 93).

worldviews because they imply an ontology that is, and should be, used to try to explain all experience realistically. In other words, universal theories have and should have ontological implications. They imply what we should take to be real. Putting Feyerabend's views on the test-procedure required for testing universal theories together with his normative views on realism, is what led Feyerabend to his leading question: If universal theories determine every experience of reality, how can experience of reality be used to test universal theories?

Einstein's Influence on Feyerabend's Views as Set out in ERE (1962)

With respect to realism, Feyerabend saw himself as following Popper, Einstein, Bohm and others, who were all also advocates of some sort of realism (in contrast to instrumentalism). According to the interpretation of Feyerabend's theoretical pluralism suggested in this essay, Feyerabend's views seem to try to unpack many of Einstein's methodological remarks: [82] Feyerabend's position (Kant-on-wheels realism), his specific focus (universal theories), his leading question (how can they be tested if they condition all experience and can always protect themselves with *ad hoc* maneuvers), his use of "incommensurable" to characterize the challenge that has to be overcome to test universal theories (it is like trying to reciprocally weigh "incommensurable qualities", specifically because of *differences* in the "basic concepts" used to state the rival universal theories) — all of this seems to build on what Einstein says in the book containing his "Autobiographical reflections" (1949), where he recounts the Brownian motion example as a crucial experiment. [83] For this reason, it seems clear that Feyerabend adopted *the term* "incommensurable" used in this methodological sense from Einstein's use. It fits both with how Einstein used the term as applied to "basic concepts" of competing rival universal theories ("Of the »realm« of theories I need not speak here, in as much as we are confining ourselves to such theories whose object is

[82] See Oberheim, "Rediscovering Einstein's Legacy…".

[83] In Albert Einstein, "Autobiographical Notes", transl. Paul Arthur Schilpp, in: Paul Arthur Schilpp (ed.), **Albert Einstein: Philosopher-Scientist**, *Library of Living Philosophers*, Vol. 7, Open Court, La Salle 1949, p. 23 [3–95] and Albert Einstein, "Remarks Concerning The Essays Brought Together in This Cooperative Volume", in: Arthur Schilpp (ed.), **Albert Einstein: Philosopher-Scientist**, Open Court, La Salle 1949, pp. 665–688. In Feyerabend, "Explanation, Reduction and Empiricism…" (1962), Feyerabend cites Bohr's paper ("Discussion with Einstein", pp. 201–241) in this book, but not directly from Einstein's papers in this same book. See Oberheim, **Feyerabend's Philosophy…**.

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the totality of all physical appearance") [84] when trying to compare them empirically by their respective deductive consequences used as tests. [85] According to Einstein:

> The second point of view [of his point of view] is not concerned with the relation to the material of observation but with the premises of the theory itself, with what may briefly but vaguely be characterized as the "naturalness" or "logical simplicity" of the premises (of the *basic concepts and of the relations between these which are taken as a basis*). [86]

Einstein continues (directly):

> This [second] point of view, an exact formulation of which meets with great difficulties, has played an important role in the selection and evaluation of theories since time immemorial. The problem here is not simply one of a kind of enumeration of the logically independent premises [note Einstein equated these "premises" with the basic concepts and their relations in the preceding sentence] (if anything like this were at all unequivocally possible), but that of a kind of reciprocal weighing of *incommensurable* qualities. [87]

Einstein is saying that the basic concepts of universal theories have "incommensurable qualities" ("an exact formulation of which meets with great difficulties"). Feyerabend should be seen to be taking up that challenge, right where Einstein left off. Feyerabend, following Einstein, was trying to describe the challenge of testing universal theories that have incompatible basic concepts. The incommensurable quality of the basic concepts reflects the fact that they are deductively disjoint, which is why falsificationism cannot take meaning change into account, according to Feyerabend in 1962 in ERE. Einstein is claiming that the basic concepts used to state universal theories are different, which makes it challenging to compare them, because it is like trying to adjudicate on the basis of reciprocally

[84] Einstein, "Autobiographical Notes…", p. 23, same pages as his use of "incommensurable" he is restricting in the same way Feyerabend later does (to such "universal theories" in ERE (1962).

[85] See Einstein, "Autobiographical Notes…", and Einstein, "Remarks Concerning the Essays Brought Together…", which are published in the same book, which Feyerabend also cites in subsequent publications. See Collodel, "Was Feyerabend a Popperian…", pp. 27–56.

[86] Einstein, "Autobiographical Notes…", p. 23 [emphasis added].

[87] Einstein, "Autobiographical Notes…", p. 23 [emphasis added].

weighing incommensurable qualities. [88] Moreover, Einstein tried to explain these challenges posed to empirically testing universal theories (in contrast to lower-level generalizations, given their all-pervasive character and their ability to protect themselves from "ad hoc maneuvers"), [89] while recounting the methodological implications of his successful predictions of "Brownian motion" *as a crucial experiment* between universal theories. [90]

If that were not enough to establish that Feyerabend was adopting the term and trying to develop Einstein's methodological insights with respect to incommensurability of universal theories due to difference in their basic concepts, at the same time, Einstein, who was a realist, was also advocating a Kant-on-wheels position, similar to Feyerabend in ERE (1962). According to Einstein, his position:

> is distinct from that of Kant only by the fact that we do not conceive of the "categories" as unalterable (conditioned by the nature of the understanding) but as (in the logical sense) free conventions. They appear to be *a priori* only insofar as thinking without the positing of categories and of concepts in general would be as impossible as is breathing in a vacuum. [91]

Einstein also explicitly adopted Duhem's contextual theory of meaning: "the meaning of the experiments does not become apparent until theory makes it so", [92] which is very similar to Feyerabend's, "the interpretation of an observation language is determined by the theories that we use to explain what we observe, and it changes as soon as those theories change", [93] setting up the difference be-

[88] It seems worth mentioning that in ERE (1962), Feyerabend primarily applies "incommensurable" directly to "concepts" of "theories" before abbreviating to "incommensurable theories", which is just supposed to mean theories stated with incommensurable (or "deductively disjoint") concepts.

[89] On this Einstein was following Duhem, see Don Howard, "Einstein and Duhem", *Synthese* 1990, Vol. 83, No. 3, pp. 363–384 and compare Duhem, **The Aim and Structure of Physical Theory...** Compare Feyerabend: "The impression that every fact suggests one and only one interpretation and that therefore our views are "determined" by the facts, this [...] situation arises whenever a fairly general point of view was held long enough to influence our expectations, our language and thereby our perceptions, and when during that period no alternative picture was seriously considered. We may prolong such a situation either by explaining away diverse facts with the help of *ad hoc hypotheses*" (Feyerabend, "An Attempt at a Realistic Interpretation of Experience...".)

[90] See Oberheim, "Rediscovering Einstein's Legacy...".

[91] Einstein, "Remarks Concerning the Essays Brought Together...", p. 374.

[92] Einstein and Infeld, **The Evolution of Physics...**, p. 69.

[93] Feyerabend, "An Attempt at a Realistic Interpretation...", p. 163, see also ERE (1962).

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tween basic sentences and basic statements that, as we shall see, Feyerabend uses to criticize Popper's account in ERE (1962):

> Indeed, a whole theory may at some time turn out to be unsatisfactory, and the need may arise to replace it by a completely different idiom based upon a different point of view. Clearly, then, the interpretation of the observation sentences will have to follow suit, for again there is no way of conferring an interpretation upon them except by incorporating them into a new and better theory. [94]

Furthermore, Einstein (following Duhem) emphasized one of the challenges Feyerabend's (and Popper's) test-model is designed to overcome, the use of *ad hoc* maneuvers to protect a theory from direct refutation: "For it is often, perhaps even always, possible to adhere to a general theoretical foundation by securing the adaption of the theory to the facts by means of artificial additional assumptions". [95] Moreover, a closer reading of Einstein reveals that Einstein had even suggested:

> the general theory of relativity [...] showed that one could take account of a wider range of empirical facts, and that, too, in a more satisfactory and complete manner, on a foundation quite different than the Newtonian. But quite apart from the superiority of one or the other, the fictitious character of fundamental principles is perfectly evident from the fact that we can point to two essentially different principles, both of which correspond with experience to a large extent; this proves at the same time that every attempt at a logical deduction of the basic concepts and postulates of mechanics from elementary experience is doomed to failure. [96]

As theory choice can be underdetermined by the evidence (the same evidence can be explained by "quite different" theories), there is no unique deductive relation between the evidence and the competing theories used to explain it. Feyerabend's idea of incommensurable as deductively disjoint can be seen to follow from such reflections. Methodologically speaking, the problem with the all-pervasive character of universal theories that condition all experience is what raises the question Feyerabend is asking. Pluralists, following Duhem (a crucial experiment

[94] Feyerabend, ERE (1962), p. 40.

[95] Einstein, "Autobiographical Notes...", pp. 22–23, same pages as "incommensurable" and "basic concepts".

[96] See Albert Einstein, "On the Method of Theoretical Physics. The Herbert Spencer Lecture, Delivered at Oxford, June 10, 1933", *Philosophy of Science* 1934, Vol. 1, No. 2, pp. 163–169, http://dx.doi.org/10.1086/286316.

attempts to refute every theory but one), especially Popper and Einstein, argue that without an alternative theory that could potentially explain an anomalous observation, existing theories can always be protected from anomalous observations by *ad hoc* maneuvers. This worry is emphasized by Feyerabend and by other pluralists, such as Popper and Einstein, and as we shall see, Popper's falsificationism is specifically designed to try to prevent this maneuver. [97] For Popper, the new theory cannot be *ad hoc* just because it should logically contradict a prediction deduced from the existing theory to falsify it. In other words, with falsificationism, Popper had already tried to solve this part of the problem, whereas Feyerabend is going to challenge the applicability of Popper's solution to testing rival incommensurable theories.

After he awoke from his Popperian slumber in December 1967, Feyerabend went on to defend his infamous epistemological anarchism, according to which there is no fixed scientific method ("anything goes"), explicitly citing Einstein:

> the external conditions, which are set for [the scientist] by the facts of experience, do not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist. [98]

ERE (1962) is the first time Feyerabend argues that Popper's epistemological system does exactly that; that is, Popper's methodology puts too much adherence on an epistemological system (falsificationism). Feyerabend's criticism of Popper's methodology in ERE (1962) is that it puts too much restriction on the admissibility of new theoretical proposals by implicitly sharing a conservative commitment to meaning invariance [99] with the dogmatic school philosophies from which it tries to distance itself. School philosophies that do not allow for meaning change do not make progress. They continue to struggle with traditional philosophical problems that should dissolve with meaning change in the course of scientific advance.

Given that Einstein set out a Kant-on-wheels realism and used the term "in-

[97] See Popper quoted by Feyerabend in Feyerabend, ERE (1962), pp. 92–93.

[98] Einstein, "Remarks Concerning the Essays Brought Together…", p. 684 see also Feyerabend, **Against Method…** (1975), pp. 10–11 and compare ERE (1962).

[99] According to Feyerabend, meaning invariance is required for formal relations between their corresponding predictions to be established.

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commensurable" to try to describe the source of a challenge that arises when comparing universal theories because of differences in their basic concepts while recounting how his predictions of Brownian motion served as a crucial experiment in a book Feyerabend cites in ERE (1962), and given that Feyerabend tried to develop other notions Einstein floated about methodology, it seems nearly conclusive that with his use of the term "incommensurable", Feyerabend was trying to develop Einstein's views about challenges posed to testing universal theories given the possibility of protecting them from direct refutation with *ad hoc* maneuvers, given the differences in their basic concepts, which allow universal theories to play the role of Kantian frameworks (but without fixed categories, that is, with categories that colud be replaced by the next revolution). Einstein had used "incommensurable" applied to the same thing (theories), restricted in the same way (to universal theories), resulting from the same thing (differences in their basic concepts) during a discussion of his prediction of "Brownian motion" as a crucial experiment between such theories. If Feyerabend were not following Einstein directly, all of this would have to be quite a remarkable coincidence. [100]

None of this is intended to suggest that Feyerabend had not already arrived at many of the underlying ideas in ERE (1962) with respect to incommensurability and methodology before he read Einstein's remarks on incommensurability, nor to suggest that many other figures (such as Philipp Frank and Herbert Feigl) did not play substantial roles in the development of Feyerabend's ideas. However, it seems very likely that Feyerabend had read Einstein's remarks (he cites Bohr's paper in same book in ERE), and that he was clearly following Einstein's use of the term "incommensurable", when he first develops his criticism of Popper's methodology in ERE (1962) as illustrated by the Brownian motion example. In sum, following Einstein, Feyerabend's position in ERE (1962) is a form of Kant-on-wheels realism that raises his leading question: If universal theories condition all possible experience of reality, then how can experience of reality be used to test *incommensurable* universal theories? As we shall see, his test-model is devised to answer exactly this question.

[100] See Eric Oberheim, "On the Historical Origins of the Contemporary Notion of Incommensurability: Paul Feyerabend's Assault on Conceptual Conservativism", *Studies in the History and Philosophy of Science A* 2005, Vol. 36, No. 2, pp. 363–390, http://dx.doi.org/10.1016/j.shpsa.2005.04.003 and Oberheim, "Rediscovering Einstein's Legacy…"; Oberheim and Hoyningen-Huene, "The Incommensurability of Scientific Theories...". On Einstein and Duhem, see Howard, "Einstein and Duhem…".

1.4 The Framework: Synthesizing Wittgenstein and Popper

Feyerabend's theoretical pluralism attempts to synthesize Wittgenstein and Popper's views, resulting in the idea of incommensurability limiting the validity of Popper's falsificationism. In ERE (1962), Feyerabend tries to combine critical rationalism (try to increase testability, to protect and promote progress), with Wittgenstein's nominalism (terms have no meaning independently of how they are used in actual, material contexts, they are dispositions to react and potentially reinterpret) and Wittgenstein's constructivism [101] (we live in worlds of facts, not things), as what he took to be Wittgenstein's "contextual theory of meaning", according to which meanings of terms are determined by their theoretical context, i.e. the theories used to interpret them. [102] This is the shared framework: Facts are theoretical constructs for both Popper and Wittgenstein. Feyerabend explicitly traced Wittgenstein's constructivist approach to ... wait for it ... *Popper* (as well as Gomperz, Mach, and D'Alembert). [103]

In Wittgenstein's writings, Feyerabend found a "new (instrumentalist, nominalist, or whatever you like to call it) theory of meaning", which he characterizes as part of an "intuitionist (pragmatist, constructivist) theory of language" that can be understood as "constructivism applied not only to the meanings of mathematical expressions but to meanings in general". [104] As Feyerabend saw it, in this respect, by rejecting Einstein's characterization of scientific research as an extension of common sense, Mach "became one of the forerunners of Gestalt psychology, constructivism in mathematics, Piaget, Lorenz, Polanyi and Wittgenstein". [105] On constructivism, Feyerabend (and Popper) thought Wittgenstein was getting

[101] On the infamous "poker incident", see and David Edmonds and John Eidinow, **Wittgenstein's Poker**, Faber and Faber, London 2001.

[102] According to Feyerabend ERE (1962), p. 68, "the contextual theory of meaning has been defended most forcefully by Wittgenstein".

[103] Paul K. Feyerabend, "Wittgenstein's **Philosophical Investigations**", *The Philosophical Review* 1955, Vol. 64, No. 3, p. 482 [449–483], https://doi.org/10.2307/2182211.

[104] Feyerabend, "Wittgenstein's **Philosophical Investigations...**", p. 462.

[105] Feyerabend, "Putnam on Incommensurability...", p. 206.

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credit for ideas Popper had developed earlier and more clearly. [106] As we shall see, incorporating what Feyerabend took to be Wittgenstein's contextual theory of meaning into Popper's critical rationalist methodological framework turned out to be extremely fruitful for Feyerabend, as the result was the idea of incommensurability, at the heart of Feyerabend's particular form of theoretical pluralism, with which he tried to criticize Popper on methodology. Basically, in ERE (1962), Feyerabend's reasoning was as follows: if meanings are determined by their theoretical context (i.e. the theories used to interpret them), then meaning variance can render theories incommensurable (deductively disjoint) so that falsificationism is wrong, because new universal theories cannot explain antiquated facts.

So the synthesis came at great costs on both sides. The price to be paid for Wittgenstein's philosophy was Feyerabend's rejection of both Wittgenstein's dictum that meaning is use and of Wittgenstein's alleged quietism, but not Wittgenstein's conception of genuinely philosophical (pseudo-) problems, which, as we shall see, Feyerabend tried to explain. As indicated, the price to be paid for Popper's philosophy was the universal validity of falsificationism, which had to be sacrificed on the altar of incommensurability. In other words, Feyerabend used (what he took to be) Wittgenstein's contextual theory of meaning to criticize Popper's falsificationism, based on the idea of incommensurability (deductive disjointedness), which limits the validity of falsificationism as a methodological rule (setting the stage for "anything goes!", that is, there are no universally valid methodological rules, not even falsificationism).

Exactly how Feyerabend came to meet Wittgenstein and how their relationship unfolded setting the stage for this attempted synthesis in ERE (1962) are both extremely interesting stories, [107] but they are long, excessively complicated and there are too many fascinating details, which will have to wait for another occasion to be examined. Even so, understanding how and why Feyerabend attempted this synthesis is extremely helpful for understanding what Feyerabend

[106] See Feyerabend, "Wittgenstein's **Philosophical Investigations...**, in German, Paul K. Feyerabend, "Wittgenstein und die Philosophie, II", *Wissenschaft und Weltbild: Monatsschrift für alle Gebiete der Forschung* 1954, H. 7, pp. 283–287 and Paul K. Feyerabend,"Wittgenstein und die Philosophie, I", *Wissenschaft und Weltbild: Monatsschrift für alle Gebiete der Forschung* 1954, H. 7, pp. 212–220; and compare Paul K. Feyerabend, "Ludwig Wittgenstein", *Merkur: Deutsche Zeitschrift für europäisches Denken* 1954, H. 8, pp. 1021–1038.

[107] See for example Eric Oberheim and John Preston, "Paul Feyerabend", in: Edward N. Zalta (ed.), **The Stanford Encyclopedia of Philosophy** 2025, https://tiny.pl/1-qkcdnm [06.08.2025].

was trying to do with ERE (1962), so it seems worthwhile pausing to reflect on how this attempted synthesis came about as, Wittgenstein and Popper were also Feyerabend's two living idols in philosophy (even if Wittgenstein had subsequently died) when by the time Feyerabend wrote ERE (1962).

Although Feyerabend's only initially uses the term "incommensurable" in ERE (1962) where he first develops it as a part of a criticism of Popper's methodology, Feyerabend's idea of incommensurability can be seen in many of his prior papers, stretching all the way back to his 1951 Ph.D. thesis: *On the Theory of Basic Statements* [My translation], [108] which Feyerabend had developed in discussions with the Kraft circle. According to Feyerabend's later recollections, in the early 1950s, he had initially presented his gesticulating views on incommensurability in both Popper's seminar and to a group of Wittgensteinians: [109]

> Popper had left me cold, Wittgenstein set me afire. His ideas of meaning were a very essential ingredient of the idea of incommensurability. I first explained this idea at a private meeting in Anscombe's household (with Hart and von Wright present) in 1952 and then again in Popper's seminar in the same year. [110]

In a letter to Kuhn in 1983, Feyerabend explained that his basic idea in 1951 was then developed through Feyerabend's rejection of Carnap's double language model in the1950s:

> Before that [his later version of incommensurability in Ch. 17 of AM] I came to incommensurability (one narrow aspect of it) via a critique of the double language model. In Minnesota where I spent two years (in installments) the situation was presented thus: there is a theoretical language, there is an observation language and meaning flows from the observation language to the theoretical language by "upward seepage" (this is how Paul Meehl, the psychologist expressed the matter). In my thesis (1951) I tried

[108] See Feyerabend, **Zur Theroie Zur...**. See Oberheim, "On the Historical Origins…".

[109] Feyerabend to Bechler 1, January 1976, in Feyerabend, "Autobiographfical Remarks 1976...", p. 28 [19–44]. Feyerabend initially presented his views on scientific change to Popper's LSE seminar and to a gathering of illustrious Wittgensteinians, including Elizabeth Anscombe, Peter Geach, H. L. A. Hart and Georg Henrik von Wright in Anscombe's Oxford flat, see Oberheim and Preston, "Paul Feyerabend...". These meetings seems to have been the first airings of Feyerabend's *idea* of incommensurability, although the term itself, only appears in Feyerabend, "Explanation, Reduction and Empiricism…" (1962) a decade later. Some form of the basic *idea* appears in many prepublications, see Oberheim, "On the Historical Origins…".

[110] See Paul K. Feyerabend, **Killing Time**, The University of Chicago Press, Chicago — London 1995 and Oberheim and Preston, "Paul Feyerabend...".

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> to show that observations taken by themselves cannot determine meanings. I therefore suggested a "downward seepage" theory i.e. I suggested that all there is are theoretical meanings, all terms are, semantically speaking, theoretical terms and the difference between observation and theory has nothing to do with meaning. [111]

Even so, as we shall see, an essential ingredient of Feyerabend's theoretical pluralism was also Carnap's distinction between the pragmatic properties of a language that determine whether a statement is an observation statement and its interpretation according to some theory, that is, the meaning it is given according to some theory that purports to explain it; or in other words, the distinction between its use as an observation statement and its meaning (its content, what it implies). Feyerabend is trying to suggest that while we interact casually with sentences based on agreements about how to use them correctly, we also interpret and can reinterpret sentences according to different theories, which is what turns those sentences into statements that can be explained by those theories. As we shall see, this Carnapian distinction becomes central to Feyerabend's account of the relation between the predictions deduced from rival incommensurable theories that he uses to criticize Popper's methodology: Rivals use the same sentences to make different statements with respect to the same measurement results, which is an insight he illustrated with the relationship between impetus and momentum, which is rather cryptically discussed in ERE (1962). Feyerabend says when Herbert Feigl pointed out to him that his top-down contextual theory of meaning would make it impossible for incommensurable theories to share common semantic elements. His response implies that crucial experiments between incommensurable theories do not require common semantic elements:

> To this Feigl objected that basing meanings on theories makes nonsense of crucial experiments between vastly different theories — such theories will have no common semantic elements. I replied that crucial experiments do not need common semantic elements, all that is needed are certain behavioral overlaps. Semantically, I said, the theories are incommensurable but that does not matter, for scientists do not argue in accordance with philosophical semantics. All of this I explained in a paper that appeared in 1958 in the *Proceeding of the Aristotelian Society* (Feigl's objection and my reply in footnote 21 of the reprint in Vol. I of my *Philosophical Papers*) though without the word "incommensurable". [112]

[111] Feyerabend to Kuhn, 4 April 1983, in Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, p. 314.

However, as we shall see, although Feyerabend developing the idea of incommensurability throughout the 1950s, he only explicitly turns it into a criticism of Popper's methodology in ERE (1962), when he uses Bohm's Brownian motion example and first starts using the term "incommensurable" (following Einstein's use of the term in that way). For example, in "An Attempt at a Realistic Interpretation of Experience" (Feyerabend 1958), while Feyerabend acknowledges that his idea of incommensurability makes crucial experiments based on "common semantic elements" impossible (as Feigl has pointed out to him), there is no criticism of Popper's methodology, nor is this the main point of Feyerabend's criticism of Popper's methodology in ERE (1962), which develops a problem with explanation in Popper's methodology. [113]

So while Feyerabend's idea of incommensurability clearly had its Wittgensteinian origins in a contextual theory of meaning, and Carnap's distinction between the causal properties of sentences and interpretations of them as statements, and while he developed the idea in methodology within a critical rationalist framework in the 1950s and beyond, he only initially explicitly turned it into a criticism of Popper's methodology in ERE (1962), where he rejects Wittgenstein's dictum that meaning is use, [114] maintaining a significant distinction between meaning and use, insisting that "meaning and application are not the same thing". [115] According to Feyerabend, meaning is not just any old use. Statements get, and should get, their meaning from the theories whose truth are presupposed when they are used to describe or explain some feature of the world. When used as tests of theories, sentences should get their meaning from their theoretical context, specifically from the theories from which they can be deduced (and thereby

[112] Feyerabend to Kuhn, 4 April 1983, in Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, pp. 314–315. Compare: "This difficulty was pointed out to me by Professor H. Feigl. I should like to add at this point that discussions with Professor Feigl and the members of his Centre have greatly helped me to clarify my ideas" (Feyerabend, "An attempt at a realistic interpretation...", p. 163, fn. 22 [143–170]; Feyerabend, "An Attempt at a Realistic Interpretation...", in: Feyerabend, Philosophical Papers, Vol. 1..., p. 31, n. 21; and compare Feyerabend, ERE (1962), p. 94, fn. 115.

[113] See Feyerabend, "An Attempt at a Realistic Interpretation...".

[114] See "the meaning of a word is its use in a language" (Ludwig Wittgenstein, **Philosophical Investigations**, Gertrude Elizabeth Margaret Anscombe and Rush Rhees (eds.), trans. G. E. M. Anscombe, Blackwell, Oxford 1953, I§43.

[115] Feyerabend, ERE (1962), p. 57.

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used to test and explain). Feyerabend rejected the distinction between theoretical and observational statements as different in kind because of their meanings, [116] and he emphasized with Popper that observations statements can and should also be tested. [117] By 1960, Feyerabend had already investigated the problem of the existence of *theoretical* entities (as opposed to directly observable ones) and concluded that there is no such problem, [118] because all entities are *equally* theoretical. In other words, observation statements are *just as* theoretical and should be just as open to test and correction as the theories that explain them.

However, Feyerabend (following Mach, Einstein, and others) goes even further to emphasize that observation statements can be harder to test than the theories used to explain them. They can be harder to test exactly because they already seem to be so well-tested. As we shall see, this differs ever so slightly from Popper and the Popperians, who according to Feyerabend, continue to give basic statements a special, privileged status, as the "empirical core" of *the* empirical method. For Popper, statements about observables are theory-laden, but they are always more secure than general statements (theories), because they are more easily testable (and so are better tested) than the theories that are tested by them (and thereby potentially explained by them). In contrast, in his early philosophy of science, Feyerabend argues that empiricism should be disinfected of its empirical core because the empirical statements used to test theories (Popper's "basic statements") should not be more stable and secure than the theories they are used to test, because "[e]ven the stability of a testable hypothesis cannot be regarded as a sign of its truth [. . .]"; as according to Feyerabend, "There is no sign by

[116] See also Paul K. Feyerabend, "Das Problem der Existenz theoretischer Entitäten", in: Viktor Kraft and Ernst Topitsch (eds.), **Probleme der Wissenschaftstheorie: Festschrift für Viktor Kraft**, Springer, Vienna 1960, pp. 35–72; Paul K. Feyerabend, "The Problem of the Existence of Theorethical Entities", in: John Preston (ed.), **Paul K. Feyerabend: Knowledge, Science and Relativism**, *Philosophical Papers,* Volume 3, Cambridge University Press, Cambridge 1999, pp. 16–49, which directly sets the stage for these views as set out in ERE (1962) in this respect.

[117] "I interpreted crucial experiments *as practical decisions not as theoretical comparisons* of statements with stable meanings. In 1962, I gave some examples of the difficulties Feigl seemed to have in mind and called them »incommensurable«" (Feyerabend to Kuhn, 12 February 1988, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**). As we shall see, in this way Feyerabend was developing a form of pragmatism.

[118] See Feyerabend, "The Problem of the Existence of Theorethical Entities..." (original German, 1960).

which factual truth may be recognized". [119] Theories cannot be proven to be true. They are conjectures, just as observation statements remain conjectures, no matter how well-established empirically they may appear to be. In other words, following Mach and Einstein, Feyerabend emphasizes that the observational success of the existing theory does not make observations statements more secure, nor should observation statements be more secure on the grounds that they are better tested than the theories they are used to test. [120] Rather, it is exactly because they *seem* more secure that they pose an obstacle that must be overcome in order for a revolutionary reconceptualization of everything, according to a new and counterintuitive incompatible theoretical proposal (for example, in which space-time bends and stretches), to succeed. [121] Feyerabend, like Einstein, recognized that because revolutionary theoretical developments affect even the most commonplace concepts, initial steps will at first appear unreasonable. [122] They involve what Feyerabend later calls a "counter-inductive" step. [123]

In ERE (1962), Feyerabend argues that how terms are typically used may change very little, even after a scientific revolution has radically changed what they mean. Feyerabend illustrated such meaning variance with examples ranging from the word pair "up-down" (from absolute to relative) [124] to "heat" (from substance to molecular motion), [125] and to "fall" (from pushed by impetus to pulled by gravity). [126] However, according to Feyerabend, *terms can be used exactly in the*

[119] Feyerabend, ERE (1962), p. 70, fn. 86.

[120] Compare Einstein: "one does not easily become aware of the free choice of such concepts, which through verification and long usage, appear to be immediately connected with the empirical material" (Einstein, "Autobiographical Notes…", p. 49).

[121] On Feyerabend's relation to Einstein's philosophy of science, see Oberheim, "Rediscovering Einstein's Legacy…".

[122] Einstein even once lamented: "Oh, how hard things are for man in this world; the path to originality leads through unreason (in the sciences), through ugliness (in the arts) at least the path that many find passable" (Einstein 1918, as cited in Howard, "Einstein and Duhem…", p. 368).

[123] See Feyerabend, **Against Method…** (1993), pp. 53 and 78.

[124] See Feyerabend, ERE (1962), p. 85. Another of Feyerabend's favorite examples is "color" (from a property of an object to a relation that varies with the relative motion between the object and the observer).

[125] See Feyerabend, ERE (1962), p. 80.

[126] See Feyerabend, ERE (1962), pp. 53–56.

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same way, even as part of the same measurement procedure in exactly the same material context, but still they will mean radically different things depending on which theories are used to interpret them. [127] Feyerabend cites Clagett on this point, who explains the procedures for measuring both impetus and momentum used in the Middle Ages (by comparing measurements of how far a ball "falls" into a block of wax when dropped from various heights). Feyerabend's text in ERE (1962) is sometimes intentionally cryptic for dramatic purposes (most notably, setting up his big reveal), but other times it is inexcusably cryptic. Feyerabend should have explained these test procedures and not left it to the reader to consult Clagett to understand what Feyerabend is talking about (and what he is trying to say), which is that with respect to dropping a ball into wax to measure impetus and momentum, the same measurement procedures produce quantitatively identical results, and yet, at the time, a single sentence expressing those results had radically different meanings depending on which theory was used to interpret it. [128] Feyerabend concludes that while it,

> is correct that the measure of this magnitude (*viz.,* mv) is identical with the measure that has been suggested for the impetus. However, it would be very mistaken if we were, on that account, to identify impetus and momentum. For whereas the impetus is supposed to be something that pushes the body along, the momentum is the result rather than the cause of its motion". [129]

This is supposed to be an example of transient underdetermination of theory by evidence, as the same measurements equally support rival theories. [130] The

[127] This is Feyerabend's main example of this central point in ERE (1962). Retrospectively, while recounting his route to the idea of incommensurability in ERE (1962), Feyerabend pointed out to Kuhn that this insight was quite a revelation in the development of his idea of incommensurability, "Reading Anneliese Maier's *Die Verläufer Galileis im 14. Jahrhundert* where she emphasizes the difference between impetus and momentum played a great role here. This was a real revelation to me" (Feyerabend to Kuhn 12 February 1988, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, citing Anneliese Maier, "Die Verläufer Galileis im 14. Jahrhundert: Studien zur Naturphilosophie der Spätscholastik, Edizioni di Storia e letteratura: Roma 1941" (review), *Philosophia Naturalis* 1950, Vol. 1, No. 1, p. 166.

[128] Feyerabend cites Marshall Clagett, **The Science of Mechanics in the Middle Ages**, University of Wisconsin Press, Madison 1959, p. 523.

[129] Feyerabend, ERE (1962), p. 56.

[130] See Paul Hoyningen-Huene, "Reconsidering the Miracle Argument on the Supposition of Transient Underdetermination", *Synthese* 2011, Vol. 180, No. 2, pp. 173–187.

classic example is the three incompatible explanations of the same measurement results recorded in the Ptolemaic tables — Ptolemy, Copernicus, and Brahe's, before new evidence broke the deadlock with new observations, for example, of the phases of Venus. Just because they used the same sentences to express the same measurement results does not imply there is no meaning variance. Later, Feyerabend explicitly warned of the pitfalls in the idea that invariance in use indicates invariance in meaning, ridiculing the idea:

> The occurrence of such changes [conceptual revisions that do not change how everyday words are typically used] quite obviously refutes the contention of some philosophers that the invariance of *usage* in the trivial and uninteresting contexts of the private lives of not-too-intelligent and inquisitive people indicates invariance of *meaning*. [131]

Feyerabend thought meaning should be *correct* use, where correct means correct according to a *realistic interpretation of our best theories*, [132] and Feyerabend often argued that we should use our best theories to correct "everyday language" accordingly [133] — until he changed his mind about this, and gradually became a chief critic of the tyranny of scientism. [134]

By *correcting* everyday language, Feyerabend explicitly means *replacing* the meanings of all our empirical terms, as universal theories are purportedly always applicable to everything happening everywhere, with new meanings determined by new ontological principles. Everything (literally!) should be reinterpreted according to the new kind of world that the new universal framework theory makes accessible: If everyday language is not corrected by *replacing* all the old meanings, but just by trying to add new ones, then we run the risk of ending up in a Wittgen-

[131] Feyerabend, "Problems of Empiricism…", p. 180.

[132] See especially Feyerabend, "An Attempt at a Realistic Interpretation…", Feyerabend, ERE (1962); and Feyerabend, "Realism and Instrumentalism…", pp. 280–308.

[133] See especially Feyerabend, "Explanation, Reduction and Empiricism…" (1962), pp. 84f; Paul K. Feyerabend, "Reply to Criticism: Comments on Smart, Sellars and Putnam", in: Robert S. Cohen and Marx W. Wartofsky (eds.), **Proceedings of the Boston Colloquium for the Philosophy of Science 1962–1964: In Honor of Philipp Frank, Boston Studies in the Philosophy of Science, Vol. 2**, Humanities Press, New York 1965, pp. 223–261; Feyerabend, "On the »Meaning« of Scientific Terms…" and Paul K. Feyerabend, "Linguistic Arguments and Scientific Method", *Telos* 1969, Vol. 1969, No. 3, pp. 43–63, https://doi.org/10.3817/0369003043.

[134] See especially Paul K. Feyerabend, **The Tyranny of Science**, Eric Oberheim (ed.), Polity, Cambridge 2011.

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steinian "philosophical fog". According to Feyerabend, the antiquated ontological principles implied by antiquated worldviews,

> must therefore be either abandoned and replaced by the language of the new and better theories even in the most common situations, or they must be completely separated from these theories (which would lead to a situation where it is possible to believe in various kinds of "truth". [135]

In his early philosophy, Feyerabend seems to recommend both! He recommends replacing ontological principles in everyday language given progress in science, and he recommends relativizing truth to worldview as part of an ever-expanding ocean of alternatives. For Feyerabend, there is no truth simpliciter. There is only truth according to this theory, in contrast to truth according to that theory. Feyerabend is insisting on having only one set of truths per language at a time on pain of incoherence. Mixing incompatible ontological principles, according to Feyerabend, is what causes the type of cognitive dissonance characteristic of Wittgensteinian philosophical pseudo-problems. Feyerabend's view is nuanced: Although they should be *replaced* with respect to a realistic interpretation of experience, they should not be forgotten or *abandoned*, because they can still contribute to the empirical content of contemporary theories as part of an ever-increasing ocean of alternatives that are better understood in contrast to each other than by themselves. In other words, Feyerabend was indeed led to a situation in which it is possible to believe in various kinds of hypothetical truths (classical and relativistic) in different contexts, given that whether a statement counts as true is relative to the worldview of which it is part, while still insisting that we should interpret experience realistically, according to our best explanations of it.

Why did Feyerabend change his mind with respect to the views developed in ERE (1962) in the late 1970s? As Feyerabend saw it, Wittgenstein's point that science is more than abstract rule*s,* that *it contains forms of life and entire traditions* finally belatedly sunk in:

> It was Wittgenstein's great merit to have spotted and criticized this procedure [assuming that explicitly formulated structural elements are the only operative ingredients in a language, and then restricting discussions of scientific theories to such struc-

[135] Feyerabend, ERE (1962), p. 76, compare David Bohm, **Causality and Chance in Modern Physics**, Routledge Kegan Paul, London 1957.

> tural elements] and the mistake that underlies it and to have emphasized that science contains not only formulae and rules for their application but entire traditions. [136]

By the mid. to late 1970s., Feyerabend begins to emphasize that science is not reducible to abstract rules. It contains whole traditions and as such, it is just one form of life among many. Moreover, common sense and other traditions and forms of life need to be protected from science, not corrected by it. In this way, Feyerabend *reversed his views on the relationship between science and common sense with respect to what we should believe is real.* In his early work, and in ERE (1962) in particular, common sense should be revised with respect to what is real given scientific advance for ethical reasons. [137] Feyerabend emphasized the ethical basis of methodological decisions, and specifically the ethical basis of his realism (science is what we make it and what we make of it), [138] but Feyerabend never raises any potentially adverse ethical consequence of revising common sense according to his realism in this early work. However, after his reversal, this is clearly no longer the case:

> We therefore have to make a choice: do we want scientific efficiency, or do we want a rich human life of the kind now known to us and described by our artists? The choice concerns the quality of our lives — it is a moral choice. [139]

In his early work, the goal is progress in science, because ideology leads to violence, whereas in his late works, scientific progress has become part of the problem, because of how it is too often made (by objectifying people in a way that dehumanizes humans) at the cost of other traditions. More specifically, in ERE (1962), the ethical justification for his normative test model is ultimately to promote scientific progress to prevent violence and war, and the tendency toward intolerance of one's opponents that results from the entrenchment of ideology. [140] In his late work, common sense needs to be protected from the tyranny of scientism. [141] In Feyerabend's early philosophy and in ERE (1962), instrumentalism is

[136] Feyerabend, **Science in a Free Society…**, p. 66.

[137] See Feyerabend, "Realism and Instrumentalism…", pp. 280–308.

[138] Most explicitly in Feyerabend, "Realism and Instrumentalism…", pp. 280–308.

[139] Added to Feyerabend, "Realism and Instrumentalism…", pp. 280–308.

[140] See, for example, Feyerabend, ERE (1962), pp. 60–61.

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a philosophy of defeat (following Popper, Bohm and Einstein), [142] in contrast to his early,

> realism [which] invites us to reject common sense and to announce the discovery: objective reality has been found to be a metaphysical mistake. [143]

By contrast, in Feyerabend's late philosophy,

> instrumentalism is not a philosophy of defeat, it is often the result of far-reaching ethical and political decisions. Realism, on the other hand, only reflects the wish of certain groups to have their ideas accepted as the foundations of an entire civilization and even of life itself. [144]

In sum, Wittgenstein was influential with respect to Feyerabend's views on meaning as developed in his early philosophy and as set out in ERE (1962), and he was influential with respect to his reversal from correcting common sense with science to protecting common sense from science, which becomes characteristic of his later work.

How did Feyerabend get directly involved with Wittgenstein and his philosophy? In 1951, Feyerabend invited Wittgenstein (in a letter he personally delivered to Wittgenstein's home) to come to the Kraft circle to discuss "basic statements" (using Popper's term). Anscombe, who was attending Kraft circle meetings and was one of Wittgenstein's most trusted students (she translated and coedited the German manuscripts that he had entrusted to her) had recommended writing Wittgenstein a letter of invitation and advised Feyerabend not to be too subservient. Feyerabend recounted he wrote roughly one line: *"we are discussing **basic statements** and we are **stuck**"*. [145] Feyerabend must have thought long and hard about how to push all the right buttons. This is code for we are discussing the foundations of "Popper's empiricism", with an invitation to set it straight, given

[141] On Feyerabend's later political philosophy, see especially Ian Kidd, "Feyerabend, Pseudo-Dionysius, and the Ineffability of Reality", *Philosophia* 2012, Vol. 40, pp. 365–377; Kidd, "»What's So Great about Science?« Feyerabend...", pp. 55–76; Kidd, "Reawakening to Wonder Wittgenstein, Feyerabend and Scientism...", pp. 101–115; Kidd, "Feyerabend, Science and Scientism...", pp. 172–190.

[142] See Feyerabend, ERE (1962), pp. 30–32, 40–41, 71, and 82.

[143] Feyerabend, **Realism, Rationalism and Scientific Method...**, p. xii.

[144] Feyerabend, **Realism, Rationalism and Scientific Method...**, p. xi.

[145] Feyerabend, "Wittgenstein's **Philosophical Investigations...**, p. 75.

that they are "stuck" — without even mentioning Popper. Marvelous. In retrospect, as we shall see, the reason they were stuck was that in Popper's account the role that basic statements play is incompatible with Feyerabend's understanding of Wittgenstein's contextual theory of meaning. This is exactly what was finally *unstuck* in ERE (1962) with a new test-model that jettisons the idea of basic statements as the empirical core of science, while limiting the validity of falsificationism to testing theories within shared frameworks. In any case, it should be clear that Wittgenstein would not have thought much of Popper's account of basic statements, and this was supposed to be an invitation to come set them straight by clarifying what is wrong with it. At the time, Anscombe reported back to Feyerabend that Wittgenstein had liked the letter but was unsure if he would attend. However, Wittgenstein did surprisingly show up. At the meeting, Feyerabend had begun summarizing what they had discussed in earlier meeting, while adding a few points of his own, when Wittgenstein, having arrived over an hour late took a seat and listened to Feyerabend for a few moments, before interrupting: "*Halt, so geht das nicht*" ("Stop that won't work") and discussed in detail what one sees when looking through a microscope. [146]

Feyerabend's increasing deemphasis on the meaning of observations in science and his increasing emphasis on meaning as connection to other people ultimately culminates in his recommendation to spend life in pursuit of love in his autobiography **Killing Time** (1995). Feyerabend, it would seem, finally saw that there are facts of common sense, and that their stability supports their ability to connect people from widely different backgrounds, which should count in their favor. He had finally become a realist about the people and their problems that he could see with his own eyes right in front of him, and not about abstractions and the kinds of things you can only calculate or see through a microscope.

Popper, of course, considered Wittgenstein to be his nemesis or archrival in philosophy. So it should come as no surprise that trying to synthesize their views would to come at some cost. So, it is worth emphasizing from the start that Feyerabend was also extremely critical of what he took to be Wittgenstein's philoso-

[146] Feyerabend, "Wittgenstein's **Philosophical Investigations...**, p. 76.

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phy [147] and linguistic philosophy [148] more generally in a discernably Popperian manner. [149] Popper's influence on Feyerabend's critical reconstruction of Wittgenstein's *Investigations* can be seen very clearly in the footnotes of his critical review published in 1955. The first reference is to Popper's **The Open Society and Its Enemies**. [150] Later in footnotes, he points out that psychoanalysis has introduced a "picture-language" so that it is inconceivable how it could possibly be refuted, and in the final footnote he again directs us to Popper's criticism of essentialism, developed *as early as 1935*, or in other words, before Wittgenstein (an important point from Popper's perspective). The criticism of Wittgenstein's philosophy is very Popperian too. For example, Popper says Freudianism *reinforces dogma* by protecting itself from criticism by making how it could ever be refuted seem inconceivable. Similarly, for Popper, if you try to disagree with Wittgenstein's philosophy, the Wittgensteinian response is too often that you must have misunderstood it. Your quarrel must be merely with what they said, not with what they were trying to show. Similarly, according to Feyerabend, Wittgenstein's philosophy *reinforces dogma* by protecting itself from criticism with "quietism" (not having said anything to contradict), as well as for claiming to have left everything as it is.

This is how Feyerabend launched what came to be known as the "quietist" interpretation of Wittgenstein's philosophy in Feyerabend (1955). Moyal-Sharrock blames exactly such misinterpretations of Wittgenstein for holding back what should have been a "Galilean revolution in philosophy". [151] The distinction between stipulative and evolutionary meaning change offers extremely fruitful possibilities for understanding both Wittgenstein and Feyerabend's Wittgensteinian change of heart later down the road. In his early work, Feyerabend wanted to stip-

[147] Feyerabend, "Wittgenstein's **Philosophical Investigations...**, p. 482, which he had claimed is what Wittgenstein's does in the **Investigations**, but in a less clear fashion than Popper.

[148] See Feyerabend, ERE (1962), pp. 62–68, and Feyerabend, "Problems of Empiricism...", p. 227, and Feyerabend, **"**Linguistic Arguments...".

[149] Popper's criticism of Wittgenstein can be found in a consecutive series of short endnotes that read like an independent short essay, see Karl R. Popper, **The Open Society and Its Enemies Volumes 1 and 2**, Routledge, London 1994, pp. 653–660, endnotes 46–52.

[150] See Feyerabend, "Wittgenstein's **Philosophical Investigations...**, p. 449.

[151] Danièle Moyal-Sharrock, "The Myth of the Quietist Wittgenstein", in: Jonathan Beale and Ian Kidd (eds.), **Wittgenstein and Scientism**, Routledge, London 2017, pp. 154 [152–174].

ulate conceptual change, having not yet digested Wittgenstein's insight that conceptual change cannot simply be stipulated (although new technologies are making mass indoctrination a scarry prospect), it changes evolutionarily.

For both Feyerabend and Popper, Wittgenstein's philosophy is *reinforced dogma* because it protects itself from criticism and thereby potential for improvement. But Feyerabend takes this Popperian line of criticism a little bit further (as was so often his penchant) offering a slightly different criticism. Feyerabend criticizes Wittgenstein because Wittgenstein's goal seems to be to *reinforce* and thereby further entrench common sense, which Feyerabend thinks is in much need of improvement; and not, as Feyerabend would have it, to contribute to improving common sense by correcting it according to a realistic interpretation of our best theories. To Feyerabend, because Wittgenstein seems merely to want to leave everything as it is, he is hindering helping *improve* common sense and the status quo with science. After his reversal, Feyerabend no longer wanted to correct common sense with science, as he had recommended in ERE (1962). Instead, he wanted to protect common sense from science with respect to what we should believe to be real, exactly because common sense contains whole traditions that he wanted to protect from scientism, and this is precisely the reversal that best captures the most significant differences between Feyerabend's early writings and his post-1976 works. [152]

ERE (1962) was not just an attempt to synthesize Wittgenstein and Popper's views by combining Wittgenstein's contextual theory of meaning with Popper's maxim to try to increase testability. ERE (1962) should also be read as an attempt to mediate the infamous "Wittgenstein's Poker" dispute between Popper and Wittgenstein. [153] As we have seen, Feyerabend's criticism of Popper's methodology is based on a point attributed to Clagett that is not explicitly illustrated in ERE (1962). [154] According to Clagett, as interpreted by Feyerabend, in the long slow

[152] Feyerabend reversal on realism was not a retraction of his Kant on wheels realism, it was a result of it. It is not that Feyerabend no longer thought that scientific theories should be interpreted realistically. Rather, Feyerabend no longer gave a special status with respect to what one should believe is real (which is basically whatever makes you happy, by helping you connect with other people, making your lives meaningful).

[153] See Edmonds and Eidinow, **Wittgenstein's Poker...**

[154] It seems worth noting, potentially adding to the fog, that in the footnote attached directly to the sub-heading "5. Second Example: The Problem of Motion", Feyerabend cites Clagett on impetus theory for a more detailed account of the theories mentioned in this section. Then in the same foot-

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transition from the Aristotelian to the Newtonian worldview, at one point, rivals used the same sentences to express radically different statements as reports of the same crude measurement procedures (dropping balls into wax and measuring how deep the dent is). This is supposed to be historical evidence supporting Feyerabend's normative test-model, according to his two-pronged argumentative strategy. Feyerabend's point is that although the definition of impetus seems to be identical with Newton's first law so that the process of derivation seems to degenerate into a triviality, in fact such a derivation is impossible.

Feyerabend claims it only *seems* that one can be deduced from the other, which can be seen by the fact that some people at the time expected the two theories to make different predictions, as for example can be seen in the case of "the theory of *mail* by Abu'l-Barakat". [155] In other words, Abu'l-Barakat expected divergences between the two seemingly identical characterizations. For this reason: "The impetus of a body in empty space which is not under the influence of any outer force remains constant" *only seems* to be identical to Newton's first law.

Feyerabend is arguing that although "impetus" and "momentum" were empirically quantitively identical in some contexts, they were still parts of incommensurable (deductively disjoint) theories, even if this does not always seem to be the case, given that one and the same sentence can be interpreted into two or more different statements. Exactly this Wittgensteinian point is what Feyerabend later tries to use to undermine the universal validity of falsificationism. It is the reason why new theories cannot predict antiquated observation statements and thereby explain antiquated facts. The essential ingredient of incommensurability that Feyerabend attributes to Wittgenstein is the point that (like in a Gestalt switch) one sentence makes two incompatible statements depending on which theory is used to interpret it, that is, given a contextual theory of meaning.

At the danger of reading too much into the text, perhaps the colorful way Feyerabend chose to make this historical point (he could have taken any example) is suggestive to his purpose (to mediate Wittgenstein and Popper's dispute about

note, he cites Popper, explicitly qualifying his nod to Popper for having clarified "the *first part* of the present section" (Feyerabend, ERE (1962), p. 52, fn. 56 [emphasis added]). In this way, Feyerabend carefully distinguished his acknowledgement to Clagett on impetus theory and their relations from Popper's discussion of the Ionians, because Feyerabend takes issue with Popper's test-model given the former (Clagett on impetus) and not with Popper on the Ionians with which he agrees.

[155] Feyerabend, ERE (1962), p. 55, fn. 61 [emphasis in the original].

philosophical problems): "in these theories, the *impetus* decreased with time in the same manner in which a *hot poker that is removed from the fire* gradually loses the heat stored in it". [156] This seems to be Feyerabend having a little fun, by illustrating a key point (meaning variance) that turns out to undermine falsificationism with an example that serves as metaphor for what Feyerabend's is trying to do with his essay with respect to the Wittgenstein's poker incident. Perhaps Feyerabend thought that Popper should get over it. He should let go of his anger, like the heat leaves the poker once it is out of the fire. [157]

But there is a substantial point here that is not based on this simple word play. In ERE (1962), Feyerabend argues that there are genuinely philosophical problems in Wittgenstein's sense and tries to explain them, criticizing Wittgenstein for failing to see the positive function that the trying to resolve them can play in scientific advance, according to Feyerabend's test-model. More generally, Feyerabend is arguing that Wittgenstein fails to see how philosophy can contribute to scientific progress, by facilitating meaning change and helping to develop tests of the most general implications of our worldview in an effort to clear up Wittgensteinian fog as scientific advances (generating new possible fogs).

On the other hand, as *Popper* saw it, with the *Investigations*, Wittgenstein was denying that there are legitimate problems that are genuinely philosophical, and so to Popper, Wittgenstein seemed to be denying what Popper took to be his own important contributions to progress in philosophy of science (for example, his solutions to the problems of induction and demarcation, not to mention his **Open Society and Its Enemies**). "Sir Karl", as Feyerabend later sometimes mockingly referred to him, [158] certainly did not think that *his* philosophy had left everything as it is. Feyerabend tries to explain the source of Wittgensteinian philosophical problems at length in ERE (1962). So while Popper seemed to be rejecting Wittgenstein's genuinely philosophical problems, Feyerabend wanted to explain them as part of his synthesis of their views. In fact, ERE (1962) should be read as Feyerabend trying to make Wittgenstein's account of genuinely philosophical problems compatible with Popper's normative maxim to try to increase testabil-

[156] Feyerabend, ERE (1962), p. 55, fn. 61, citing Clagett, **The Science of Mechanics…**

[157] Feyerabend essays should be read like works of literature that contain dramatic strategies, implicit allusions, and veritable easter eggs.

[158] See Oberheim, **Feyerabend's Philosophy…**, Chapter 3.4.

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ity, by trying to *explain* Wittgenstein's conception of genuinely philosophical problems with the idea of incommensurability as part of a criticism of Popper's methodology. [159] In ERE (1962), Feyerabend forewarns:

> Now, it will turn out, in the course of this essay, that *any form of meaning invariance* is bound to lead to difficulties when the task arises either to give a proper account of the growth of knowledge, and of discoveries contributing to this growth, or to establish correlations between entities which are described with the help of what we shall later call incommensurable concepts. It will also turn out that these are exactly the difficulties we encounter in trying to solve such age-old problems as the mind-body problem, the problem of the reality of the external world, and the problem of other minds. [160]

Moreover, according to Feyerabend,

> [the] transition from a point of view which demands that certain "basic" terms retain their meaning, come what may, to a more liberal point of view which allows for changes anywhere in the system employed is bound to influence profoundly our attitude with respect to many philosophical problems and will also facilitate their solution. [161]

In ERE (1962), Feyerabend develops the idea that Wittgensteinian philosophical problems are cognitive dissonance that result from the clash between antiquated scientific principles and the ontological principles implied by the contemporary point of view. The clash occurs because the same terms and the same sentence are used to mean something radically different, something defined in a new way and which has different implications, one of which is that the older uses were incorrect because the terms used to state them having "zero extension". Wittgenstein's elusive "grammar" (the source of his special philosophical fog) is exactly

[159] Feyerabend clearly did not think *all* philosophical problems are Wittgensteinian. See for example Paul K. Feyerabend, "Physik und Ontologie", *Wissenschaft und Weltbild: Monatschrift für alle Gebiete der Forschung* 1954, Vol. 7, pp. 464–476, where Feyerabend explains his peculiar Popperian understanding of the relationship between physics and ontology (which is philosophy). Feyerabend's form of scientific philosophy (following Popper) investigates the implications our best theories have as worldviews (what kind of world should we believe is real because it is implied by our best theories, i.e. ontology) and why should we believe it is real (methodology), and even attempts to make otherwise metaphysical (untestable) predictions to test the purported ontological implications of our best theories empirical. For example, Popper infamously tried to propose an experiment to try to test determinism.

[160] Feyerabend, ERE (1962), p. 31.

[161] Feyerabend, ERE (1962), p. 90.

what Feyerabend understood as *antiquated ontological principles* that were part of realistic interpretations of earlier universal theories that supported radically different worldviews. [162] In other words, in ERE (1962), Feyerabend equates Wittgenstein's "grammar" with the incommensurable, antiquated scientific principles of former worldviews that he faults for the source of philosophical confusion and lack of progress typical of traditional philosophical problems, while dismissively criticizing linguistic philosophy as unhelpful and unnecessary. For example:

> All that is needed is the adoption of the terminology and the "*grammar*" of the most detailed and most successful theory throughout the domain of its application. This automatically takes care of whatever incommensurabilities may arise, and it does so without any linguistic detective work (which therefore turns out to be entirely unnecessary for the progress of knowledge). [163]

Feyerabend makes a similar point, while noting:

> One hears frequently that a complete replacement of the *grammar* and the terminology of the "old language" is impossible because this old language will be needed for introducing the new language and will, therefore, infect at least part of the new language. This is curious reasoning indeed if we consider that children learn languages without the help of a previously known idiom. Is it really asserted that what is possible for a small child will be impossible for a philosopher, a linguistic philosopher at that? [164]

In this way, Feyerabend claims that Wittgenstein's philosophical fog is caused by antiquated ontological principles that continue to haunt everyday language. From Feyerabend's perspective, age-old philosophical problems arise historically as science advances through revolutions that develop deductively disjoint universal theories. Older theories, which were once successfully used to experience reality, are incommensurable (deductively disjoint) with later replacements, while contemporary universal theories inform many of the same sentences in radically new ways, so that many of the same sentences, used in much the same ways, refer to radically different kinds of all the same stuff. This is how antiquated universal theories continue to haunt everyday language, periodically causing confusion. In-

[162] That Feyerabend was following Wittgenstein's suggestions about "grammar" can clearly be seen in Feyerabend, ERE (1962), p. 91, fn. 11.

[163] Feyerabend, ERE (1962), pp. 75–76 scare quotes in original [emphasis added].

[164] Feyerabend, ERE (1962), p. 76, fn. 92 [emphasis added].

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compatible ontological interpretations give radically different meanings to the same sentences while also making former impossibilities possible: "space" now compresses. "Philosophy" could be scientific, and not just armchair or analytic. [165] "Analytic" is not a property of a proposition independently of its use. It is just one way a statement can be used, given that exactly the same statement can also be tested in another context. [166] Scientific knowledge includes many such testable definitions that have been replaced in the course of scientific advance. That is why Feyerabend chastises Bohr, Heisenberg von Weizsäcker and Wittgenstein. They appear to have overlooked that "man is capable not only of applying, but also of inventing, [whole new] languages". [167]

For Feyerabend, everyday language is philosophically foggy because we use the same sentences to have radically different meanings in the same contexts, depending on which theories are used to interpret what is being said. Sentences are polymorphic and have incompatible meanings that are part of deductively disjoint conceptual schemes that result from progress in the sciences and differences in worldviews. Feyerabend was trying to explain why age-old philosophical problems do not disappear unless meanings are allowed to change, and to explain Wittgenstein's conception of genuinely philosophical problems, by treating antiquated principles implied by earlier worldviews as Wittgenstein's elusive "grammar", that is as the source of the conceptual tension resulting in philosophical fog, while still maintaining with Popper (and Bohm) that there are *also* legitimate

[165] This is as true for terms like "philosophy" and "science" and "meaning" as it is for "up", "hot" or "fell". "Positivism", "metaphysics", "philosophy", "science" — all have historically taken on numerous meanings that bear mere family resemblances to each other as they continue to be developed (which also accounts for some of the philosophical fog surrounding these issues).

[166] For example, each of Newton's laws can be test empirically in turn, by treating others as analytic for the sake of testing it. On Feyerabend's rejection of the analytic/synthetic distinction, see Paul K. Feyerabend, "A Note on the Paradox of Analysis", *Philosophical Studies* 1956, Vol. 7, No. 6, pp. 92–96, and compare Thomas S. Kuhn, **The Structure of Scientific Revolutions. Second Edition, Enlarged**, *International Encyclopedia of Unified Science* Vol. 2, No. 2, Chicago University Press, Chicago 1970.

[167] Feyerabend, ERE (1962), p. 49. For Feyerabend's views on Bohr in this period, see Paul K. Feyerabend, "Complementarity", *Proceedings of the Aristotelian Society. Supplementary Volume* 1958, Vol. 32, No. 1, pp. 75–104 and Paul K. Feyerabend, "Niels Bohr's Interpretation of the Quantum Theory", in: Herbert Feigl and Grover Maxwell (eds.), **Current Issues in the Philosophy of Science. Symposia of Scientists and Philosophers**, **Proceedings of Section L of the American Association for the Advancement of Science, 1959,** Holt, Rinehart and Winston, New York 1961, pp. 371–390.

philosophical problems such as ontological problems that may involve genuinely philosophical confusions in Wittgenstein's sense, but which may also be clarified and eventually even lead to empirical tests. This is an essential ingredient of revolutionary scientific progress as according to and illustrated by Feyerabend's test-model (see the Postscript to part 1).

Feyerabend's synthesis accepts Wittgenstein's point that there are genuinely philosophical (pseudo-) problems caused by tension that arises due to the deductive disjointedness of the ontological principles used to explain everything according to different worldviews, while also maintaining with Popper that there are *genuine* philosophical problems (in contrast to *genuinely* philosophical problems), which are perpetually raised anew after every revolution, as for example the principle that everything is ultimately energy in the transition from Energeticism to Atomism. As we shall see, when Brownian motion was used to settle that dispute, it raised new fascinating ontological issues in the foundations of quantum theory, that Popper and Feyerabend (and Bohm and many others) were trying to understand and devise ways of testing. Feyerabend identifies Wittgenstein's "grammar" as antiquated ontological principles that were implied by some superseded universal theory that are incompatible with the ontological principles implied by the current theory. If you are conceptually confused in a Wittgensteinian fog about which way "up" is because of the tension between antiquated principles and contemporary theories, then just look "up" (letting the fly out of the bottle), and the tension will dissipate like fog at sunrise, allowing for a clear view, as it were, of which way is "up". That is how genuinely philosophical pseudo-problems can be dissolved without anything unknown discovered so that only how you already understand is made clear. On this reading, the infamous "Poker dispute" turns into a pseudo-disagreement due to conflating "genuinely philosophical" and "genuine philosophical" problems, the resolution of both of which have roles to play in science (from Feyerabend's perspective).

Feyerabend maintains that there will always be new ontological problems for philosophers to work on after each revolution that results in a new worldview — unless it seems like everything has been explained and nobody has any better proposals, which is exactly the danger of conformism that Feyerabend so often says he is trying to combat throughout his early philosophy of science as "a plea for tolerance in matters epistemological" which is part of the title of a paper that

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is explicitly a *simplified summary* of his views in ERE (1962). [168] Ontological problems differ from other empirical questions because they do not concern what the theory implies about particulars, or less general statements, that can be deduced from the theory. Ontological principles are universal, and they are used to realistically interpret theories to give them meaning to support our worldviews. But in contrast to empirical statements about particulars that can be used to test theories and lower-level statements empirically, ontological principles always imply something about everything. They are as general as the universal theories they give meaning to, and they are not easy to test. Yet for Feyerabend, following Popper, finding ingenious ways to test ontological principles is exactly how to revolutionize our worldviews and the world along with it. Popper spent much of his life trying to devise and design experiments to test ontological principles like determinism and indeterminism. Einstein identified a flaw in Popper's experimental set-up, which he avoided in the design of the EPR thought experiment. Einstein even sent a letter of encouragement to Popper for motivating him (Einstein) to publish the EPR paper, and Popper had the letter that Einstein sent to him published as an addendum to his **The Logik**. [169] According to Popper and Feyerabend's scientific conception of philosophy, ontology is where metaphysical speculation can become empirical.

As we shall see, according to Feyerabend, universal theories based on incompatible frameworks are much better tools for testing established theories than testing them against commensurable theories by falsification within a shared framework, and the tests used to test them test much more than what is tested in a falsification. Because rival universal ontological principles give different meanings to the terms used to state deductively disjoint universal theories that are used to explain everything, presumably, these principles cannot formally contradict either, because they must mean something different by "everything". Feyerabend suggests that the theories are based on incompatible rules of usage for the same terms so that their predictions cannot enter formally relations. Otherwise, contemporary theories would be able to explain antiquated facts, which according to Feyerabend is not the case.

[168] Feyerabend, "How to Be a Good Empiricist...".

[169] See Karl R. Popper, **The Logic of Scientific Discovery**, Basic Books, New York 2002; "New Appendices, xii, »The Experiment of Einstein, Podolsky and Rosen. A Letter from Albert Einstein, 1935«", pp. 481f.

There is one last point worth mentioning about Wittgenstein and ERE (1962), which leads nicely into the topic of the next section. As indicated earlier, Wittgenstein turns out to be the first reveal in ERE (1962), as about ten pages in, we finally learn who those "some philosophers" (see above) he is criticizing are:

> [Wittgenstein] dwells upon the difficulties one will encounter when trying to change a language in a very fundamental way and he thereby insinuates that it may be altogether impossible to carry out decisive changes: The reason for this pessimism seems to be identical with the one I briefly discussed in the introductory part of this paper: *It is assumed that the all-pervasive character of a language makes it impossible to specify grounds for abandoning it*. [170]

Here, Feyerabend decisively reveals exactly who he was criticizing and why. As we shall see, several other questions raised in the introduction still remain open at this point in the drama.

In sum, Feyerabend tries to synthesize Wittgenstein's views on meaning with Popper's critical rationalist maxim to increase testability. According to Feyerabend, on the one hand, Wittgenstein is right that there are genuinely philosophical (pseudo-) problems, but Wittgenstein is wrong that resolving them must leave everything as it is. For Feyerabend, (as we shall see) resolving genuinely philosophical pseudo problems by making them empirical can play a very important role in scientific advance. On the other hand, according to Feyerabend, while Popper is right that we can specify grounds for abandoning an all-pervasive language, Popper's falsificationism cannot take how this can be done into account. In ERE (1962), Feyerabend was trying to develop his ideas, following Einstein's use of the term "incommensurable", to try to criticize both Popper and Wittgenstein, while developing his own methodological argument for pluralism in support of his own brand of theoretical pluralism in contrast to Popper's methodology and as part of a criticism of it.

[170] Feyerabend, ERE (1962), p. 39, fn. 27.

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1.5 The Performance: A Big Reveal and Ethics as the Basis of Methodology

In the early 1980s, Feyerabend wrote a manuscript that revises and considerably extends his inaugural lecture at the ETH Zurich in 1981 with the title "Science as an Art". [171] This section could likewise have been called "Scientific philosophy as an art". By then, Feyerabend who had initially wanted to be a philosopher-scientist, had become a scientific philosopher (following Popper), and then rebelled, as he did not want to be seen as a philosopher of science, and he did not want to advance his own "position", because philosophy of science as general methodology is disconnected from its subject and so broken by design. It can only offer adages that are empty because there is no way to know which one applies in any specific case. He considered some of his published works to be art. For example, he called **Against Method** (1975) a collage.

Feyerabend was a performer by nature. His performances were his most consequential activities (especially for him). One of them had gotten him shot, [172] another led to Popperstein's baby (see part 2). Feyerabend lectured Nazi ideology, acted in theaters, and studied opera, before becoming a renowned public speaker. Performances all. Some of his published papers are also clearly performative. Feyerabend considered itsome of his published papers to be works of art, in some ways comparable to the products of painters and novelists. For example, having been repeatedly accused of plagiarism by Popper, in 1968 Feyerabend asked Agassi:

> Do you have any ideas on the history of attributing ideas to the originator? A painter does not say in his paintings, nor does a novelist say in his novel (he may, but he is not obliged to by a tradition) that he got the style, the manner of presentation etc. from so and so. Why should this be the case in philosophy? [173]

[171] Compare Paul K. Feyerabend, **Wissenschaft als Kunst**, Suhrkamp, Frankfurt am Main 1984.

[172] "Playing the operatic hero once again," Feyerabend recounts how he ran out into an intersection and started directing traffic during a battle with shooting from all sides (Feyerabend, **Killing Time...**, p. 52). The events he claims to have witnessed while retreating from the Easter front [including rounding up an executing civilians) seem to have haunted his dreams for the rest of his life: "I often dream of having committed treason or murder" (Feyerabend, **Killing Time...**, p. 52).

[173] Feyerabend to Agassi, 11 June 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

ERE (1962) is performative. To understand it, it certainly helps to read it as such. For to appreciate it, one needs to recognize its clear dramatology: How it builds suspense in the first two acts, with attacks on Hempel and Nagel's accounts, for what develops into a seemingly *inevitable* patricide of Popper's methodology to come. Read in context of Feyerabend's formative development, there are clearly indirect allusions (for example, to Kuhn, Bohm and Ehrenhaft), easter eggs (if you really want to go there), and other dramatic rhetorical tools that are easy to identify. ERE (1962) is philosophy of science as an art in the sense that the form of the presentation expresses the intent of the author. He is trying to break with Popper on methodology in a big reveal by challenging its misleading implication that there can be formal relations between statements deduced from universal theories separated by scientific revolutions. ERE (1962) contains a series of sophisticated and interconnected arguments, based on a two-pronged strategy, explicitly grounded primarily in ethical considerations presented in an essay with a clear dramatology that illuminates its purpose. [174] The main point of this section is to outline Feyerabend's argumentative strategy, exposing the performative aspects of ERE (1962) that have contributed to ERE (1962) being read as an extension of Popper's methodology instead of as a criticism of it based on a model that was developed explicitly in contrast to it.

The Two-Prongs of Feyerabend's Argumentative Strategy

In ERE (1962), Feyerabend deploys a two-pronged argumentative strategy to criticize all three of his main contemporary empiricist targets both on descriptive and normative grounds that are ultimately based on ethical justifications. According to Feyerabend, science is what we make it and what we make of it. With respect to Feyerabend's formative development, this remains constant from start to finish. Nonetheless, the first prong of Feyerabend's argumentative strategy is descriptive. Simply put, Feyerabend criticizes all three of his main targets by pointing out that their accounts do not match how progress through revolution has been made. He presents the views he criticizes and argues that history tells a different story. More specifically, with respect to Popper's methodology, as we shall see, Feyerabend argues that Popper's description of the transition from classical mechanics to relativity theory is incorrect, because in transitions between univer-

[174] See Oberheim, **Feyerabend's Philosophy…**, Chapter 7.

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sal theories, there can be no falsifications due to a lack of formal relations that results from meaning variance in the terms used to state the rival theories. (The lack of formal relations between the corresponding predictions is not supposed to be a problem for scientists, but for philosophies of science such as those developed by Hempel and Popper). Furthermore, Feyerabend argues that as descriptions of successful science, the accounts he targets are all inaccurate for the same reason: Because they posit formal logical relations that do not exist given meaning variance that results from scientific revolutions. Put directly, Feyerabend is claiming that revolutionary theories cannot explain the facts they antiquate. It just looks that way because the same terms are used to mean different things. Feyerabend argues this by trying to tie all his targets to some version of the principle of meaning invariance, according to which meanings remain invariant with respect to explanation, reduction, and empiricism.

One reason that Feyerabend has been misunderstood as supporting Popper's methodology, when in fact he was trying to criticize it, is that Popper has no quarrel with meaning variance or the "theory-ladenness" of observation statements, so Feyerabend's argument based on meaning variance does not seem to apply to Popper's methodology. Feyerabend knew that Popper increasingly emphasized these things. Popper's account of the role of basic statements is specifically designed to avoid difficulties that could arise due to meaning variance and the theory-ladenness of observation statements by grounding tests on mutually agreed upon "basic statements" (that's why the statements are supposed to be "basic"). According to Popper's account, meanings do vary, but this does not affect methodology, because rival theories are tested according to accepted basic statements regardless of what those statements mean, as long as they are accepted by both parties, they can play their roles in crucial experiments by falsification. For Popper, if accepted basic statements are inconsistent with predictions deduced from a theory, perhaps the theory is correct, and the basic statements need to be revised, as Popper takes basic statements to be theoretical and testable.

Feyerabend is implying that for Popper, the onus is always squarely on the theory, because (general) theories are less well-corroborated than the basic statements (about particulars) used to test them. Moreover, as we shall see, Feyerabend's point is that meaning variance, which Popper tries to take into account (with his "relative autonomy of basic statements") is incompatible with falsificationism, given Popper's own assumptions about meaning. In ERE (1962), Feyer-

abend is trying to disagree with Popper on both of these points: On the relevance of meaning variance to testing rival universal theories empirically *and* that the onus should be on the theory as a general statement, because basic statements about particulars are better tested than the general theories explained by them. In ERE (1962), Feyerabend rejects with Popper's account of basic statements. He concludes that meaning variance undermines falsificationism because the new theory cannot formally explain the existing theories successful predictions, which (as Feigl pointed out to him) must also then apply to crucial experiments; [175] that is, if meaning variance renders rival theories deductively disjoint so that one cannot explain predictions deduced from the other, than their predictions cannot formally contradict each other either. [176]

As we shall see, the source of the problem of explanation in Popper's methodology is initially what Feyerabend uses to argue against Hempel, before it is turned against Popper, whose model has the same erroneous implications given that Hempel's model is subsumed by Popper's account. (According to Feyerabend, Hempel's criteria for what should count as a more comprehensive explanation are the same as in Popper's model, but in a more definite form. Popper adds additional criteria for the justification of replacing an established explanation to Hempel's account). Feyerabend argues that for this reason, falsificationism puts a dogmatic, overly restrictive condition on the admissibility of replacing established theories with new proposals. Universal theories should not have to be able to explain the successful predictions of their rivals in order to justify replacing the established theory on empirical grounds. In stark contrast to Popper, Feyerabend rejects the idea that basic statements and background knowledge are easier to test than theories. For Feyerabend, basic statements and background knowledge are harder to test, and so more in need of testing, than theories, just because they are so well-entrenched. This can make it hard even to imagine how they could be tested. This is part of the second normative prong of Feyerabend's argumentative strategy.

[175] Feyerabend explicitly credits Feigl with making between this point in Feyerabend, "An Attempt at a Realistic Interpretation…" and in Feyerabend, ERE (1962): "As Professor Feigl has pointed out to me, this difficulty also arises in the case or crucial experiments", p. 94 fn. 115).

[176] That the predictions deduced from deductively disjoint theories cannot logically contradict each other in a crucial experiment is not the point that Feyerabend used to criticize Popper's methodology. He does not think scientists are bothered by semantic considerations.

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Feyerabend is not going to target Popper's account simply by arguing that it is historically incorrect. That is not the point of the Brownian motion example (which Feyerabend uses to illustrate the test-model he proposes in contrast to Popper's methodology). This is not the episode that Feyerabend explicitly claims Popper's account misdescribes in ERE (1962), which is the transition from classical to mechanics to relativity theory. Popper says that was a falsification. Feyerabend says it was not. But even that Popper's methodology is descriptively incorrect in both cases according to Feyerabend's view, this is not the main point of the argument. Feyerabend is criticizing falsificationism as a *normative methodology*, not simply as a description of science that can be criticized by comparing it to actual scientific practice. In ERE (1962), Feyerabend explicitly takes sides with Popper against Kuhn in this respect (that is as to whether questions about empiricism should be treated as normative, and not descriptive), and Feyerabend criticizes Kuhn accordingly:

> For them [Kuhn and the historical school] actual scientific practice is the material from which they start, and a methodology is considered reasonable only to the extent to which it mirrors such practice. [177]

Feyerabend explicitly criticizes this view. According to Feyerabend:

> Expressed in more pedestrian terms, this maneuver propagates the acceptance of unsatisfactory hypotheses on the grounds that this is what everybody is doing. [178]

Instead, Feyerabend makes some fairly obvious allusions, emphasizing that while,

> increased attention paid to the historical aspects of a subject, and the attempts to break down the distinction between the synthetic and the analytic must be welcomed as steps in the right direction. However, even here there are drawbacks. [However] Only very few of the enthusiastic proponents of an increased study of the history of a subject realize the methodological importance of their investigations [alluding to Kuhn]. The justification they give for their interest is either sentimental or psychological ("it gives me ideas" [Kuhn]), or based upon some very implausible (because Hegelian) notions concerning the "growth" of knowledge [Bohm and perhaps Kuhn too]. What these thinkers [Kuhn and Bohm] need in order not to fall victims to all sorts of quasi-philosophies is a methodological backbone, and I hope that the theory

[177] Feyerabend, ERE (1962), p. 60 and fn. 68.

[178] Feyerabend, ERE (1962), p. 61.

> of test which has been sketched above in its merest outlines will provide such a backbone. [179]

By developing his normative test-model, Feyerabend is trying to help Kuhn and Bohm free themselves of their "quasi-philosophies". In contrast to Kuhn's historical approach, as we shall see, Feyerabend uses the Brownian motion example to illustrate why Popper's account *cannot* work when testing rival incommensurable theories empirically as part of a normative methodological argument that it stifles progress by making the justification for replacing an established theory with an incommensurable proposal impossible, overlooking the possibility of testing theories empirically, not just by establishing formal relations between their corresponding predictions, but by testing their rivals' ontologies — and not just that Popper's methodology does not work in this one case.

As we shall see, the point of the Brownian motion example is that it was discovered to be caused by atoms, and so classical phenomenological thermodynamics was shown to have the wrong ontology, not that it makes the wrong predictions. This is what shows that it could never have discovered that Brownian motion refutes it without the development of an incompatible rival explanation of it. The new theory is needed to establish the fact that Brownian motion can be used to test the established theories ontology (not predictions deduced from it). In this way, ontology becomes part of a theory's empirical content, because universal theories are tested by conjecture and *novel* corroboration. The "novel" is important. The corroborations should be novel in the sense they are theory-laden by the new proposal rendering the rivals deductively disjoint due to meaning variance in the terms used to state them, but still indirectly refute the theory by undermining its ontology by changing the rules of usage that govern the correct use of the terms used to state the rival theories. As we shall see, this is why Feyerabend suggests that there is more to a theory's empirical content than the testable predictions that can be deduced from it.

According to Feyerabend, Popper's account is normatively unhelpful and descriptively incorrect. Popper's account misdescribes the relation between classical mechanics and relativity theory as a falsification, when there can be no falsification (according to Feyerabend) due to meaning variance in the terms used to state them that renders their corresponding predictions deductively disjoint so

[179] Feyerabend, ERE (1962), p. 68, fn. 82.

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that they are unable to enter formal relations (they have no formally comparable consequences) so that the new theory cannot explain the successful predictions deduced from the established theory. The reason that Popper's methodology is detrimental, normatively speaking, is that it is purportedly *anti-progressive*, by putting undue constraints on the justification of new universal theories that impede progress. Feyerabend is very explicit that, "it is of paramount importance to insist upon the normative character of scientific method", [180] while transitioning from the descriptive prong to the normative methodological prong of his two-prong argumentative strategy, while repeatedly praising Popper for making *this* point clear, that is, for making it very clear that that contemporary empiricists *should be* normative like Popper's methodology. Feyerabend's proposed test-model for incommensurable theories is also normative. It tries to model how universal theories should be compared empirically. Many passages praising and explicitly supporting the normative approach were removed from the republication in his 1981 volume of collected papers, erasing the original emphasis on the normative character of Feyerabend's approach to methodology at the time. Again, this makes it very clear that at the time for Feyerabend contemporary empiricism should be, and is in Popper's case, *normative*, as is Feyerabend's test-model and the justification for it (it should promote progress in contrast to stifling it). Feyerabend emphasizes that:

> scientific method, as well as the rules for reduction and explanation connected with it, is not supposed to describe what scientists actually do. Rather, it is supposed to provide us with normative rules which should be followed, and to which actual scientific practice will correspond *only more or less closely*. [181]

Clearly, Feyerabend is signaling that he is not trying to target Popper's account merely by showing that scientists did something different in the Brownian motion case, or by arguing that his model does not apply to a particular case. That is clearly not the point of his criticism of Popper's methodology. Popper's account was never supposed to correctly describe every episode of scientific progress. It is just supposed to be a rule of thumb that recommends the most rational course of action for when to replace an established theory by another that is better justified given the empirical evidence. According to Popper (and Feyerabend), given any

[180] Feyerabend, ERE (1962), p. 62.

[181] Feyerabend, ERE (1962), p. 60.

number of historical contingencies, there is no guarantee that being rational will lead to the most fruitful results in any particular case. For Popper (and Feyerabend), making the most rational decision about which theory is better empirically given the evidence at some time was never supposed to guarantee that this decision will always automatically lead to progress.

The descriptive prong of Feyerabend's argument can make it look very much like Feyerabend is simply misrepresenting Popper's methodology as purportedly universally valid, when in fact falsificationism was only ever just supposed to be a rule of thumb. That is one of the reasons why Watkins and Popper thought Feyerabend's criticism fails to hit its intended target. However, as we shall see, Feyerabend's criticism is not that falsificationism does not work. It is that it cannot work as a model for comparing rival universal theories empirically. This is what makes the rule of thumb defense of Popper's methodology irrelevant. It is beside the point.

Normatively, all three of the contemporary empiricist accounts that Feyerabend criticizes are detrimental, for the same reason, because they put undue restrictions on the admissibility of replacing established universal theories with new proposals and thereby stultify progress. This contributes to the spread of dangerous ideologies (belief in some absolute truth). In this way, in Popper's case, Feyerabend was challenging Popper's normative contention that the predictions deduced from new theories *should* have to logically be formally consistent with the successful predictions of the established theory (while also contradicting "other predictions deduced from it to justify replacing it), as just such an overly restrictive condition in the same way that Hempel and Nagel's accounts are overly restrictive when interpreted as normative methodologies. Feyerabend concludes that Popper's test-model (falsificationism) *cannot* take testing incommensurable theories into account. [182] Falsificationism (and any normative account that posits formal relations between incommensurable theories or statements deduced from them, such as Nagel's deductive account of "reduction" and Hempel's deductive account of a better "explanation") is both *descriptively* inaccurate "with respect to transitions between universal theories separated by scientific revolutions that change the rules of usage of the terms used to state the theories, and normatively undesirable because it impedes progress, which is dangerous. These are the two

[182] Feyerabend, ERE (1962), pp. 92–93.

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strands of Feyerabend's argument. The normative argument criticizes contemporary empiricism because it puts overly restrictive conditions on the justification of replacing established universal theories with new proposals. Abiding by the recommendations implied by contemporary empiricism would inhibit the very progress it attempts to promote by making revolutionary advance impossible to justify. Feyerabend seems to be suggesting that contemporary empiricism (Popper's included) inadvertently contributes to the entrenchment of dangerous dogmatic ideologies that become increasingly immune from revision in a downward spiral that makes it harder and harder for revolutionary progress to correct everyday beliefs in the course of progress.

Ethics as The Basis of Methodological Decisions

In his early normative philosophy of science such as ERE (1962), methodological issues are not ultimately decided by matters of fact. [183] Following Dingler, science is both what we make it, and what we make of it. In ERE (1962), one of Feyerabend's goals was to contribute to our understanding of scientific justification. That is, while his goals were *primarily* normative, they were clearly also to some extent intellectual. He was trying to understand how revolutionary theories are justified empirically, but he does not want to commit to another one-size fits all model. It is only one size fits all with respect to revolutions. His model is not supposed to be applicable to comparing commensurable theories empirically. His explicit goal is primarily to help to promote *perpetual* revolutionary progress in science, as an ever-expanding ocean of alternatives — not primarily to get to some truth (about science or about methodology), or even somehow get "closer" to some such truths. The main goal is to try to prevent dogmatic exclusion of alternative views, because that denigrates our best theories into dogmatic ideologies. The goal is to keep science moving, because stopping science and its perpetual relativization of ideas to frameworks is precisely what leads to disaster. As the German saying goes: "*Der Weg ist das Ziel*". In German, this appears to imply, paradoxically, that *the journey itself is the destination*. In other words, the goal is perpetual improvement (perpetual progress), not primarily for the sake of the improvement itself, but primarily because of the danger of deteriorating into dogmatic stagnation without it.

[183] See especially Feyerabend, "Realism and Instrumentalism...", pp. 280–308.

Feyerabend indicates that he realizes that his readers will think that he is hyperbolizing the danger:

> It may be that the word "explanation" sounds beautiful to some ears — but who would think it reasonable to start a war for, or against, its elimination?. [184]

His readers may think he is overstating the danger, as nobody cares enough about "explanation" to fight for it or about it, but Feyerabend is quite serious. He says that dogmatism, especially in the form of unwavering belief in some "absolute truth" is what "reinforces all the maneuvers which may be used for the preservation of the theory [or ideology] (elimination of opponents included)". [185] Feyerabend notes that:

> Today, of course, the "elimination" takes the more refined form of a refusal to publish (or to read) what is not in agreement with the accepted doctrine. However, this "liberalism" applies to physical theories only. It does not seem to apply to political theories. [186]

By "today", Feyerabend could be read as indirectly alluding to his participation in the Second World War. He had directly promoted and participated in war and the propagation of a dangerous dogmatic ideology, having lectured candidates at officers' school on the justification of Nazi ideology, [187] before himself eventually being sent to the front shortly before the war ended (they seem to have run out of candidates for him to teach), as the German army retreated from Russian advances on the Eastern Front, at which point he was wounded and paralyzed from the waist down for the rest of his life. As we shall see in part 3, with his allusion tosuppression of publication in physics and an intolerant attitude taken toward alternative "political theories", he may be alluding to Bohm, who had fled the USA because of his alleged political views given that Rosenfeld had been pre-

[184] Feyerabend, ERE (1962), p. 61.

[185] Feyerabend, ERE (1962), p. 69.

[186] Feyerabend, ERE (1962), p.69, fn. 85.

[187] Feyerabend was very familiar with Alfred Baeumler's book **Männerbund und Wissenschaft**, Junter und Dünnhaupt Verlag, Berlin 1934, which he appears to have closely studied in preparation for his officer's school lectures on the justification for the war. Baumler was a leading academic proponent of National Socialism in Nazi Germany. He was appointed Professor of Political Education at Berlin University in 1933 and emerged as the chief liaison between the universities and Rosenberg's office of ideological education. Baumler infamously tried to use Nietzsche's ideas to promote Nazi ideology.

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venting the dissemination and publication of Bohm's work. Feyerabend is claiming that when people start believing in *the* truth, and not the truth according to this theory, as opposed to the truth according to that theory, then *the* truth can easily become something worth fighting, and even dying, for. That is how dogmatism becomes dangerous — by insulating itself from criticism. To exemplify Feyerabend's point, just take the recent, dangerous rise in authoritarianism in conjunction with Donald Trump's relationship to criticism as a topical example. Trump can't cope with criticism, and for Trump (and some of his armed supporters), it seems there is only one truth: Trump's truth. Nothing else seems to matter to them.

Immanent Criticism

Another reason that Feyerabend's views can be difficult to disentangle from those he criticizes it that Feyerabend tried to develop immanent criticisms of his targets, in the sense that Feyerabend tries to criticize his targets by developing implications of their own accounts. For example, Feyerabend emphasizes that Popper and Wittgenstein shared the contextual theory of meaning, and he uses it to criticize suggestions made by each of them (falsificationism and quietism, respectively). Whenever Feyerabend identifies claims that his targets have not explicitly agreed with that function as premises of his arguments, he tries to offer independent arguments for those claims. This can make it quite challenging at times to differentiate Feyerabend's views from those he is criticizing in the essay.

Feyerabend's criticism of Popper is an immanent criticism because Feyerabend pits Popper's views on meaning against Popper's claim that to falsify a theory, successful predictions should be deduced from it that logically contradict some of the predictions made by the existing theory, while being logically consistent with the successful predictions of the existing theory, potentially explaining them. In this way Feyerabend pits Popper against himself, just as he tries to pit Nagel, Carnap and Hempel against themselves, to expose,

> the unwitting and partial return to the ideology of sense data, that is responsible for many of the "inner contradictions" which are so characteristic of contemporary empiricism as well as for the pronounced similarity of this philosophy [contemporary empiricism] to the "school philosophies" it has attacked. [188]

[188] Feyerabend, ERE (1962), p. 43.

As we shall see, after Feyerabend's big reveal, it turns out that by "contemporary empiricism", Feyerabend specifically means to include Popper's methodology, as sharing the same dogmatic element with school philosophies, in this category.

With respect to Feyerabend's assessment of Popper's methodology, it can be especially difficult to differentiate Feyerabend's views from those of Popper as Feyerabend's critical target, because he assumes a critical rationalist framework as part of an imminent criticism of Popper's methodology, and in his criticisms of Hempel and Nagel, Feyerabend praises Popper's account as better than Hempel or Nagel's accounts interpreted normatively as applied to (the limited range of) commensurable theories. In other words, Feyerabend does not criticize Popper for being *completely* inaccurate as a methodology. He implies that Popper's methodology works to a limited (and to him relatively uninteresting extent), [189] because it is limited to testing lower-level laws and generalization within frameworks and is not applicable to testing universal framework theories that determine entire worldviews by implying ontological principles. To put it crudely, Feyerabend's attitude is that revolutions are where the real action is at because science should be in permanent revolution. From this perspective, Popper's methodology only applies to what Kuhn calls "normal science", which at the time Feyerabend considered to be a dogmatic abomination, because everything should be open to test all the time (by the 1980s, Feyerabend seems to have come to accept Kuhn's point).

For Feyerabend, methodological issues should be decided based on their ethical consequences presupposing promoting progress in science is something good in contrast to ideology and its dangers. In other words, Feyerabend thought by showing that falsificationism cannot take universal theories into account because they are deductively disjoint, he had demonstrated that falsificationism is impossible with respect to universal theories, and so it is anti-progressive. To Feyerabend, testing commensurable theories is also relatively uninteresting, because falsificationism only applies to testing commensurable theories when there is no meaning variance, for example, when Kepler's laws and Galileo's law of free fall were falsified by Newton's theory. Feyerabend argues that Popper has shown that in a falsification the new theory could formally explain the successful predictions

[189] Kuhn and Bohm put much more emphasis on the productivity and necessity of commensurable testing within frameworks as prerequisite to testing frameworks against each other than Feyerabend, who thought that science should be in a state of permanent revolution. See part 3.

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of the established theory. For example, according to Feyerabend, Popper has shown that Newton's theory is supported by confirmed predictions deduced from it that can be deduced from Galileo's law of free fall and Kepler's laws. According to Feyerabend, Popper has *correctly* described *these* episodes with falsificationism, because in these episodes the theories and laws involved are all commensurable with each other. [190] This is not a case involving rival universal theories, nor meaning variance, which is why Feyerabend's criticism of Popper's methodology only limits the validity of falsificationism (see section 1.9).

In any case, Feyerabend's agreement with Popper with respect to the superiority of Popper's methodology to Hempel and Nagel's accounts (normatively interpreted) for commensurable theories can make it seem like Feyerabend is simply siding with Popper against logical empiricism as represented by Hempel, Nagel *and* Carnap, whose "double-language model" is also explicitly a target of the same criticism in ERE (1962) that Feyerabend launches against Hempel and Nagel, before reaching Popper's empiricism in the conclusion of the essay. Feyerabend's qualms with Carnap's double language is what kept the ball rolling with respect to his idea of incommensurability as he developed it in the 1950s. [191] Feyerabend was proposing a top-down contextual theory of meaning to replace the idea of independently meaningful observation sentences According to Feyerabend, Carnap's "procedure quite obviously presupposes that the meaning of the observational terms is fixed independently of their connection with theoretical systems",[192] so that Carnap's account,

> seem[s] to imply that the meaning of an observational statement is already fixed by the way in which the sentence expressing it is handled in the immediate observational situation (note the emphasis upon absolute confirmability for observational sentences!), i.e., they seem to imply the rejection of the pragmatic theory. [193]

This turns out to be part of Feyerabend's criticism of both Carnap and Popper's views in ERE (1962), [194] but only retrospectively, as it were, and without the note on the emphasis of absolute confirmability in Popper's case. In other words,

[190] Feyerabend, ERE (1962), pp. 93–94.

[191] See Collodel and Oberheim "Introduction" in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**

[192] Feyerabend, ERE (1962), p. 41.

[193] Feyerabend, ERE (1962), p. 42.

at this point in the essay, Feyerabend has not yet made clear that his criticism of contemporary empiricism also applies to Popper's methodology (in the same way for the same reason), which he leaves until his big reveal in the conclusions. Carnap's double language model is a target *along with* Hempel's account used to illustrate the problem with Hempel's account, before turning to Nagel account and many others *along the way*.

According to Feyerabend in ERE (1962), Popper's falsificationism is preferable to Hempel and Nagel's accounts, interpreted normatively. But this does not imply that Popper's falsificationism does not also put overly restrictive conditions on theory-testing and the admissibility of new theoretical proposals. Feyerabend distinguished the case where Popper's falsificationism works, from empirically testing rival universal theories, where it does not. For Popper, Newton's theory was falsified by predictions made by Einstein's theory in the same way as Kepler's laws and Galileo's theory of freefall. For Feyerabend in ERE (1962), these two cases are explicitly used to represent the distinction between testing lower-level laws and theories that are commensurable by falsification, *in contrast* to testing universal theories that are incommensurable (or "deductively disjoint"). In other words, Feyerabend's test-model is designed to take universal incommensurable theories into account, picking up exactly where falsificationism breaks down, that is, whenever there is meaning variance that renders rival theories deductively disjoint. For this reason, because it complements Popper's falsificationism in this way, Feyerabend's test-model can seem like an extension of Popper's views, when in fact it is supposed to bepart of a criticism of Popper's views, that was developed in contrast to them.

In sum, by structuring the essay as a performance, with a big reveal, Feyerabend leaves it to the reader to work backwards, as it were, to process the implications of his two-pronged immanent criticism after they find out that Popper's methodology fairs no better with respect to universal theories. We will see how Feyerabend builds up his big reveal shortly. For now, it is enough to emphasize that when Feyerabend concludes that Popper's methodology cannot take testing rival incommensurable theories into account, he does not directly call Popper's

[194] Compare Feyerabend, ERE (1962), p. 31, p. 43 and p. 64 to: "This is not the only instance where, on closer scrutiny, a rather close relation emerges between some versions of modern empiricism and the »school philosophies« it attacks", p. 70, fn. 86, then see Feyerabend's criticism of Popper's methodology, pp. 93–95.

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methodology a dogmatic school philosophy because it implies meaning invariance. Rather, he had claimed that methodologies that imply meaning invariance share a dogmatic element with school philosophies that make them anti-progressive. Then he explains how and why Hempel and Nagel's contemporary empiricist accounts imply meaning invariance, before concluding that his argument also applies to Popper's account. All of this helps to explain why when Feyerabend tried to criticize Popper's methodology for having become a dogmatic school philosophy in ERE (1962), he was misunderstood by Popper. It also helps explain why there continues to be a well-entrenched myth about Feyerabend: That he was an ardent supporter of Popper's falsificationism until the late 1960s (see part 2 of this essay), when in fact Feyerabend begins to criticize falsificationism in the early 1960s and then a generalization of the implications of his 1962 criticism of Popper's methodology is what led him to break with Popper's school in December 1967.

1.6 Feyerabend's Test-Model: Conjectures and Novel Corroborations

On this interpretation, from ERE (1962) through **Against Method**, Feyerabend's methodological argument for theoretical pluralism is based on Feyerabend's test-model for comparing rival incommensurable universal theories empirically as illustrated by the Brownian motion example. The argument, the test-model and the example are the heart of Feyerabend's theoretical pluralism *in contrast to* Popper's theoretical pluralism based on Popper's test-model: falsification. As we have seen, Feyerabend's views are not always clearly developed and presented *in contrast* to or as a criticism of Popper's pluralism. But in ERE (1962), where they are first set out, this is explicitly what he was trying to do. In fact, the best way to understand Feyerabend's methodological argument for theoretical pluralism and his test-model based on the methodological implications of the Brownian motion example is to compare and *contrast* it to its closest sibling, Popper's argument for theoretical pluralism based on Popper's test-model (falsification) as illustrated by Popper's examples in "The Aim of Science" (1957). Why? Because this is exactly how Feyerabend develops his methodological argument for theoretical pluralism in ERE (1962). According to Feyerabend, either rival theo-

ries can be consistent and inconsistent with respect to their corresponding predictions and falsification is possible, as Popper's test-model correctly illustrates with the transition from Kepler and Galileo's lower-level laws and theory to Newton's universal (framework) theory, *or* the rival theories can be incommensurable and they cannot can be consistent and inconsistent with respect to corresponding predictions deduced from them, like in the case of the transition from classical mechanics to relativity theory. [195] In other words, Feyerabend argues that Popper mistakenly treats the two transitions as the same (as falsifications), when one is a transition between lower-level laws and a new commensurable universal theory, and the other is a transition between rival universal theories that are incommensurable. In this way, in ERE (1962), it is Popper's example (Newton's theory replacing Kepler and Galileo's accounts), illustrating Popper's test-model (falsification) to support Popper's argument for pluralism *in contrast* to Feyerabend's example, as illustrated by Feyerabend's test-model and the Brownian motion example, *as a criticism of Popper's normative methodology* (falsificationism), which misdescribes the justification for the transition from Newton to Einstein's theories, and more generally, cannot take testing rival incommensurable theories into account due to meaning variance in the terms used to state them.

Popper's Test-Model and Popper's Argument for Theoretical Pluralism

In Popper's 1948 paper on natural laws and theoretical systems and in Popper's account of the logic of scientific justification, [196] Popper argues that even direct refutation (deducing a prediction form a theory that contradicts an accepted basic statement) is insufficient for falsification, which is something much stronger. With direct refutation, without an alternative that explains the anomalous prediction, the question remains "open" with respect to how to proceed. However, according to Popper's falsificationism, with a falsification, the question is "closed" (or resolved at least for the time being), and in this sense, it serves as

[195] For example, Feyerabend asks: "Under these circumstances, the set of laws *T*'' following from T inside *D*' will either be inconsistent with *T*' *or* incommensurable with it?" (Feyerabend, ERE (1962), p. 92). It is an either/or. Notice that in ERE (1962), incommensurability is clearly contrasted with the possibility of there being logical inconsistency between statements deduced from incommensurable rival theories.

[196] See Popper, "Naturgesetze und Theoretische Systeme..." and Popper, **Logik der Forschung...** and Popper, **The Logic of Scientific Discovery...**

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a crucial experiment between them. In Popper's account, this provides the justification for recommending that the new theory should replace the established theory. In this way, falsificationism is the basis of Popper's argument for pluralism. Alternative theories are needed for falsifications as stronger tests than direct refutations, which are otherwise easily dismissed with *ad hoc* maneuvers (see section 2.8 and the backstory, on Feyerabend's relation to the impotence of Ehrenhaft's experimental effects). For Popper, even if there were a new theory that might explain an anomaly (something the existing theory does not explain), even that by itself would not warrant changing to it, unless it can be shown that the new theory also predicts all the successful predictions that had been deduced from the established theory. In this sense, in progress by falsification, empirical content grows cumulatively. In a falsification, one theory could explain all the observations statements the other could explain by predicting them correctly, while also predicting something correctly that its rival predicts incorrectly (the falsifier). [197]

In other words, according to Popper's test-model, in contrast to Hempel's account of a more comprehensive explanation, the new theory should not just have to show that its rival fails to be able to explain as much, and that it can predict everything its rival successfully predicts, but also that its rival is *wrong* about something it purportedly should be able to explain (the falsifier). Falsification is based on formal relations between statements deduced from each theory. That is what does the work, formally speaking. That is how one theory falsifies its rival. For Popper, for a successful falsification, all the successful predictions of the existing theory should be explainable, and those that were incorrect should be "replaced" by something better, the predictions of the new theory. Popper is very explicit about this. Earlier confirmed predictions of the existing view should count as cor-

[197] By contrast, Feyerabend's model is basically, one theory can (potentially) predict everything *better than* the other, which only thought it was making correct predictions, plus something special, something that shows that while there is an apparent anomaly for its rival (Brownian motion), its rival could never have discovered (by itself) that this anomaly in fact refutes it. This is because an incommensurable rival theory is needed to establish the fact (the stochastic character of Brownian motion is caused by atoms) that undermines the ontological principles the established theory implies, which is why it cannot be falsified by predictions deduced from the new theory. The "theory-ladenness" of the fact renders it deductively disjoint from the established theory that it then can only indirectly refute by positing an incompatible ontology with rules of usage that imply that the terms used to state the existing theory have zero extension, not directly refute by establishing formal relations between corresponding predictions.

roborative evidence within the margin of errors in which their predictions overlap. In this sense, according to Popper (as cited in ERE [1962]), the successful predictions of the earlier theory are explained, and its unsuccessful predictions are corrected by the predictions deduced from the new theory that replaces them.

According to Popper, a falsification is an "indirect refutation" of one theory by another and not a "direct refutation" of a theory by predictions deduced from it, because confirmed predictions deduced from one theory should logically contradict statements deduced from the other. This is an indirect refutation because the new theory is falsifying the existing theory, in contrast to when a theory is directly refuted by its own predictions. For a falsification, the better theory should have to show that it is better corroborated than its rival can be, by showing that its rival is wrong about a falsifier deduced from it that the new theory correctly predicts. This is how new theories should indirectly refute established views according to Popper's falsificationism, and this is why pluralism is necessary for progress according to Popper. Put directly, for Popper, pluralism is necessary for progress because progress is by falsification. For Feyerabend, pluralism is necessary for progress because revolutionary progress cannot be by falsification.

Feyerabend's Test-Model and Feyerabend's Argument for Theoretical Pluralism

Feyerabend's Brownian motion example is very different from Popper's falsificationism. It seems to be dialectically related to it in that what is tested is the opposite (statements about everything as opposed to statements about particulars) and the relation between the two theories is not a formal relation because they are deductively disjoint, while the two test-models complement each other by showing their respective limits of validity.

According to Feyerabend's test-model, to test incommensurable theories, the predictions of the new theory should not have to be logically consistent with the predictions deduced from the existing theory in the domain that those predictions agree and are both confirmed. Hempel and Popper's accounts both require this condition, which is achieved in each of their accounts by deduction of corresponding predictions from the rival theories. For Feyerabend, in contrast, when the theories are incommensurable, these apparent overlaps in confirmed predictions (within some acceptable margin of error) are merely the basis of pseudo-agree-

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ments based on the same *sentences* used to make different statements when deduced from each theory and used to test them. Feyerabend is claiming that these statements are not logically consistent, although the same sentences are used to make them. Put another way, when using these sentences to test theories, agreement is not about the content of the statements made using the same sentences, it is about which sentences to use to test the theories, regardless of what statements they may be used to make, when interpreted according to rival incommensurable theories. A key distinction in ERE (1962), and throughout Feyerabend's early philosophy ever since his 1951 dissertation [198] is drawn from a pragmatic (or casual) theory of observation sentences combined with a contextual theory of meaning. It is the distinction between observation *sentences* and observation *statements*. The terms "observation sentence" and "observation statement" are used to specify the respective conditions for the uninterpreted and interpreted status of a casual interaction with words:

> [T]he pragmatic conditions concern the relation between observation-sentences *(not statements)* and human beings *without making any stipulation as to what those sentences are supposed to assert*. Further conditions will have to be added if we want to obtain a fully-fledged language. *Any complete class of such further conditions will be called an interpretation*. A particular observation-language is completely specified by its characteristic [i.e. a set of pragmatic conditions] together with its interpretation. *The distinction between the pragmatic properties of a language and its interpretation is clear and unambiguous*. [199]

Feyerabend is suggesting that while we interact casually with sentences based on agreements about how to use them correctly, we also interpret sentences according to some theory, which is what turns those sentences into statements. With this idea, Feyerabend saw himself as developing suggestions he saw in Carnap's 1932 paper. [200] In ERE (1962),

[198] See Feyerabend, **Zur Theorie der Basissätze...**, p. 60.

[199] Feyerabend, "An Attempt at a Realistic Interpretation...", pp. 145–146 [emphasis added].

[200] See Rudolf Carnap, "Die physikalische Sprache als Universalsprache der Wissenschaft", *Erkenntnis* 1932, Vol. 2, pp. 432–465. For an excellent discussion, see Kuby, "Carnap, Feyerabend, and the Pragmatic...".

> an observation sentence is distinguished from other sentences of a theory not by its content, but by the cause of its production, by the fact that its production conforms to certain behavioral patterns. [201]

Moreover, "the interpretation of an observation language is determined by the theories that we use to explain what we observe, and it changes as soon as those theories change". [202] For Feyerabend:

> Taken by themselves the indication of instruments do not mean anything unless we possess a theory which teaches us what situations we are to expect in the world, and which guarantees that there exists a reliable correlation between the indications of the instrument and such a particular situation. [203]

Taken together, the pragmatic theory of observation sentences and a contextual theory of meaning imply that one sentence makes two incompatible statements when it is interpreted with incommensurable rival theories. For example, Feyerabend was trying to explain that rivals used the same sentence to covertly make incompatible statements when testing theories about "momentum" and "impetus". In the example to which Feyerabend refers, the sentence "The ball fell" was interpreted to mean either that it was pushed or that it was pulled. The statements about how far into the wax block it penetrated, which were used to test the rival theories, cannot be logically consistent, even if they appear to be, because one sentence is covertly used to express one of two mutually exclusive statements: "The ball fell that far into the wax" implies either that "The ball was pushed down that far into the wax" or "The ball was pulled down that far into the wax" (or something else). According to Feyerabend, this happens whenever rivals use deductively disjoint theories to interpret the results of the same experiments that are used to test them. According to the new theory, all the predictions that were thought to have been successful that are still expressed with the same sentences should be reinterpreted according to the new theory as part of the revolution. How the sentences are used does not change in those specific circumstances (recording the results of the measurement procedures), but what the sentences means does.

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[201] Feyerabend, ERE (1962), p. 36.

[202] Feyerabend, "An Attempt at a Realistic Interpretation...", p. 163. Compare Feyerabend, ERE (1962), p. 88.

[203] Feyerabend, ERE (1962), pp. 36–37.

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In a sense, Feyerabend seems to be settling for a lot less than Popper, even less than Hempel. Feyerabend argues that it should not be required for the new theory to be able to explain all the predictions deduced form the existing theory, because universal theories are deductively disjoint due to meaning variance, severing the logical relations between the statements deduced from the rival theories. Moreover, in cases when they appear to agree but formally do not, Feyerabend seems even willing to settle for *a* promise to *re-explain* all the classical effects realistically even as they continue to be discovered. Moreover, on Feyerabend's test-model (and Popper's), this is not "accomplished automatically by deducing the existing theory from the new proposal (or some form of it as in Nagel's account). For Popper, for a falsification, the established theory cannot be a deductive consequence of the new theory, because if it is then it cannot produce predictions by deduction that contradict the established theory that correct and replace the unsuccessful predictions of its rival so that the new theory can explain why the established theory is a good approximation.

In a sense, Popper and Feyerabend's test-models are both indirect refutations (requiring an alternative theory for testing, and then replacing, the existing theory), but not in same way. For Feyerabend, the new theory should not have to contradict confirmed predictions deduced from its rival falsifying it, nor formally be able to explain why its rival's predictions were successful to the extent that they were. Instead, it should have to do something that provides a stronger test than falsification. The new theory should show the existing theory was wrong about everything that it implies (including its ontological principles that purportedly apply to "everything") because it implies an incorrect ontology. Even the sentences it seemed to have predicted correctly, as it turns out, were incorrect statements because the terms used to state the established theory turn out to have zero extension. They do not refer to anything real.

According to the new theory, the established theory was wrong about everything that it was once thought to have correctly predicted and potentially explained. Statements deduced from deductively disjoint rivals cannot logically contradict, on Feyerabend's account, because they give different meanings to all the statements made that can be deduced from them, in order to be used to test them. Neither the ontological principles implied by rival universal theories about "everything", nor the measurable predictions deduced from incommensurable rival theories that are used to test them, can make statements that are logically consis-

tent or inconsistent with each other exactly because the theories are deductively disjoint due to meaning variance in the terms used to state them, which get their meanings from incompatible ontological principles (principles with incompatible rules of usage for the same terms). As we shall see, this does not mean that ontological principles and universal theories cannot be tested. It just implies that they cannot be tested according to falsificationism, that is, by establishing formal relations between heir corresponding predictions. Moreover, the test-model Feyerabend recommends sets a very high bar. New universal theories should show that their rival was wrong about everything, while explaining something that the rival, by itself, could never even have discovered *in fact* refutes it, just because a new incompatible theory is needed to establish that fact.

Feyerabend's problem with Popper's model is not *just* that a new theory should not have to contradict the existing theory's confirmed predictions. By calling theories "incommensurable" or "deductively disjoint" due to meaning differences, Feyerabend is saying that predictions deduced from the new theory *cannot* be used to confirm and thereby potentially explain the predictions deduced from the existing theory. Meaning differences render all statements deduced from incommensurable theories "deductively disjoint", regardless as to whether they are lower-level theories, laws, mere generalizations, or empirically testable statements about particulars deduced from their rivals, and regardless if they seem to be *prima facia* consistent or inconsistent in any particular domain, and regardless as to whether they are expressed as one and the same basic sentence, because basic sentences actually covertly make incompatible basic statements when they are interpreted by incommensurable rival theories. Similarly, when the rivals make *prima facia* logically contradicting claims with the same sentence negated, in fact, the incompatible statements that it can be interpreted to make do not logically contradict each other due to covert meaning differences. [204]

For Feyerabend, in cases of deductively disjoint universal theories, like Newton and Einstein's, it *cannot* be like the case described in Popper's example of testing commensurable theories. Falsificationism cannot take testing rival incommensurable theories into account. There can be no contradictions between predictions

[204] The differences are not covert in the sense that they cannot be identified and discussed in a meta-language, just that they are not usually explicit in the sense that after scientific revolutions, in our observation language and in everyday language, we continue to use the same sentences with no explicit indication that the meanings have changed.

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deduced from deductively disjoint rivals because of the meaning variance, and so recommending trying to falsify, as Popper's model suggests, is asking the impossible. In this way, Popper is arguing for pluralism with falsificationism, while Feyerabend is explicitly arguing for pluralism *against* falsificationism.

As we shall soon see, Popper is Feyerabend's ultimate target, but first it is important to emphasize that Feyerabend agreed with Popper in ERE (1962) and after, that falsifying instances that indirectly refute an alternative are much stronger than direct refutations:

> Indirect refutations of the kind described [i.e. based on alternatives] occur much more frequently than is usually admitted. It does not seem to be an exaggeration to assume that all decisive refutations make use of alternatives. In any case, a refutation that is based on a successful alternative is much stronger than is a refutation resulting from the direct comparison of theory and "facts". [...] The reason why a refutation is stronger is easily seen. The direct case is "open" in the sense that a different explanation of the apparent failure of the theory (of the inconsistency between the theory and certain singular statements) might seem to be possible. The presence of an alternative makes this attitude much more difficult, if not impossible, for we possess now not only the appearance of failure (*viz.,* the *inconsistency*) but also an explanation, based on a successful theory, of why failure actually occurred. [205]

Feyerabend also characterizes his test-model as an indirect refutation (because it makes use of an alternative to close the question), but in his test-model there is no falsification, nor should there be required to be one for new proposals, because the theories proposed may be deductively disjoint, and so can neither predict, nor logically contradict, the predictions deduced form the existing theory. [206] As Feyerabend points out in ERE (1962), Popper's test-model is a formal model of the transitivity of corroborative evidence. According to Popper, as set out in ERE (1962), "the empirical success of the old theory may be said to corroborate the new theory," so that "even the evidence of the explicandum itself becomes independent evidence for the new theory". [207] For Feyerabend, evidence is not formally transitive between incommensurable theories in this way. The observation statements that were earlier considered successfully confirmed predictions

[205] Feyerabend, "Problems of Empiricism...", pp. 249–250, fn. 122; compare Feyerabend, ERE (1962), pp. 91–92.

[206] In contrast to Popper as cited by Feyerabend in ERE (1962), pp. 92–93.

[207] Popper cited in Feyerabend, ERE (1962), p. 92.

cannot corroborate the new deductively disjoint universal theory. They are reinterpreted into different statements (often expressed by the same sentences) that count as corroborations of the new theory which successfully predicts and potentially explains them.

However, calling Feyerabend's test-model an indirect refutation like Popper's test-model, can be somewhat misleading, because in Feyerabend's test-model there are no logical relations between the two theories or any of the statements deduced from them. One indirectly refutes the other, by explaining something the other cannot, but in a way that is far more critical of the existing theory than a falsification. The test must accomplish much more to justify replacing the reigning theory changing our entire worldview. As we have seen, Popper's model of indirect refutation is based on a logical contradiction, as the falsifying evidence should be deduced from the new theory contradicting the prediction deduced from the existing theory, indirectly refuting it, with one and the same falsifier doing the falsifying. The correct contrast between Feyerabend's test-model and Popper's is essentially the difference between Popper's *falsification* based logical consistency and contradiction of corresponding basic statements and Feyerabend's account of a novel *corroboration* based on establishing a novel type of phenomenon; that is, a new *kind* of fact that shows why the existing theory could never have explained the anomaly by itself, and so never have discovered that it was definitively a refutation of it. With respect to universal theories (and any other deductively disjoint theories), the relevant logic of scientific justification cannot be based on formal relations between the prediction of rival theories. It should be based on the corroboration of a novel phenomenon that shows that otherwise that phenomena would continue to have remained anomalous and so could not refute the established theory. It is a novel corroboration that indirectly refutes the established theory by showing that the non-logical terms used to state it need to be replaced (together with the ontological principle that govern their correct use) because they have zero extension.

For Feyerabend, Popper's test-model should be replaced with respect to revolutions. Revolutions are and should not be justified by a process of conjecture and indirect refutation by falsification. [208] They should be justified by a conjecture and a novel corroboration that forces a revision to the ontological principles that give

[208] See especially Karl R. Popper, **Conjectures and Refutations: The Growth of Scientific Knowledge**, Routledge, London 1963.

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its terms meaning when the theory is realistically interpreted. Put more directly, revolutionary progress should not be made by not conjectures and refutations. It should be made by conjectures and novel corroborations. Universal theories cannot be indirectly falsified by predictions deduced from their incommensurable rivals, and they can always be protected from direct refutation, by conditioning all experience according to them with *ad hoc* maneuvers. However, an empirical justification for replacing a universal theory can still be given. That is what Feyerabend's test-model tries to show. Universal theories are superseded by rivals that are more general, in a swelling ocean of incompatible alternatives that compete for this privilege of being used to interpret experience realistically.

While Feyerabend praises Popper repeatedly in ERE (1962), and afterward, for emphasizing the need for an alternative for a falsification, Feyerabend's own argument for pluralism is not supposed to fail to take testing rival universal theories into account for the reasons Popper's does. Feyerabend's test-model is explicitly designed to take testing universal theories that are deductively disjoint (that have "no comparable consequences") into account so that there can still be crucial experiments of them without a falsification of one by another. That is how Feyerabend's argument is a constructive criticism of Popper's falsificationism in the context of Popper's maxim to try to increase testability. In fact, Feyerabend's test-model only works when the new theory cannot meet the two conditions that should be met for a falsification to be justified (successful predictions that are logically consistent and close inside the domain in which they overlap, and logically contradictory outside of it).

This should not be confused with Popper's claim, with which Feyerabend agrees, that established theories themselves should *not* be deducible from the new theory. As mentioned earlier, Popper recommends that the established theory should not be deducible from the new theory, otherwise it could not falsify it by contradicting some of its predictions, while at the same time, the successful predictions deduced from the established theory should be shown to be logically consistent to the deductive predictions of the new theory in the domain in which they overlap. When the predictions of the two theories start to diverge to the extent that one is no longer seen as a good approximation of the other, then the rival predictions become logically inconsistent with each other. In other words, for Popper (in contrast to Nagel), the established theory cannot be deduced from the

new theory, but some of the predictions of the established theory can, and some cannot.

According to Feyerabend, his model is the only way that can, and thus should, be used to try to test rival universal theories, because it allows for meaning variance, while providing guidelines for how to stage a crucial experiment between deductively disjoint rivals that does not require establishing formal relations for a falsification. Moreover, as Feyerabend increasingly emphasized while trying to explain his views, testing rival deductively disjoint theories empirically is supposed to offer an even sharper test then just testing them against sentences accepted by both parties. [209]

As Feyerabend tries to indicate in ERE (1962) in some footnotes along the way and in the last few cryptic pages of the essay, his argument has clear implications for Popper's account of "basic statements" [210] and for a very Popperian reason. Feyerabend's test-model tries to *increase* the testability of the existing theory by making evidence available for testing it that would otherwise always remain unavailable to it, not by "importing an anomaly" into the existing framework to logically contradict it, [211] but by showing that the established theory is wrong about the anomaly, and everything else, including "everything" by changing the rules of usage of the terms used to state the theories. According to Feyerabend:

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[209] For Feyerabend, incommensurable rivals also provide better means of testing than trying to test it with commensurable alternatives, because the consequences of his test procedure are much more far reaching, as it requires meaning changes that affect all empirical terms, forcing their reinterpretation.

[210] Feyerabend implies that basic statements are *BS* with respect to testing incommensurable theories, along the way in ERE (1962), and then more definitively after targeting Popper explicitly, when Feyerabend discusses the implications of Popper's account of testing understood as based on agreement of behavior and not agreement with respect to the content of the sentences used to test the theories, although Feyerabend, ERE (1962) rarely if ever seems to be understood that way. As we shall see, in ERE (1962) Feyerabend lumps Popper's basic statements together with Bohr's views on the irreplaceability of classical concepts, and rejects both for the same reason.

[211] Paul Hoyningen-Huene, "Paul Feyerabend and Thomas Kuhn", in: John Preston, Gonzalo Munévar, and David Lamb (eds.), **The Worst Enemy of Science? Essays in Memory of Paul Feyerabend**, Oxford University Press, New York 2000; Paul Hoyningen-Huene, "Three Biographies: Kuhn, Feyerabend, and Incommensurability", in: Randy Allen Harris (ed.), **Rhetoric and Incommensurability**, Parlor Press, West Lafayette 2005, pp. 150–175; and Karim Bschir, "Feyerabend and Popper on Theory Proliferation and Anomaly Import: On the Compatibility of Theoretical Pluralism and Critical Rationalism", *HOPOS: The Journal of the International Society for the History of Philosophy of Science* 2015, Vol. 5, No. 1, pp. 24–55.

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> The function of such concrete alternatives is, however, this: they provide means of criticizing the accepted theory in a manner that goes beyond the criticism provided by a comparison of that theory with the "facts". However closely a theory seems to reflect the facts, however universal its use, and however necessary its existence seems to be to those speaking the corresponding idiom, its factual adequacy can be asserted only after it has been confronted with alternatives whose invention and detailed development must therefore proceed any final assertion of practical success and factual adequacy. [212]

According to Feyerabend's test-model, the invention of an alternative universal theory can even be *necessary* in order to discover that some specific phenomenon actually refutes an existing universal theory, thereby indirectly refuting it without falsifying it. As Feyerabend argues, without a rival incompatible explanation, the existing theory would continue to be protected with *ad hoc* maneuvers (as it had been by many for more than half a century in the case of Brownian motion). [213] As Feyerabend later clarified:

> This, then, is the methodological justification of a plurality of theories: such a plurality allows for a much sharper criticism of accepted ideas than does the comparison with a domain of agreed facts stated in what is supposed to be given independently of *theoretical considerations*. [214]

Popper's basic statements are supposed to make testing theories independent from theoretical considerations possible. This is supposed to be part of Feyerabend's argument that his own test-model is a stronger test than Popper's falsificationism, because it offers a more stringent test of established views than Popper's falsificationism. Falsificationism is based on agreed facts stated as basic statements, which makes it harder to test basic statements than the theories from which they can be deduced. Perhaps for this reason, Popperians read the passage above as parroting Popper's argument for falsificationism (as an indirect refutation, it offers a more stringent means of testing than direct refutation), but it was

[212] Feyerabend, "How to Be a Good Empiricist...", pp. 14-15, *similar* in: Feyerabend, "Problems of Empiricism...", p. 150.

[213] Feyerabend focused on the use of *ad hoc* maneuvers to protect existing points of view in the context of his involvement and experience with the impotence of Ehrenhaft's experiments in the late 1940s, see the postscript to part 3 for the backstory and Ehrenhaft's foundational role in the genesis of Feyerabend's theoretical pluralism as first set out in ERE (1962).

[214] Feyerabend, "How to Be a Good Empiricist...", pp. 14–15, similar in: Feyerabend, "Problems of Empiricism...", p. 150 [emphasis added].

intended as part of a criticism of Popper's falsificationism as anti-progressive because requiring the predictions of a new proposal to explain basic statements deduced from incommensurable rivals would make it impossible to establish deductively disjoint (incommensurable) new theories just because universal theories have to reinterpret all the accepted basic sentences into new statements, most of which continue to be expressed with the same sentences in exactly the same contexts, but now with different meanings. For example, as it turns out, falling apples are neither pushed from within by impetus nor pulled from outside by a gravitational force, but when we try to test a theory by deducing a measurable prediction that the apple will fall under certain circumstances, and the prediction turns out to be correct, we still say "the apple fell".

Falsificationism is modeled on a comparison of rival theories within a domain of agreed "facts" stated in a language that is supposed to *function by agreement independently of theoretical considerations*. That is what makes basic statements basic, allowing them to play the role of "neutral" (agreed upon) falsifiers. Popperians are sure to object that Popper emphasized that basic statements are theoretical and open to test, just like theories. While this is correct, as observation statements are revisable conjectures for both Popper and Feyerabend, as we shall see, Feyerabend's point is that in Popper's account, while basic statements are supposed to be testable *just like theories*, they are not supposed to be *just as testable as theories*. According to Feyerabend, Popper's account makes them harder to test just because they have already survived so much extensive testing. Feyerabend is pointing to the possibility that all these purportedly successful tests may share a common mistaken ontological assumption, and if the theory is shown to have an incorrect ontology, it turns out to be wrong about everything, like in the transition from classical mechanics to relativity theory in contrast to the transition from Kepler or Galileo's laws to classical mechanics.

Feyerabend is trying to emphasize that new theoretical considerations *were necessary* to organize the evidence used to indirectly refute the established theory (by corroborating an ontologically incompatible rival that undercuts its ontology), because the established theory lacked the conceptual resources to potentially explain the evidence, just because it is based on an incorrect ontology. The evidence cannot even be stated without implying the established theory was wrong about everything, including every statement deduced from it that it was once thought to have been able to explain.

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Popper takes basic statements to be theoretical, but the *function* of basic statements in Popper's empiricism, is exactly to take controversial "theoretical considerations" out of the equation. In other words, theoretical considerations (given the "theory-ladenness" of observation statements) are supposed to be *irrelevant* to falsificationism according to Popper, just because the evidence used to test the theories is stated as basic statements that are agreed upon regardless of what they are taken to mean by those agreeing to take them as basic statements to test their rival theories. In this way, Popper tries to allow for meaning variance in the terms used to state rival theories, while still providing an empirical core (basic statements) for testing them according to formal relations. That Popper's model implies that meaning variance is irrelevant to testing theories can be seen by comparing his two examples. In the first example, the transition from Kepler and Galileo's laws to classical mechanics, there is no meaning variance, so there *can* be a logical contradiction between basic statements deduced from each of them that can serve as a falsification of one by the other. In Popper's second example, the transition from classical mechanics to relativity theory, there is meaning variance, but for Popper, there can still be a falsification based on a logical contradiction between agreed upon accepted basic statements.

Exactly here, Feyerabend disagrees with Popper. Meaning variance renders universal theories deductively disjoint, so there can be no formal relations between statements deduced from each and so no falsification of one by the other. From Feyerabend's perspective, the problem with Popper's test-model is that it only recommends that rivals should both *agree* to the same sentences used to express the results of the same set of measurements to be used to test the rival theories, but it does not require that they mean the same thing when doing so. For Feyerabend, as long as the sentences do not mean the same thing (are not interpreted by the same theory), there can be no formal relations between them, and so no falsification is possible.

As we have seen above, for Feyerabend, the function of such concrete alternatives is that they provide means of criticizing the accepted theory "in a manner that goes beyond the criticism provided by a comparison of that theory with the »facts«". [215] Putting "scare quotes" around ("facts"), emphasizes that with respect to rival incommensurable theories, agreement of basic sentences is not a genuine

[215] Feyerabend, "How to Be a Good Empiricist...", pp. 14–15, similar in: Feyerabend, "Problems of Empiricism...", p. 150.

agreement about genuine facts, just as disagreement about basic sentences is not genuine disagreement (that is, based on logical contradiction between *statements*). As we shall soon see, that is why in ERE (1962) Feyerabend criticizes Popper's account for cases of testing the predictions of rival universal theories empirically, in the long quote establishing Popper's position, with which he takes issue in ERE (1962), and which he indicates Watkins has tried to clarify in a letter. [216] The main conclusion ERE (1962) has been building toward is that agreement of behavior with respect to how sentences are used is not enough to establish agreement about their content, or about "facts" that can be given independently of theoretical considerations, and that is exactly why Feyerabend is ultimately going to conclude that "a formal and »objective« account of explanation cannot be given" for universal theories. [217]

It is very important to see that Feyerabend's argument in the final passages of ERE (1962) is supposed to be explicitly directed against Popper's account of basic statements, according to which agreement of behavior with respect to sentences is sufficient for empirically testing theories, regardless as to the content of the sentences about which they purportedly agree or disagree. As we shall see, following his big reveal that in the summary and conclusion section, Popper is in fact the ultimate target of his criticism, Feyerabend implicitly argues that Popper's basic statements cannot save falsificationism from his criticism, which limits its validity, just because Popper's account is based on agreement of behavior with respect to the use of *sentences* and not about the content of the statements made by those sentences. If universal theories are incommensurable, then they cannot genuinely agree or disagree about any "facts", because they offer incompatible interpretations of all of them.

Feyerabend is trying to explain that with respect to universal theories, basic statements deduced from one theory cannot logically contradict basic statements deduced from its rival, because the two sets of basic statements have different meanings, both when the rivals seem to agree to them and when they seem to disagree about them. In other words, from Feyerabend's perspective, basic statements are just "BS" in the vulgar sense, that is with respect to what is purportedly agreed or disagreed upon by rivals testing rival universal theories with them, and

[216] Feyerabend, ERE (1962), pp. 92–93.

[217] Feyerabend, ERE (1962), p. 95, final sentence.

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the role they are supposed to be play in Popper's methodology just because Popper's model ignores the difference between pseudo-agreements and pseudo-disagreement, merely disguised as genuine agreements and genuine disagreements, from genuine agreements and genuine disagreements. From Feyerabend's perspective, Popper's account overlooks the (Wittgensteinian) insight that the same sentences (or their negations) are used to express deductively disjoint, incompatible statements (or their negations). Feyerabend explains why meaning variance poses a problem for Hempel's account of explanation, and Nagel's account of reduction, when discussing his first illustration of pairs of incommensurable concepts (impetus and momentum):

> This being the case the demand for explanation and reduction clearly cannot arise if this demand is interpreted as the demand for the explanation, or reduction, of T', rather than of a set of laws that is in some respect similar to T' but in other respects (*meanings of fundamental terms included*) very different from it. *For such a demand would imply the demand to derive, from correct premises, what is false, and to incorporate what is incommensurable*. [218]

Feyerabend describes the situation very precisely. He is claiming that:

> there exist pairs of theories, *T* and *T*', which overlap in a domain *D*' and which are *incompatible* (though experimentally indistinguishable) in this domain. Outside *D*', *T* has been confirmed, and it is also more coherent, more general, and less *ad hoc* than *T*'. The conceptual apparatus of *T* and *T*' is such that *it is possible neither to define the primitive descriptive terms of T' on the basis of the primitive descriptive terms of T nor to establish correct empirical relations involving both these terms (correct, that is, from the point of view of T*). This being the case, explanation of *T*' on the basis of T or reduction of *T*' to *T* is clearly impossible. [219]

Feyerabend is discussing to the relationship between rival universal theories such as classical mechanics and the special theory of relativity. In ERE (1962), Feyerabend calls pairs of *concepts* incommensurable, and he explains that when he calls theories incommensurable, he means the concepts represented by pairs of corresponding terms used to state the theories, so that he is explicitly using "theory" as an abbreviation for such corresponding pairs of concepts. In other words, when Feyerabend says things like, *T* and *T*' are incommensurable, he

[218] Feyerabend, ERE (1962), p. 59 [emphasis added].

[219] Feyerabend, ERE (1962), pp. 57–58 [emphasis added].

means the corresponding pairs of concepts used to state *T* and *T'* are incommensurable. [220]

While Popper takes Feyerabend's "incommensurable" to imply incomparable or untestable, according to Feyerabend, incommensurable theories are testable and empirically comparable, according to his test-model of a crucial experiment between incommensurable rivals in contrast to falsificationism. However, for Feyerabend, because rival incommensurable theories are conceptually "incompatible" (one implies the other's concepts have zero extension) one cannot be deduced from the other, nor can one falsify the other. This miscommunication between Popper and Feyerabend with respect to their disagreement about methodology resembles a typical Wittgensteinian fog. It seems clear that a disagreement about a model of the logic of scientific justification should not be based on such a miscommunication. However, it seems even more clear that the justification for replacing one theory by another should not be based on covert miscommunications between rivals agreeing to test their theories using basic sentences that make different statements that have incompatible meanings with different implications. Falsificationism is supposed to take examples of testing incommensurable theories into account, but in so doing, it disguises miscommunication as communication, while basing falsification on formal relations between predictions deduced from rival theories that cannot exist due to meaning variance in the terms used to state those theories, because the basic sentences used to test the theories cannot mean the same thing if they are deduced from incommensurable rivals and in order to be used to test them.

Feyerabend's (Bohmian) example: Brownian Motion as a Crucial Experiment between Incommensurable Rivals

In ERE (1962), Feyerabend illustrates his test-model of a crucial experiment between rival incommensurable (deductively disjoint) theories with Perrin's confirmation of Einstein's prediction of the stochastic character of Brownian motion, which corroborated a deductively disjoint (or "incommensurable") universal theory (kinetic theory) that implies an incompatible ontology (atomism) with respect to the established theory and the ontology it implied (energeticism). Confirmation of Einstein's predictions shows that the best (only) available possible explanation

[220] See Oberheim and Hoyningen-Huene, "The Incommensurability of Scientific Theories...".

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of Brownian motion is that it caused by atoms. According to energeticism, there are no atoms. Everything is ultimately energy. The corroboration of kinetic theory by the conformation of Einstein's prediction of Brownian motion led to a revision of the second law of thermodynamics and the demise of energeticism and its ontology, which was replaced by: Atomism. While energeticism has all but been forgotten, the spirit of its ontological principle lives on in string theory and quantum loop-theory, which try to show that energy is still more fundamental than matter on an even lower level. But while everyone today accepts atoms, some seem to have seen more methodological implications in the Brownian motion example than others. [221]

Feyerabend argues that Einstein's successful prediction of the stochastic character of Brownian motion is an indirect refutation in the form of a corroboration that shows that the existing theory could never have discovered by itself that Brownian motion directly refutes it. In this case, according to Feyerabend, a conceptually incompatible deductively disjoint alternative showed that the existing theory was wrong about everything, while also showing that by itself, the established theory never could have found out that Brownian motion does in fact refute it. For that, Brownian motion needs to be uccessfully predicted and potentially ex-

[221] As we shall see, at the time (1962), Bohm was stressing the methodological implications of the Brownian motion example for several reasons, but few of Bohm's contemporaries (given his defense of realism, not even Feyerabend) seem to have been prepared to have endorsed Bohm's idea of the limited validity of all universal theories, which was also taken to be one of many methodological implications of the Brownian motion example by Bohm. Even universal theories, according to Bohm, have a limited validity, because they are universal on different levels. In ERE (1962), Feyerabend does not explicitly endorse Bohm's idea of the limited validity of all universal theories (just an ever-expanding ocean of alternatives), although Bohm's idea of the "qualitative infinity of nature" clearly seems significantly similar to some of Feyerabend's later ideas, such as the "abundance" in Paul K. Feyerabend, **The Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago — London 1999. After Feyerabend eventually realized the pernicious implications of his early scientism (a lesson belatedly credited to Wittgenstein), according to which we should use our best scientific theories as our guides to what is real, Feyerabend stops recommending taking science as the best guide for deciding what is real, emphasizing that the pursuit of happiness should play that role (see especially Kidd, "Reawakening to Wonder Wittgenstein..."). Feyerabend's early views on everyday language and scientific progress are most fully developed in his essay on "Linguistic Arguments and Scientific Method" published in 1969. It is precisely the scientism in this belatedly published paper (with respect to his break with Popper) that Feyerabend later tried to correct and distance himself from, while formalization continued to be one aspect of the problems he was dealing with in his seminars, such as the loss of interconnectedness (love) resulting in dehumanization promoted by the tyranny of *the* truth that, if not actively held back, can quickly devolve into hate and violence.

plained; and for that a new ontologically incompatible theory was required. Einstein's successful prediction of Brownian motion shows that there are shaking molecules, as that is what causes it, and this is what indirectly refutes energeticism by refuting its ontology without logically contradicting any predictions deduced from the established theory about Brownian motion. In ERE (1962), Feyerabend implies that incommensurable alternative proposals provide a better means of testing established views than new commensurable proposals tested by falsification, because the new theory can explain the anomaly in a way that shows that the other theory by itself could never have potentially explained it. By establishing that Brownian motion is incompatible with the ontology of phenomenological thermodynamics, the kinetic theory increased its empirical content as new evidence became available for testing it. At the same time, because the established theory's concepts turn out to have "zero extension" given the meaning variance that changed the system of classes used to state the theory, according to the new theory, the established theory was wrong about everything. The corroboration of the kinetic theory by confirmation of the predictions Einstein deduced from it provided sufficient warrant for replacing the existing theory with the new theory and its ontology, and for trusting that the new theory will eventually be able to resolve all the problems the existing theory thought it had already solved.

According to Feyerabend, falsification of classical phenomenological thermodynamics with Brownian motion is an impossibility, for two reasons. First, direct measurements measurements that do no interfere with the system are impossible to make. According to Feyerabend: "Such measurements [...] are beyond experimental possibilities" (he cites Fuerth with respect to "the extreme difficulties of following the motion of the Brownian particle in all its details"). [222] Moreover, from the perspective of classical phenomenological thermodynamics, there must be unexplained extra-energy coming from somewhere, otherwise Brownian motion would be breaking the law of the conservation of energy. The claim that there must be an external source of energy can be used as an *ad hoc* maneuver to protect the theory from being directly refuted by the phenomenon in this way, as proving there is no external energy would require proving the negative existence of something. But this is not Feyerabend's main reason, as Feyerabend himself

[222] Feyerabend, ERE (1962), p. 65, and fn. 18. He could have referred to Vigier, who had made the same point about the impossibility of direct measurements of Brownian motion in the discussions at the Colston conference in 1957.

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later emphasizes that what counts as a test is never fixed, and such experimental impossibilities periodically turn into experimental possibilities, and Feyerabend's test-model is specifically designed to show how this can happen given the introduction of a new ontologically incompatible theory. According to Feyerabend, Einstein used a theory that had been recently proposed to make new kinds of calculations of their stochastic character that allowed Brownian motion to serve as the basis of a crucial experiment between incommensurable rivals *by being theory-laden*, not by being neutral. This is the second and real reason that falsification was an impossibility (not that direct measurements were impossible at the time). Falsification by Brownian motion was impossible, according to Feyerabend, follows from the fact that a deductively disjoint (incommensurable, conceptually incompatible and ontologically) alternative was needed to be able to explain what Brownian motion is:

> the actual refutation of the second law was brought about in a very different manner. It was brought about via the kinetic theory and Einstein's utilization of it in the calculation of the statistical properties of the Brownian motion. [223]

The experiments that confirmed these calculations provided the novel corroboration. According to Feyerabend, Brownian motion cannot be used to falsify classical phenomenological thermodynamics in a crucial experiment directly, just because the corroboration of kinetic theory with Brownian motion as a stochastic phenomenon required developing incompatible concepts that imply that the concepts of classical phenomenological thermodynamics have zero extension, making it wrong about everything, even what it purportedly had gotten right. To do this required introducing incompatible rules of usage with respect to the corresponding pairs of terms used to state the rival theories. For Feyerabend, such novel corroborations *should be* theory-laden and deductively disjoint from accepted basic statements, but they are also given a herculean task, considering what should be, and was, accomplished, and given that *ad hoc* maneuvers are always available with which to protect established views, and (as Popper emphasizes) they are too often unjustifiably used in this way to exclude new theoretical proposals. [224] More-

[223] Feyerabend, ERE (1962), p. 65.

[224] According to Feyerabend, Popper and Kuhn, theories are *routinely* protected with *ad hoc* maneuvers when confronted with direct refutations that leave the question open. On Kuhn, see Paul Hoyningen-Huene, "Paul Feyerabend und Thomas Kuhn", *Journal for General Philosophy of Science* 2002, Vol. 33, pp. 61–68. Without ignoring some anomalies, there can be no worldview that purport-

over, to Feyerabend the fact that universal theories like classical mechanics are never falsified, but only superseded by better corroborated rivals, makes perfect sense, given that antiquated universal theories such as classical mechanics are still used to *guide* experiments and perform that can be used to learn new things, even after a rival (relativity theory) has undermined its ontology. They still continue to inform our understating of their rivals and reality.

Feyerabend's test-model can be used to explain both how there was a crucial test between incommensurable rivals without a falsification, and how resolving a typical Wittgenstein "fog" was involved. For example, the missing energy source was both "internal" and "external" to the system in that the motion is caused by the energy "internal" to the system at a lower "external" level, where different laws apply. So, as it turns out, there was an unaccounted-for "external" energy source, but it was already internal to the system, so no matter how hard they tried to rule out external energy sources with insolation, they never would have been able to establish that Brownian motion refutes the second law of classical phenomenological thermodynamics with direct measurements to explain what causes it.

In sum, according to Feyerabend's test-model as illustrated by Brownian motion, Brownian motion cannot falsify classical phenomenological thermodynamics, because a rival deductively disjoint universal theory (the kinetic theory) is needed to make the statistical calculations that served as confirmed predictions that corroborate the kinetic theory that potentially explaining what it is. Once presumed to be correct, the new theory implies that classical phenomenological thermodynamics and its entire ontology must be wrong (about everything it predicted and everything), so that there can be no falsification based on a logical consistency and contradiction between corresponding predictions about the "same" things or "everything". For Bohm, that all universal theories can only ever have a limited given that different laws apply on different levels, but not for Feyerabend, who was promoting the idea that with respect to a realistic interpretation of experience, there *should be* just one best universal theory applicable all the time everywhere at a time. They should compete.

Although Feyerabend almost always illustrated his test-model of conjectures and novel corroborations solely with the Brownian motion example, sometimes

edly conditions all possible theories according to it.

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he also suggested or alluded to other examples of experiments that required the invention of an incommensurable rival. [225] For example, according to Feyerabend:

> *Observer dependence* was therefore not refuted by the production of observations inconsistent with it [the established theory], or by the realization that a language using it was impractical. It was refuted by the construction, and confirmation, of a theory which worked with observer dependence, *and it could not have been refuted in any other way*. [226]

As we have seen, according to Feyerabend, with incommensurable universal theories, it's not conjectures and refutations, because when there is meaning variance, there can be only conjectures and novel corroborations that indirectly refute the existing theory by refuting its ontology through the corroboration of an incompatible rival. The incompatibility is due to incompatible rules of usage because the same terms are used to state the rival theories, but they have incompatible meanings. As we have seen, not just any old corroboration will do. It must fulfill several functions. Most importantly, the corroboration should show that its rival was wrong about everything and never could have discovered that fact by itself, just because it implies the wrong ontology.

1.7 The Purpose and The Targets: Disinfecting Contemporary Empiricism (Hempel, Nagel, *and* Popper)

In this section, we will establish Popper as the ultimate of three main targets of Feyerabend's criticism in a big reveal, built up dramatologically with hints along the way, until in the "Summary and Conclusion" at the end of the essay, Feyerabend *explicitly* argues that *Popper's empiricism* (today aka falsificationism) cannot take testing rival incommensurable theories into account. Popper's methodology cannot take testing rival incommensurable theories into account, according to Feyerabend, because meaning variance renders universal theories deductively disjoint so that the successful predictions deduced from the new theory can neither be logically consistent with the successful predictions deduced from the existing theory in the domain they overlap, thereby potentially explaining

[225] In a letter to Kuhn, Feyerabend mentions Eddington's infamous observations, without going into any detail.

[226] Feyerabend, "Linguistic Arguments...", p. 62.

them, nor can they logically contradict the predictions deduced from the existing theory outside that domain falsifying it. When testing rival incommensurable theories, a falsification is impossible because they are deductively disjoint due to the differences in the meaning of the terms used to state the theories. According to Feyerabend, neither of Popper's conditions for a falsification can be met, and so the suggestion that they should have to be met is anti-progressive, because it impedes the very progress it purports to promote. Falsification is impossible because incommensurable theories have *no comparable consequences*, or in other words, no statement deduced from one can enter formal logical relations with any statement deduced from the other, even when the rival *statements* are expressed with one and the same *sentence* or with the negation of the same *sentence*. In the former case, they are still not formally consistent. In the latter case, they still do not logically contradict. "As already indicated, the aim of this section of this essay is to *establish* this interpretation of ERE (1962) by showing how Feyerabend develops this conclusion in a big reveal toward the end of the essay in the summary and conclusions section. [227] In this way, his first unrevealed criticism is initially covertly directed at Wittgenstein's quietism, and the last application of his criticism he reveals is directed at Popper's falsificationism, which remains unstated

[227] In the next section, it will also be shown that Feyerabend's retrospective remarks and the unpublished record at the time (from 1960–1967) also corroborate this interpretation of ERE (1962) as an attempt to *criticize* falsificationism (in contrast to the widespread view that Feyerabend was trying to extend or improve falsificationism). In short, neither the published, nor the unpublished, record is compatible with the widespread view that Feyerabend was trying to *improve* or *extend* falsificationism until 1967, because he had explicitly tried to *criticize* and *replace* it with respect to the logic of scientific justification of revolutionary advance in a draft of ERE (1962), which was written between the spring of in 1960 and the spring of 1961. Feyerabend sent an unfinished and much shorter version of a draft of what became ERE (1962) to Figel at the MCPS in early 1960. Feyerabend notes that Feigl had pointed out to him that if the new theory's predictions are not logically consistent with the predictions of the established theories, they cannot contradict them either: "As Professor Feigl has pointed out to me, this difficulty also arises in the case or crucial experiments" (Feyerabend, ERE (1962), p. 94, fn. 115), repeating his acknowledgement to Feigl on this point in Feyerabend, "An Attempt at a Realistic Interpretation...". A complete draft was later circulated among Feyerabend's colleagues and friends in the mid-summer of 1960. See "Explanation, Reduction, and Empiricism", KP 423.4 (66 ss., T, which is the final draft version of Feyerabend, "Explanation, Reduction and Empiricism..." (1962), which was later retyped at the MCPS as Paul K. Feyerabend, "Explanation, Reduction, and Empiricism", HF 08-22-01 (139 ss., T). In 1960, Watkins, in his comments on the initial draft of ERE (1962), explicitly wrote that he does not understand Feyerabend's *criticism* of Popper's methodology, and Feyerabend then tried to clarify his *criticism* of Popper's methodology to Watkins. This shws that the draft of ERE (1962) was intended as a criticism of Popper's methodology from its inception in. As we shall see, this criticism of Popper's methodology which eventually led to his break with Popper's school in December 1967. See part 2.

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right up until the “big reveal” in the conclusion. More specifically, this section will establish that, and how, Feyerabend develops Popper’s falsificationism as the third and ultimate main target of his criticism of *contemporary empiricism*, after e criticizes Hempel and Nagel’s accounts. It’s Hempel and Nagel and Popper — oh my! (We're off to see the Wizard).

The Dramatology: Building up to The Big Reveal

Feyerabend lists four related things that he will show in his essay. First, he promises to show Hempel’s account of when one explanation is more comprehensive than another fails as normative methodology, because it cannot take testing rival incommensurable theories into account due to meaning variance. "Feyerabend argues that in the case of incommensurable rival theories, the new explanation cannot *explain* statements deduced from the established explanation, because meaning variance renders them deductively disjoint, so although the corresponding sentences may appear to be logically consistent, they are not. Insisting that the new theory should formally explain what the established explanation explains is anti-progressive because it puts an inappropriate, unfulfillable condition on the legitimacy of new proposals. If this condition should have to be met to justify replacing the existing explanation with a new explanation, then there could never be a legitimate justification for replacing the existing explanation with a new incommensurable proposal, just because statements deduced from an incommensurable proposal cannot be logically consistent with predictions deduced from an incommensurable proposal due to meaning variance in the terms used to state the rival explanations.

Second, Feyerabend promises to show Nagel’s account of reduction fails as normative methodologies for the same reason (it cannot take incommensurable rivals into account because of meaning variance) and so it too is anti-progressive because it stifles the progress it purports to promote by suggesting an inappropriate, unfulfillable condition on the legitimacy of new proposals. Nagel’s account of reduction as a normative methodology cannot take incommensurable rivals into account due to meaning variance in the terms used to state the theories that renders them deductively disjoint so that one cannot be a deductive consequence of the other. Specifically, the established theory (or some form of it) cannot *be deduced* from the new theory because meaning variance in the terms used to state the theories renders those theories deductively disjoint. Insisting that the estab-

lished theory should be shown to be formally deducible from the new theory in a reduction is anti-progressive because it puts an inappropriate, unfulfillable condition on the legitimacy of new proposals. If this condition should have to be met to justify replacing the established theory with new theory, then there could never be a legitimate justification for replacing the established theory with a new incommensurable proposal, just because the established theory cannot be deduced from (and thereby "reduced" to and thus explained by; hence "reduction") an incommensurable rival due to meaning variance in the terms used to state the rival theories. Third, Feyerabend promises to show that school philosophies (such as Platonism and Cartesianism) fail to make progress on traditional philosophical problems for the same reason, because they do not allow for meaning variance:

> Now, it will also turn out, *in the course of this essay*, that these are exactly the difficulties [the condition of meaning invariance] we encounter in trying to solve such age-old problems as the mind-body problem, the problem of the reality of the external world, and the problem of other minds. [228]

In other words, incommensurability will also be used to explain the apparent un-resolvability of genuinely philosophically foggy (Wittgensteinian) problems (see section 1.4). Feyerabend suggests that there is a positive function that dissolving (not resolving) such problems can play in progress in empirical science with his model of testing theories according to their ontological principles. (We will return to this point in the postscript to part 1). But then, fourthly, Feyerabend explicitly says there is still one last thing that he *also* intends to show. That is, *in addition* to criticizing Hempel and Nagel *and in addition* to explaining why assuming meaning invariance leads to genuinely foggy and unresolvable Wittgensteinian ("traditional") philosophical problems, Feyerabend explicitly distinguishes and emphasizes that there will be one more thing he is going to show.

Starting the new next paragraph, Feyerabend immediately adds: "It will *also* be shown that the demand for meaning invariance is incompatible with empiricism". [229] Empiricism is left completely unqualified. Feyerabend is plainly suggest-

[228] Feyerabend, ERE (1962), p. 31 [emphasis added]. In ERE (1962), Feyerabend argues either explicitly or implicitly that many fields are being held back by the condition that new theoretical proposals are consistent with established views. These include cognitive psychology, medicine, and quantum theory.

[229] Feyerabend, ERE (1962), p. 31 [emphasis added].

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ing that this demand (meaning invariance) is incompatible with what he often refers to as "contemporary empiricism" (sometimes "modern empiricism") throughout the essay. If his contemporary empiricist targets were just Hempel and Nagel, then why this "also"? Feyerabend then immediately states his goal:

> once contemporary empiricism has been freed from the elements which it still shares with its more dogmatic opponents [meaning invariance], it will be able to make swift progress in the solution of the above [traditional philosophical] problems. *It is the purpose of the present paper to develop and to defend the outlines of such a disinfected empiricism.* [230]

This establishes the main purpose of the essay: To disinfect contemporary empiricism so that it can take meaning variance into account. By stating his goals in this order, Feyerabend also opens a question: Who else (which other empiricists) is Feyerabend talking about? In other words, by introducing his main purposes *in this way*, without specifying whose empiricism *also* needs to be disinfected explicitly *in addition to* Hempel and Nagel's accounts, Feyerabend plainly implicitly raises a natural question for his readers to ask themselves: Who else's accounts of contemporary empiricism (in addition to Hempel and Nagel's) needs to be disinfected so that it can take meaning variance into account? Recall, as we have seen in section 1.1, a page earlier in the introduction, Feyerabend had already set up a similar reveal situation by claiming that "some philosophers" doubted that it would be possible ever to get outside a universal theory to test it and so make progress by replacing it with a new one, opening the question: Who are these "some philosophers"? As we have seen in section 1.4, later this turns out to be part of a criticism of Wittgenstein, who is explicitly criticized for holding exactly that view. Moreover, in the very first footnote, Feyerabend chastises "many thinkers for having been seduced by the similarity of form into thinking that the test procedures will be the same in both cases". [231] Who are these many thinkers?

In other words, readers had already been left to wonder who these "some philosophers" and who the "many thinkers" are supposed to include, and now just one page later and still in the introduction, Feyerabend leaves the reader wondering who the contemporary empiricists are that he is going to criticize after Hempel and Nagel. By adding the also and ordering his goals in that way, Feyer-

[230] Feyerabend, ERE (1962), p. 31 [emphasis added].

[231] Feyerabend, ERE (1962), p. 28, fn. 1.

abend signals that we should expect there to be further, as yet unnamed empiricists, whose accounts will also fall prey to Feyerabend's argument based on meaning variance. This is how Feyerabend sets up his big reveal, as his target is going to be part of his main conclusion with respect to disinfecting contemporary empiricism of its empirical core so that it can take meaning variance into account and thereby allow swift progress to be made.

Why would Feyerabend use "also" and explicitly add a last *additional* goal (freeing empiricism from the assumption of meaning invariance so that science can make progress unimpeded by dogmatic ideologies), setting up a reveal in this way, if there were no further empiricist whose view he is going to criticize as antiprogressive for not being able to take meaning variance into account? That this is the correct interpretation of Feyerabend's intentions is easy to test. If this is the correct interpretation of the introduction, that is, that Feyerabend is setting up a big reveal, we can simply check if Feyerabend does in fact finally explicitly turn his argument against any additional contemporary empiricists after criticizing Hempel and Nagel's views as forms of empiricism. This is easily confirmed by the summary and conclusion section, where Feyerabend *explicitly also* finally applies his argument based on meaning variance to his mentor's most prized achievement in philosophy of science (Popper's methodology, today also known as falsificationism) directly *after* he had finished and summarized his criticisms of Hempel and Nagel's accounts, *as part of the main conclusion of the essay*. [232]

In other words, Feyerabend's ultimate main target, as explicitly set out in the main text of the conclusion of the essay, is Popper's empiricism, which according to Feyerabend, requires formal logical relations between the predictions deduced from rival theories for a falsification, and so cannot take testing incommensurable rivals into account because of meaning variance in the terms used to state them. [233] In this way, Feyerabend's ultimate main target is also quite specifically precisely what Feyerabend tells us was his exact starting point: a problem with explanation in Popper's account, which is indeed the last thing Feyerabend explicitly shows to be incompatible with meaning variance in the terms used to state rival incommensurable universal theories in ERE (1962). This is why the essay is

[232] See Feyerabend, ERE (1962), p. 92: "In what sense, then, can *T* be said to explain *T'*? This question has been answered by Popper", and then see Feyerabend's argument against Popper's account directly thereafter.

[233] See Feyerabend, ERE (1962), pp. 92–95.

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titled “Explanation, Reduction and Empiricism” and not just “Explanation and Reduction”. [234] Put more plainly, the “Empiricism” in the title is supposed to stand for Popper’s methodology as ERE (1962) is ultimately explicitly a criticism of Popper’s empiricism, and this confirms that Feyerabend was trying to set up Popper’s falsificationism in a big reveal in the introduction.

In this way, even the title itself helps set up the big reveal. If he is criticizing Hempel on explanation and Nagel on reduction, who is he criticizing on empiricism? The answer is a key passage in the essay:

> It does not seem to me that it [Popper’s falsificationism] can deal with the case where T' and T are incommensurable. The reason is as follows. [235]

Feyerabend then proceeds to argue for the claim that Popper’s view cannot take testing incommensurable rival theories into account due to a lack of formal relations between corresponding predictions given meaning variance in the terms used to state them and in the terms used to state the theories that can be used to explain them. As Feyerabend puts it, incommensurable theories “have no comparable consequences”. This is how Feyerabend *explicitly* targets falsificationism with the same argument that he used to criticize Hempel and Nagel’s accounts for being anti-progressive as normative methodologies. None of these three accounts can take meaning variance into account. Or in other words, according to Feyerabend, this is also the reason that Popper’s methodology needs to be disinfected of its empirical core: So that it can take meaning variance into account. Only then can scientific progress proceed uninhibited by this unwarranted methodological

[234] Another “contemporary empiricist” Feyerabend *also* targets is Carnap, who Feyerabend praises together with *Popper* for trying to make decisions about testing a matter of agreement of behavior and not the content of the statements. In other words, while Feyerabend also explicitly says that his argument based on meaning variance undermines Carnap’s “dual language’ model in the course of the essay, Carnap is not a main target. The main focus in the final pages of the conclusion is definitely Popper’s falsificationism, and not Carnap “dual language” model, which had already been criticized for implying meaning invariance much earlier in the essay along the way and in prior publications.

[235] Feyerabend, ERE (1962), p. 93. That “it” refers to Popper’s falsificationism in this sentence is clear by the fact that Feyerabend is referring to the position delineated by Popper as represented by a long quote that explains how falsificationism works as a normative methodology that began on the previous page, which Watkins tries to clairfy.

dogmatic restriction Popper's methodology *unwittingly* shares with "school philosophies". [236]

It is important to notice how directly Feyerabend connects the contemporary empiricism he is going to criticize with school philosophies that also stifle progress because they do not allow for meaning variance. According to Feyerabend, contemporary empiricism and school philosophies both insist that "the meanings of certain key terms" are left "unchanged". [237] Feyerabend argues that falsificationism requires that the meanings of such "key terms" are left unchanged, otherwise formal logical relations of consistency and inconsistency cannot be established. The "key terms" in question in Popper's account, are the terms used in basic statements to test rival theories. Feyerabend presents these key terms as if they are supposed to provide a neutral basis for establishing formal relations between the predictions of rival theories in Popper's account. According to Feyerabend, in this way, Popper's basic statements are supposed to make meaning variance in the terms used to state the rival theories *irrelevant* to testing those theories. They purportedly provide a neutral language, as mutually agreed upon, that can be used as the empirical foundation for testing rival theories whose terms may have change meanings. In this way, Feyerabend sets up Popper's methodology *as an additional* form of contemporary empiricism that needs to be disinfected of its empirical core. Feyerabend is intimating that falsificationism requires that "the meanings of certain key terms are left unchanged", because if the meanings of the terms in the statements deduced from the rival theories have changed, then they cannot be formally related. Or in other words, if the theories are incommensurable, the corresponding (from the same measurement procedures) predictions deduced from them cannot enter formal relations because the rival theories are deductively disjoint, and so a falsification of one by the other is impossible.

More generally, Feyerabend's point is that, new theories cannot explain antiquated facts. According to Feyerabend's account, in contrast to Popper's falsificationism, crucial experiments between incommensurable rivals such as in the Brownian motion example involve incommensurable theories that are deduc-

[236] It is important to emphasize *unwittingly*, because Popper emphatically accepts that meanings vary in the terms used to state the theories. He just tries to make meaning variance irrelevant to testing theories by basing testing on accepted basic statements regardless of their meanings.

[237] Feyerabend, ERE (1962), p. 31.

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tively disjoint. The idea is that evidence used to undermine an incommensurable rival is theory-laden by an incompatible theory, so that evidence cannot enter formal relations with its rival, nor any of predictions deduced from it. One and the same set of measurement results provides two sets of evidence, when interpreted by incommensurable rivals.

Read in this way, by the end of the essay, everything falls into place and it becomes very clear that Feyerabend is primarily *targeting* views espoused by Hempel, Nagel, and Popper, all for the same reason (they cannot take meaning variance into account), and all with the same consequence: As normative methodologies, they are anti-progressive because they stifle the progress they purport to promote. [238] Specifically, Feyerabend argues that Hempel's account of a more comprehensive explanation erroneously implies that new proposals are only admissible if they formally contain, or are at least consistent with, the statements explained by the established explanations; [239] Nagel's account of reduction erroneously implies that established theories should be formally reducible as a limiting case of the new proposal; [240] and then Popper's empiricism erroneously implies that new theories should predict all of the successful predictions of the theories they replace (just as with Hempel), and that successful predictions deduced

[238] In Feyerabend, "Explanation, Reduction and Empiricism..." (1962), meaning variance is explicitly used to criticize various suggestions ascribed to Bohr (p. 43 and p. 88), Born (pp. 44-45, and fn. 39 and especially fn. 40), Carnap (p. 36, and pp. 41-42), the "Copenhagen interpretation" (p. 43 and p. 73), Heisenberg (p. 49, fn. 59, and especially p. 89), Hempel (p. 28, p. 34, pp. 41-47, p. 62, fn. 73, p. 64, p. 78, p. 92), "linguistic" or "Oxford' philosophers (pp. 60-61, pp. 75-76, and p. 75, fn. 91, p. 76, fn. 92, p. 81, pp. 86-87, especially fn. 104, and p. 91), Nagel (p. 28, pp. 32-34, pp. 46-47, pp. 55-58, and pp. 76-92), traditional philosophers (p. 31, fn. 6, pp. 43-46, and p. 91), von Weizsäcker (p. 49, fn. 50), and *some* Wittgensteinians (p. 49, fn. 50, for named names, and a further qualification, see Feyerabend, "Linguistic Arguments..."), and of course Wittgenstein himself. Moreover, all the accounts ascribed to these individuals are also criticized in subsequent publications, and more were to join the list, as Feyerabend repeatedly recast his methodological argument for theoretical pluralism, rehearsing the methodological implications of his meaning variance and model for testing rival incommensurable theories as illustrated by the Brownian motion example. For further references, see Oberheim Oberheim, **Feyerabend's Philosophy...**

[239] Feyerabend cites Hempel, "Studies in the Logic..."; Carl G. Hempel and Paul Oppenheim, "Studies in the Logic of Explanation", *Philosophy of Science* 1948, Vol. 15, No. 2, pp. 135–175.

[240] Feyerabend cites Nagel, "The Meaning of Reduction..." and Ernest Nagel, **The Structure of Science**, Brace and Company, New York — Harcourt 1961.

from the new theory should also formally contradict unsuccessful predictions deduced from the established theory, so that it is not *ad hoc*. [241]

In other words, ERE (1962) was intended as a criticism of Hempel (on explanation), Nagel (on reduction) and Popper (on empiricism) — hence the title: "Explanation, Reduction and Empiricism". These are the three main targets of Feyerabend's criticism based on meaning variance in ERE (1962): Hempel, Nagel *and Popper*.

In sum, Feyerabend argues that all three accounts erroneously posit formal relations that do not exist either directly between the incommensurable rivals themselves (Nagel), or between established observation statements and new kinds of explanations of them (Hempel), or between their respective predictions (Popper), and so would put undue dogmatic conditions on the legitimacy of new incommensurable proposals, and thereby stifle progress. Disinfecting Popper's methodology of its empirical core would allow it to take meaning variance properly into account (instead of glossing over the problem) and allow science to progress less inhibited. But this would require abandoning falsificationism (as a normative rule of thumb) as the one and only empirical method in and outside of the natural sciences, because basic statements deduced from rival universal theories cannot be formally consistent or inconsistent with each other, because the universal theories used to interpret them as empirical evidence are "incommensurable" or "deductively disjoint".

Hints that Feyerabend's Criticism Will Turn out to Apply to Popper's Methodology

If the dramatology was intended to be a big reveal, then we might expect Feyerabend to prepare the ground along the way to make his conclusions seem naturally to follow from facts that had already been agreed upon, and in fact that is exactly what happens as the essay progresses. In ERE (1962), in the introduction and along the way, Feyerabend *repeatedly hints* that his criticism of Hempel and Nagel will also turn out to apply to Popper as the "contemporary" empiricists that he will ultimately criticize, before he explicitly makes his big reveal with respect to Popper's account in the summary and conclusion section, where we finally

[241] See Feyerabend, ERE (1962), p. 93, where Feyerabend cites Popper to show that the second condition (logical contradiction) is what shows that "the new theory cannot possibly be ad *hoc*".

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learn that all of the contemporary empiricists he is criticizing share the feature that they recommend that deductive relations should be established either directly between deductively disjoint theories or between statements deduced from deductively disjoint theories. None of these accounts (Hempel, and Nagel, and Popper — oh my!) will work for universal theories, according to Feyerabend, due to the incompatibility of meaning variance and the formal relations on which all three accounts rely.

The first hint can be seen from the starting point and the proximity of Popper's account to the question Feyerabend leaves open. Directly after opening the question (who is the contemporary empiricist Feyerabend is ultimately going to criticize?) by claiming that "it is the purpose of the present paper to develop and to defend the outlines of such a disinfected empiricism" (who else's?), retrospectively the very next sentences can be seen as suggestive (once one realizes that Popper is explicitly the ultimate main target in the conclusion, when this becomes explicitly clear):

> Popper's admirable **Logic of Scientific Discovery** and his paper "The Aim of Science" have been both the starting point and the motive force of the investigations to follow. [242]

By itself, (taken out of context), the statement is rather ambiguous: One possible interpretation is that Feyerabend is saying he is simply going to try to extend Popper's methodological views, which motivated him to criticize other forms of contemporary empiricism. Another possible interpretation is that Feyerabend is hinting that he is going to criticize Popper's empiricism right alongside Hempel and Nagel's accounts and for exactly the same reason. As we have seen, not only does he explicitly *not* exempt Popper's empiricism from his criticism, he also explicitly concludes it does not escape his criticism in a big reveal. Feyerabend criticizes Popper's views on methodology by limiting their validity and he develops an account of testing incommensurable theories in contrast to falsificationism as part of a criticism of it within a critical rationalist framework. Furthermore, given that his starting point *only later* turns out explicitly to be a *problem* of explanation in Popper's account, when he argues that Popper's view cannot take testing incommensurable rival theories into account (just like Hempel and Nagel's accounts),

[242] Feyerabend, ERE (1962), pp. 31–32.

retrospectively Feyerabend seems to be hinting right from the start that his starting point is not going to be his end point.

The second hint can be seen in the effusive and profuse praise of Popper (his mentor, benefactor, and father figure) [243] in the introduction of the essay of exactly the view that he criticizes in the conclusion. Popperian readings of Feyerabend take this praise to be evidence that in ERE (1962), Feyerabend was merely repeating Popper and extending his falsificationist views, which he did not start to criticize until the late 1960s. However, on the reading suggested in this essay, the excessiveness of the praise reflects the severity and depth of the criticism to come. Feyerabend is criticizing what Popper took to be one of his greatest accomplishments: His normative account of the logic of scientific justification. He is claiming it shares an anti-progressive dogmatic element with the school philosophies from which it so eagerly tries to distance itself. Feyerabend's effusive praise about how "admirable" Popper's work is, put in the context of the criticism to come, clearly seems to be an attempt to blunt the sharpness of this criticism. On my reading of the introduction, Feyerabend's effusive praise seems to be something like although your account is indisputably *admirable*, as it works for testing commensurable theories empirically, it is going to turn out that unfortunately it cannot work for testing incommensurable theories empirically and so becomes part of a problem.

Third, even before all of that, Feyerabend takes an implicit jab at Popper in the very first footnote, which he uses to set up his criticism of Popper's methodology to come by showing where the limits of its validity are going to be:

> Many thinkers have been seduced by the similarity of form [between testing "empirical generalizations [...] such as "All *A*'s are *B*'s" and testing "[u]niversal theories"] into thinking that the test procedures will be the same in both cases. [244]

That Feyerabend clearly means Popper as one of these many thinkers, becomes be clear because according to Feyerabend in ERE (1962), Popper mistakenly treats the case of testing Galileo's laws with predictions deduced from classical mechanics as the same test procedure as testing classical mechanics with relativ-

[243] See Collodel and Oberheim, **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper…**

[244] See Feyerabend, ERE (1962), p. 28, fn.1.

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ity theory. That is also exactly where the problem with Popper's account lies, according to Feyerabend (see section 1.8). Precisely this distinction in the first footnote becomes part of how Feyerabend later targets Popper's methodology, as he uses the distinction between the two test-procedures set out in the first footnote to distinguish cases in which falsification works from cases it does not in the conclusion. Put more directly, Feyerabend uses this distinction to establish the limited validity of falsificationism. In other words, he uses this distinction to develop and delimit his criticism of falsificationism, arguing that falsificationism cannot take testing incommensurable universal theories into account, in contrast to testing mere commensurable (and lower level) empirical generalization, which falsificationism *can* take into account. [245]

A fourth hint is rather obvious. Feyerabend *explicitly* points out that the assumption he criticizes in Hempel's account of explanation interpreted as a normative methodology is an elaboration of Popper's account put in a more definite form by Hempel. In other words, Feyerabend views Hempel's account of explanation as part of Popper's view:

> Two very similar assumptions, or demands, play a decisive role in the orthodox theory of explanation, which may be regarded as an elaboration of suggestions that were first made, in a less definite form by Popper. [246]

The assumptions Feyerabend challenges in Hempel's account are the deducibility of the successful predictions of the existing theory form the new proposal and the requirement meanings remain invariant through explanation. If Feyerabend criticizes these assumptions in Hempel's account, and Hempel's account is an elaboration of Popper's account (as Popper's account is Hempel's account plus a falsifier), then we should very well expect Feyerabend's criticism of Hempel to apply also to Popper — and if not, then at least there should be a specific reason as to why not, given that Feyerabend is explicitly telling us that the views he is criticizing in Hempel an elaboration of Popper's views, just in a less definite form, [247] as one of Popper's two conditions for a falsification: First, logical

[245] Feyerabend, ERE (1962), p. 93.

[246] Feyerabend, ERE (1962), p. 33, citing Popper, **The Logic of Scientific Discovery...,** sec. 12.

[247] According to Feyerabend, Hempel is less ambiguous than Popper about Popper's first condition (logical consistency of overlapping predictions), which is why Feyerabend consults Watkins with respect to Popper's views on explanation in the process of falsification. Popper is ambiguous

consistency of statements deduced from rival theories in the domain in which they overlap and are successful; and second, logical inconsistency of statements deduced from rival theories in the domain in which the new theory is successful and the established theory is unsuccessful. This second condition is what Hempel's account as a normative methodology does not require, which is why Popper's methodology provides a stronger test and therefore better justification for replacing an established explanation with a new proposal than Hempel's account which leaves open the possibility that the new proposal is merely ad hoc. For Popper, that is what should justify replacing the existing theory with the new one (a falsification). New theories should predict everything the established theory correctly predicts, plus a falsification based on a logical contradiction between statements deduced from the rival theories.[248] That Feyerabend's criticism of Hempel must also apply to Popper's can also be seen by observing the fact that Popper's "Hypothetico-Deductive" model of testing theories and Hempel's "Deductive-Nomological' model of explanation are exactly the same, except that one is an account of explanation, while the other is an account of testing potential explanations (theories interpreted realistically). Again, it is also very hard to see how a criticism of Hempel's account, when applied to testing incommensurable rival theories, would not also apply to Popper's account. This is because Feyerabend argues Hempel's account fails for incommensurable rival explanations, because there can be no deductive relations between the statements deduced from them. If the lack of deductive relations between statements deduced from rival explanations undermines Hempel's account, then that same lack of deductive relations should also undermine Popper's account for the same reason.

A fifth (rather obvious) hint that Feyerabend thinks his criticism applies to Popper's views can be seen when Feyerabend compares Popper's "basic statements" with Carnap's independent "thing" language and Bohr's views on the irre-

because on the one hand he says the statements used as evidence that were used to support the earlier view are *explained* by the new theory and become evidence for the new theory by being logical consistent with the evidence deduced from the new theory, but Popper also can be read as suggesting that evidence supporting the old theory is *replaced* by the evidence deduced from the existing theory. In any case, for a falsification, the evidence successfully predicted by being deduced from new theory is formally supposed to be related to the evidence successfully predicted by being deduced from the existing theory, which Feyerabend rejects.

[248] In Popper's model, the statements deduced from the rivals should be couched as mutually accepted basic statements securing the possibility of using their rival predictions to test them empirically.

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placeability of classical concepts — finding fault with all of them because they do not seem to be able to deal with meaning variance. If Feyerabend was trying to repeat Popper's position and to not criticize it, then why does he not adopt Popper's account of basic statements instead of implying that it has the same kind of problem as Carnap and Bohr's views. According to Feyerabend's critical Popperian exposition of Bohr's, this would imply that view:

> In the case of classical physics, therefore, "every basic statement must either be itself a statement about relative positions of physical bodies [...] or it must be equivalent to some basic statement of this »mechanistic« [...] kind",

citing Popper, while emphasizing in the footnote that "Popper himself does not restrict his characterization to the observation statements of classical physics". [249]

This clearly implies that the same kind of criticism of Carnap's suggestion of a meaningful "thing language" independent from theoretical context and of Bohr's suggestion that classical concepts will forever be irreplaceable, also applies to Popper's "mechanistic" account of basic statements. Popper's methodology basis empirical testing of rival theories solely on statements deduced from them that are couched in a language that is accepted by both parties as basic statements so that they can either be logically consistent or inconsistent providing the possibility for a falsification, *even if the meanings of the terms vary*. Feyerabend's criticism of Popper's methodology is that statements that corroborate a new theory *cannot* be couched as basic statements in a neutral language accepted by both parties, because those statements must be deduced from incommensurable rival theories in order to count as evidence that can be used to test them.

As we have seen, according to Feyerabend's test-model, the new theory's explanation of the new fact can still indirectly refute the existing theory, but not by logically contradicting a prediction deduced from it. Instead, it refutes its ontology. Einstein's successful prediction of the stochastic character of Brownian motion was theory-laden rendering it deductively disjoint from the theory it indirectly refutes. However, by establishing that Brownian motion is caused by shaking atoms, the experiments corroborating this theory forced a revision of the second law of classical thermodynamics limiting its validity, while indirectly refuting the ontology of energeticism. In this crucial experiment between incommensu-

[249] Feyerabend, ERE (1962), pp. 35–36 and then p. 36, fn. 18.

rable rivals, no new predictions were deduced from classical phenomenological thermodynamics that logically contradict Einstein's prediction. More generally, the predictions deduced from rival incommensurable theories are theory-laden by the ontologically incompatible theories they are deduced from, so they cannot enter formal logical relations.

In the introduction to ERE (1962), Feyerabend also announces that one of his main aims in the paper is to show an inconsistency or *tension* between empiricism and the orthodox theories of explanation and reduction:

> As soon as this model is replaced by a model in which we make use of at least two factually adequate but mutually inconsistent theories, the first idea [of the two ideas of his position] becomes compatible with the demand for testability which must now be interpreted as a demand for crucial tests either between two explicitly formulated theories, or between a theory and our "background knowledge". In this form, however, the test model turns out to be inconsistent with the "orthodox" theory of explanation and reduction. It is one of the aims of the present paper to exhibit this inconsistency. [250]

This specific inclusion of "background knowledge" with respect to testing is yet another, sixth, very obvious hint that his criticism of contemporary empiricism is going to turn out to apply to Popper, which can be seen because Feyerabend explicitly praises Popper for showing that theories can be tested against background knowledge, while arguing against the idea that background knowledge provides a theory neural basis for empirically comparing universal theories according to formal relations between their corresponding predictions (see section 1.8). As we shall see, the inconsistency or tension that Feyerabend is trying to illustrate between empiricism and explanation and reduction turns out to be due to the incompatibility of meaning variance with the "principle of deducibility", according to which "explanation is achieved by deduction in the strict logical sense". [251] Feyerabend tries to show that Hempel's criteria for replacing an established explanation with a more comprehensive one is necessary but not sufficient for a falsification according to Popper's model, and yet Popper's model is inconsistent with Hempel and Nagel's models because Popper also suggests statements deduced from the rivals are formally inconsistent, which would be impossible if

[250] Feyerabend, ERE (1962), p. 30.

[251] Feyerabend, ERE (1962), p. 30.

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one theory could be deduced from the other. That this is what Feyerabend is getting at only becomes clear in the summary and conclusion section after Feyerabend's big reveal.

Yet another seventh hint comes when Feyerabend emphasizes: "According to Popper this procedure of testing a theory by comparing it with experience, as seen in the light of alternatives, is identical with *the* scientific method", [252] and just a few lines later "for Popper the test procedure outlined is *the* empirical method". [253] Here, Feyerabend is clearly distancing himself from Popper, not endorsing Popper's view. As we are about to see, Feyerabend is going to *criticize* Popper's methodology, arguing that it cannot be *the* empirical method as it cannot take testing incommensurable rival theories into account, as opposed to testing existing lower-level *empirical generalizations*, such as in the transitions from Kepler's laws and Galileo's theory of free-fall to classical mechanics, where falsification can and does work because there is no meaning variance.

Finally The Big Reveal: Popper's Falsificationism Cannot Account for Testing Incommensurable Theories

Having established the set-up, and examined some hints, we are now in a position to consider Feyerabend's big reveal directly. In the final section, "8. Summary and Conclusion", Feyerabend finally makes it *explicitly* clear that the ultimate critical target that is been building toward all along has been Popper's view, which was precisely his starting point and motive for criticizing contemporary empiricism in the first place. In the summary and conclusion, after developing a summary of his argument against Hempel and Nagel in the form of a paradox ("*Hempel-Oppenheim explanations cannot use satisfactory theories as explanantia. And satisfactory theories cannot function as explanantia in Hempel-Oppenheim explanations*"), [254] in footnote 113, Feyerabend finally reveals that a "problem of explanation" in contemporary empiricism, specifically, with Popper's account, according to which the corresponding predictions of rival theories should be *consistent* in the domain in which they overlap, was the starting point of his investigation.

[252] Feyerabend, ERE (1962), p. 73 [emphasis added].

[253] Feyerabend, ERE (1962), p. 74 [emphasis added].

[254] Feyerabend, ERE (1962), p. 92 [emphasis added].

In this way, ERE (1962) turns out to be a discussion of a problem with explanation that Feyerabend criticizes in Hempel's account of *explanation*, Nagel's account of *reduction*, and then ultimately Popper's account of *empiricism*, *and all for the same reason* (they share the same dogmatic element) *and with the same consequence* (they are anti-progressive); hence, the title of the essay: "Explanation, Reduction and Empiricism". These three accounts all fail as normative test-models for rival universal theories because they recommend trying to establish deductive relations between deductively disjoint theories. In Popper's case, according to Feyerabend, falsificationism requires logical consistency where their corresponding successful predictions match so that the successful predictions of the existing theory are potentially *explained* by the new theory, and it also requires logical inconsistency between a successful prediction deduced from the new theory and an unsuccessful prediction deduced from the existing theory. For this reason (requiring these formal relations), according to Feyerabend, falsificationism cannot take meaning variance that renders theories deductively disjoint into account. [255]

Moreover, Feyerabend's criticism of Popper's methodology is very similar to his criticism of Hempel and Nagel's accounts with respect both to how and why they are criticized, and with respect to the consequence the criticism is supposed to have for the account criticized. In Popper's particular case, Popper is criticized for recommending an empirical method (falsificationism) that would unwittingly stultify progress by putting two overly restrictive formal conditions on new proposals. In the same way, Hempel and Nagel's accounts would unwittingly stultify progress by putting overly restrictive conditions on new proposals when taken as normative methodologies. More importantly, according to Feyerabend, this is exactly what all three of these accounts (Hempel, Nagel and Popper) share in common with traditional, dogmatic school philosophies. None of them allow for meaning variance.

According to Feyerabend, there are *two* overly restrictive conditions in Popper's model as applied to incommensurable rivals. The first overly restrictive condition in Popper's falsificationism is that the predictions of the competing theories

[255] Popper's test-model is designed to take meaning variance out of the equation by making testing and thus justification a matter of agreement about basic statements regardless as to what they mean (their "content"). Feyerabend is criticizing Popper's model because it is based on the deducibility of logically consistent and inconsistent predictions, which requires that the meanings be invariant, which in fact they are not.

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must be logically consistent within the domain they are both overlap and are successful thereby potentially *explaining* them. Feyerabend's point is that if meanings vary, then deductive relations cannot be established between the new theory and the theory to be replaced, nor can the new theory potentially *explain* the successful predictions of the established theory. All it can formally potentially explain, according to Feyerabend, are its own successful predictions. On Popper's account, according to Feyerabend, the established theory itself is falsified, not explained, but the successful predictions of the existing theory in the domain in which they overlap with the predictions of the new theory become *evidence* for the new theory and are thus potentially explained by the new theory: "the empirical success of the old theory may be said to corroborate the new theory [...] even the evidence of the explicandum itself becomes independent evidence for the new theory". [256]

Feyerabend then proceeds to try to argue that if the theories are incommensurable (due to meaning variance in the terms used to state them), then formal relations between the new theory and the predictions deduced from the existing theory cannot be established. What he wants to say is that revolutionary new theories do not explain the successful predictions deduced from the theories they replace. Just as the new theory replaces the established theory, so too do new predictions replace the predictions deduced from the established view, just as new kinds of facts replace the old facts, when the same observation sentences are re-explained accordingly.

The second overly restrictive condition for incommensurable theories is specific to Popper's falsificationist methodology. It features neither in Hempel's explanation, nor Nagel's reduction. Moreover, although this new condition is overly restrictive for testing incommensurable theories, this is also exactly the condition that makes falsificationism *preferable* to Hempel and Nagel's accounts for testing commensurable theories, because it provides a stronger test, making it harder to protect the existing theory with *ad hoc* maneuvers (according to Feyerabend, supportively citing Popper). This is not the main target of Feyerabend's criticism of Popper's methodology as one of his three main targets. For a Popperian falsification, at least one of the predictions deduced from the existing theory must logically contradict a successful prediction deduced from the new theory so that without an alternative theory that predicts a statement that logical contradicts a pre-

[256] Feyerabend citing Popper, "The Aim of Science...", p. 33.

diction deduced from its rival, there is no falsification. If the new theory merely potentially explains what can already be explained, and nothing new that the established theory can't also potentially explain, then the new theory may be *ad hoc*. It would seem to have been developed exactly for that purpose, and it has no independent empirical content. For Popper, the new theory does not just have to offer something new with respect to what can be predicted, the new theory also should show that it succeeds where its rival fails. That is how it shows it is not merely an *ad hoc* extension of the existing theory *by establishing a formal inconsistency between their corresponding predictions*.

Feyerabend's view implies that Bohm's proposal has independent empirical content even if they are empirically indistinguishable, because empirical content does not exist independently of the theories that give meanings to the statements used to test it so that Bohm's theory has a different empirical content, although it successfully predicts and thus potentially explains the same observation sentences, because it purportedly explains them as different kinds of processes, and as different kinds of facts. Moreover, new theories can make new evidence available for testing established theories and so should always be welcome as proposed alternatives.

According to Feyerabend, in a big reveal, meaning variance that renders rival theories incommensurable (deductively disjoint) makes fulfilling Popper's conditions impossible. To see this big reveal in its context, note that Feyerabend targets falsification very prominently in the "Summary and conclusion" of ERE (1962) as the first part of his two-part final conclusion. To do this, Feyerabend uses a very prominent relatively *long quote* from Popper, and then a short clarification of it by Watkin's, [257] to try to pin down Popper's view, specifically with respect to the role explanation plays in it, to show that the same considerations that led to his rejection of Hempel and Nagel's accounts with respect to scientific revolutions also ap-

[257] Watkins is used to clarify Popper's misleading suggestion in the long quote from Popper that Newton's theory "explains" Kepler's laws, when in fact, on closer inspection Popper really means that while the successful predictions of Newton's theory *falsify* Kepler's laws, as they are logically inconsistent with their predictions, Newton's theory does *not* "explain" Kepler's laws themselves, because those laws cannot be deduced from it. Put more directly, Feyerabend uses Watkins to clarify that according to Popper, Newton's theory does not explain Kepler's laws, it explains why Kepler's laws are good approximations, empirically speaking by predicting and correcting their observation statements.

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plies to Popper's falsificationism. According to Feyerabend, *Popper* says that Newton's theory:

> unifies Galileo's and Kepler's. But far from being a mere conjunction of these two theories — which play the part of *explicanda* [things being explained] for Newton — it corrects them *while explaining them*. The original explanatory task was the deduction of the earlier results. It is solved, not by deducing them, but by deducing something better in their place: new results which, under the special conditions of the older results, come numerically very close to these older results, and at the same time correct them. *Thus the empirical success of the old theory may be said to corroborate the new theory*; and in addition, the corrections may be tested in their turn [...] What is brought out strongly by (this) [...] situation [...] is the fact that the new theory cannot possibly be *ad hoc* [...] Far from repeating its explicandum, *the new theory contradicts it and corrects it*. In this way, even *the evidence of the explicandum itself becomes independent evidence for the new theory*. [258]

Here we see that in the summary and conclusion, Feyerabend finally very concisely presents the specifics of Popper's account of empiricism as a normative methodology (also known as falsificationism) *in Popper's own words with respect to the role explanation plays in Popper's account*, which he then proceeds to criticize. Note, as we have seen, the reader was promised an additional criticism of a further contemporary empiricist after Hempel and Nagel and here in the conclusion a new form of empiricism (Popper's falsificationism) is finally very conspicuously presented. Moreover, here Feyerabend does not lather praise on Popper or falsificationism for being "admirable" (as in the introduction). Here in the conclusion, he explicitly tries to *criticize* a problem of explanation in Popper's account, arguing that it cannot take comparing universal theories into account due to meaning variance in the terms used to state them that renders them deductively disjoint ("incommensurable"):

> It seems to me that this theory [Popper's methodology] can well deal with all the problems that arise when *T* and *T*' are commensurable, but inconsistent inside *D*'. It does not seem to me that it can deal with the case where T' and T are incommensurable. [259]

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[258] Feyerabend, ERE (1962), pp. 92–93, quoting Popper, "The Aim of Science...", p. 33 [emphasis added].

[259] Feyerabend, ERE (1962), p. 93. Feyerabend later chastised Popper for not having noticed that ERE (1962), was not a repetition, but a criticism, of Popper's methodology. Note that Feyerabend's criticism of Popper was not buried in some footnote. Feyerabend's criticism of Popper's methodol-

1.8 The Argument: Falsificationism Cannot Take Incommensurability Into Account

The final section of ERE (1962) is section "8. Summary and Conclusion", which begins with a very brief summary of his criticism of Hempel and Nagel (two short paragraphs), followed by almost four pages that make a concluding argument: Popper's methodology cannot take testing incommensurable rivals into account because meaning variance in the terms used to state rival universal theories renders them deductively disjoint, so that there can be no formal relations between them or their corresponding predictions (resulting from the same measurement procedure), which is then generalised in the final sentence: No formal account of explanation can be given for incommensurable rivals.

According to Feyerabend's summary:

> Two basic assumptions of the orthodox theory of reduction and explanation have been found to be in disagreement with actual scientific practice and with reasonable methodology. The first assumption was that the explanandum is *derivable* from the explanans. The second assumption was that *meanings are invariant* with respect to the process of reduction and explanation. [260]

Feyerabend argues that by requiring formal relations for a falsification, Popper's methodology is committed to the second assumption for the same reason Hempel's is.

First Feyerabend re-emphasizes that Popper's account is better than Hempel and Nagel's accounts as normative methodologies, because it provides a stronger test, and consequently a better justification, for replacing the established theory with the new proposal. According to Feyerabend:

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ogy appears prominently in the summary and conclusion of ERE (1962), as a main conclusion in the main text which is then genialized to the claim (the second part of his two-part conclusion) that any formal account of explanation (through a scientific revolution, that is, of antiquated facts by contemporary theories in the course of scientific advance) must fail, not just Popper's.

[260] Feyerabend, ERE (1962), p. 91.

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> from the point of view of scientific method, *T* will be *most* satisfactory if it is (α) inconsistent with *T*' in the domain where they both overlap; and if it is (*β*) incommensurable with *T*'. [261]

This can be very confusing and may make it seem like Feyerabend is suggesting that rival theories can and should be both inconsistent and incommensurable with their rivals, but this is not what Feyerabend is suggesting, as can be seen by what he says directly below on the same page:

> Consider again *T* [new theory] and *T*' [existing theory] as described above. Under these circumstances, the set of laws *T*'' following from T inside *D*' will either be inconsistent with *T*' or incommensurable with it. [262]

So, for Feyerabend, it is explicitly an either/or: One possibility precludes the other. He is trying to say that *either* the laws deduced from the new theory can be inconsistent with the existing theory *or* the laws deduced from the new theory can be incommensurable with the existing theory. There are the two best possibilities. Thus, in the passage above that seemed to suggest rival incommensurable theories can also be inconsistent, Feyerabend was trying to say that while he has argued that Popper's model is *most* satisfactory (more satisfactory than Hempel and Nagel's accounts for cases of rival commensurable theories) in the case of commensurable theories, he also argues that his own account is most satisfactory (better than Hempel, Nagel and Popper's accounts) for cases of rival incommensurable theories. Otherwise, how can the new theory be *most* satisfactory when inconsistent predictions can be deduced from them and *most* satisfactory when this is not the case because meaning variance in the terms used to state them renders them deductively disjoint (that is, when they are incommensurable)? The riddle is easily resolved in the way indicated. The new theory is "most satisfactory" if predictions deduced from them are inconsistent when they are commensurable, and the new theory is "most satisfactory" if it is incommensurable with the existing theory due to meaning variance that renders them deductively disjoint for testing established universal theories.

Feyerabend is about to argue that his test-model is the only one that can take meaning variance into account *explicitly in contrast to Popper's methodology*,

[261] Feyerabend, ERE (1962), pp. 91–92.

[262] Feyerabend, ERE (1962), p. 92.

which (according to Feyerabend) basis testing on agreed upon (and thereby purportedly neutral with respect to content) basic statements in order to establish formal relations between rival predictions that allows us to justify the claim that the new theory is quantitively better empirically than the other. In this case, the set of successful predictions of the established theory should be a proper subset of the set of successful predictions of new theory, which would then justify replacing the existing theory with the new one. Notice that to set up his argument, Feyerabend explicitly points to the role of explanation in Popper's account (the starting point of his investigation). Feyerabend asks:

> In what sense, then, can *T* [the new theory] be said to explain *T*' [the existing theory]? This question has been answered by Popper for the case of the inconsistency of *T*' [existing theory] and *T*'' [the set of laws deduced from the new theory in the domain in which their predictions overlap]. [263]

This is the source of the problem of explanation under investigation in ERE (1962). Feyerabend is claiming that an explanation is only possible if the rival theories are commensurable. Thus, Feyerabend clearly is *implying* that while Popper's account satisfactorily answers this question with respect to commensurable rivals that are logically inconsistent, Popper's account is not going to satisfactorily answer this question with respect to rival incommensurable theories, and to show exactly this, Feyerabend introduces the long quote of Popper setting out Popper's methodology (see above).

Feyerabend is clearly trying to show that according to Popperian falsificationism, the established *theory* is not formally *explained* by being deduced from the new theory, and so is not reducible to the new theory in Nagel's sense. The existing theory should not have to be a deductive consequence of the new theory, because the new theory should produce a successful prediction that logically contradicts an unsuccessful prediction deduced from the existing theory, which would be impossible if the established theory were a deductive consequence of it. Feyerabend is trying to explaining that for Popper, the existing theory cannot be a deductive consequence of the new theory, because in order for the new theory to be deemed a better explanation, not only should it potentially explain and correct all of the correct predictions that had been successfully deduced from the existing theory, the new theory should show that it is not *ad hoc* by also falsifying the ex-

[263] Feyerabend, ERE (1962), p. 92.

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isting theory by successfully predicting a statement that logical contradicts a statement deduced from the existing theory. When the theories are incommensurable, the new theory cannot formally explain the established theory, nor any of its predictions even in the domain in which they appear to overlap by using the same observation sentences to make different statements.

To support his presentation of Popper's views, Feyerabend cites Popper's paper on "The Aim of Science" (1957). In that paper, in addition to the Kepler to Newton transition, Popper illustrates falsificationism with the confirmed discrepancies in the predicted locations of objects moving at relatively high velocities deduced from classical mechanics as correctly predicted by the special theory of relativity. According to Popper, these confirmed predictions (deduced from relativity theory) falsify the existing theory (classical mechanics) by logically contradicting predictions deduced from it. At the same time, all of the successfully confirmed predictions of classical mechanics (at relatively low speeds compared to the speed of light) are also deducible (within some margin of error) from relativity theory, so that the evidence supporting classical mechanics in the domain in which they are both successful and overlap is transitive with respect to relativity theory. In other words, according to Popper's account, relativity theory can explain the successful predictions can deduced from classical mechanics on the basis of formal relations of deducibility and logical consistency. According to Popper's account, these confirmed predictions *corroborate* relativity theory while classical mechanics is also falsified on the basis of a falsifier *exactly analogously* to how Kepler's laws and Galileo's theory were falsified by classical mechanics, which according to Popper explains why Kepler's laws and Galileo's theory are good approximations empirically to the extent that they are. Popper takes the chain "Kepler-Newton-Einstein" as his paradigmatic example of a series of falsifications in this paper. [264]

Feyerabend, in stark contrast, explicitly distinguishes between these two examples as different kinds of cases based on the methodological test procedures outlined in the very first footnote. The transition from Kepler to Newton is a commensurable transition from lower-level laws to a universal theory. To test these lower-level laws, they do not need to be deduced from any theory, but their successful predictions can. According to Feyerabend, *in contrast*, the transition from Newton to Einstein's theory is a scientific revolution: A transition in which one

[264] See Popper, "The Aim of Science...".

universal theory is replaced by another that is incommensurable with it. For Popper, both cases (testing commensurable theories and testing incommensurable theories) have one and the same test-procedure, as according to Popper, there is only one justifiable test-procedure for all empirical investigation: Falsification. For Popper, both cases (classical mechanics tested by relativity theory and Kepler's laws and Galileo's theory as tested by classical mechanics) were falsifications. By contrast, for Feyerabend in ERE (1962), Newton's classical mechanics is a universal theory, and universal theories cannot be falsified by incommensurable rivals like Einstein's relativity theory due to meaning variance in the terms used to state them. In other words, no predictions deduced from relativity theory can logically contradict any of the predictions deduced from classical mechanics, but more importantly (as scientists don't care about semantics, only philosphers), the new theory cannot explain the successful predictions of the established view just because the two theories are deductively disjoint due to meaning variance of the corresponding pairs of terms used to state the rival theories. According to Feyerabend this is the case *even when* the two sets of corresponding predictions deduced from the rival theories indisputably *prima facia* appear to be either consistent inside the domain they are both successful (as they are often expressed by exactly the same sentences that result from exactly the same measurement procedures) or logically inconsistent with each other (for example the discrepancies in predictions at high velocities relative to the speed of light of classical mechanics and relativity theory), where the rival theories make quantitively different predictions and one gets it right and the other wrong. That these measurements *falsify* Newton's theory seems to have been so self-evident to some people, that they simply did not realize that Feyerabend was trying to deny it According to Feyerabend, Newton's theory was never falsified.

According to Feyerabend, classical mechanics cannot be falsified by predictions deduced from relativity theory, because while some of the statements deduced from those theory appear *prima facia* formally to contradict, while others appear to be logically consistent within some margin of error (at relatively low velocities compared to the speed of light), their predictions cannot formally contradict if they have been deduced from incommensurable rivals that give them incompatible meanings. Feyerabend is insisting that just because they must be deduced from those incommensurable rival theories to be used to test them, the same sentences are given incompatible ontological interpretations when those in-

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commensurable rival theories are tested by them. One prediction implies something about how an object moved because of its absolute mass, and the other about how it moved because of its relative and rest masses, and "motion", "location", and "object" all take on new meanings when interpreted by incommensurable rival theories and deduced from them. That they do not have the same content can be seen by the fact and they do not have the same implications, and because they do not have the same content, they cannot formally contradict each other.

In Popper's model, according to Feyerabend, the correct predictions of the established theory become evidence for the new theory and are explained by it, while the incorrect predictions are corrected and replaced. For Feyerabend they are all replaced, as none of them can be explained by the new theory. They are all re-explained by a new theory that implies the established theory was wrong about everything, including all of its successful predictions. Even the ones once thought to have been successful are replaced together with the theory that had been thought to have explained them. The facts expressed by the same sentences used to express different facts become antiquated together with the theory that is replaced, while the sentences remain the same, but make different statements about different kinds of facts according to the new theory.

Not all of this always come across very clearly in ERE (1962). The following key passage appears to have been the source of quite some confusion:

> If it is correct, as has been argued all the way through the present paper, that the meanings of observational terms depend on the theory on behalf of which the observations have been made, then the observational material referred to in this modified sketch of explanation must be presented in terms of this theory also. Now *incommensurable theories may not possess any comparable consequences, observational or otherwise. Hence, there may not exist any possibility of finding a characterization of the observations which are supposed to confirm two incommensurable theories*. [265]

The italicized portion is a key passage in ERE (1962), because, as Feyerabend tells us in later publications, he is trying to explain that incommensurability just means deductive disjointedness and nothing else. By "consequences", Feyerabend means "predictions deduced from the theory". By claiming that none of them may possess any "comparable consequences", Feyerabend means, no predictions de-

[265] Feyerabend, ERE (1962), p. 92.

duced from incommensurable rivals can enter formal relations of consistency or inconsistency with any predictions deduced from its rival, because they are "deductively disjoint". In other words, this is the cryptic passage in which Feyerabend is claiming that none of the statements deduced from one universal theory can enter formal relations with any of the statements deduced from an incommensurable rival. This is precisely the root of the problem of explanation in Popper's account of falsification according to Feyerabend's analysis in ERE (1962). Feyerabend thinks that falsificationism erroneously implies that contemporary theories explain antiquated facts.

ERE (1962) has often been mistaken to imply that incommensurable theories cannot be empirically compared (because they have no comparable consequences observable or otherwise), while Feyerabend argues the opposite: that incommensurable theories can be empirically compared, but not by establishing formal relations between their corresponding predictions. According to Feyerabend, while incommensurable rivals have no formally comparable consequences, this does not mean they cannot be empirically compared. This is simply supposed to mean that statements deduced from them cannot enter formal relations (due to meaning variance in the terms used to state them). It is not supposed to mean that incommensurable theories cannot be empirically tested by crucial experiments. That is exactly what Feyerabend's test-model, in contrast to Popper's falsificationism, illustrated by the Brownian motion example, as set out in ERE (1962) is supposed to model.

It is important to emphasize that Feyerabend does not *explicitly* run through (once again), (as I am about to do) the consequences of his criticism of contemporary empiricism after his big reveal that his argument also applies to Popper's methodology, that is after arguing that the same criticism that applies to Hempel's account (which is part of Popper's account) also applies to Popper's account. However, it is clearly left explicitly implied. Feyerabend leaves it to the reader to reflect retrospectively on what those consequences were given the problem with Hempel's account, and how the argument has been structured. As we have seen, Feyerabend had indicated earlier that the contemporary empiricism that he will criticize shares the same undesirable feature with school philosophies that prevents them from making progress with traditional philosophical problems, which is why contemporary empiricism needs to be disinfected of its empirical core. The problem with school philosophies (Platonism, Cartesianism) is the same problem

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Feyerabend finds fault with in Hempel and Nagel's accounts (meaning variance), now turns out *also* to be a problem for Popper's account. Moreover, according to Feyerabend:

> It is interesting to note that the development toward dogmatism as it has been described in the text occurred both in the "school philosophies" and in empiricism, and, in the latter case, notably in physics: there is no indication that empiricism provides special protection against dogmatic petrification. The only difference is that its defense will be in terms of "experience" rather than in terms of "intuition" or "revelation". [266]

If this were not meant to include Popper's methodology, if Feyerabend thought Popper's methodology can take meaning variance into account, then why would Feyerabend not continue, except for Popper's methodology which protects science for dogmatic petrification by taking meaning variance into account? Put simply, the problem is, as normative methodologies, none of these accounts can take meanings variance into account. Just as school philosophies require meaning invariance and so do not make progress on traditional philosophical problems, all three accounts (Hempel, and Nagel and Popper's — oh my!) taken as normative methodologies stifle revolutionary progress, which involves meaning variance. Feyerabend tries to take meaning variance into account by showing how there can be crucial experiments between incommensurable rivals without a falsification that includes an *explanation* of the successful predictions deduced from the theory that is replaced. The new theory should be allowed to re-explain what were once thought to have been successful predictions, but which turned out to be about the wrong kinds of things, which turn out to have different properties.

As we have seen, the main targets of Feyerabend's criticism were Hempel, Nagel, and ultimately Popper, and the purpose of the paper is to develop a disinfected empiricism by recognizing the methodological implications of the fact that rival universal theories are incommensurable. Feyerabend's argument is that meaning variance renders them deductively disjoint so that new universal theories cannot explain the facts that were explained by the established point of view. In "8. Summary and Conclusion" Feyerabend explicitly targets Popper's methodology with his idea of incommensurability: deductively disjoint universal theo-

[266] Feyerabend, ERE (1962), p. 70, fn. 86.

ries. [267] For Popper, according to Feyerabend, falsificationism requires an alternative to the theory falsified exactly because the established theory cannot be falsified until a new theory both predicts all of the successful predictions of the established theory, and predicts a confirmed statement that logically contradicts a statement deduced from the established theory establishing formal relations between their corresponding observation statements. The successful predictions that had been made based on the established theory carrier over transitively as corroborations of the new theory *because they are logically consistent with them*. This is precisely the point where Feyerabend disagrees with Popper, because he does not believe contemporary theories can explain antiquated facts that were once expressed by the same sentences that we now use to express different statements about different kinds of facts.

Feyerabend and Popper both rejected inductive and deductive accounts of scientific progress on the level of successive theories. However, Popper's account of the logic of scientific justification offers a formal, deductive account of testing rival theories empirically. It is a deductive formal account of empirical content that makes empirical content cumulative through falsification. Falsification is a formal, deductive account of empiricism because it uses formal relations to claim that Newton's theory *falsifies* Kepler's laws because predictions deduced from Newton's theory are logically inconsistent with predictions deduced from Kepler's laws, while earlier corroboration of Kepler's laws still count as corroborations of Newton's theory as they are logically consistent with predictions deduced from it. According to Feyerabend, while these two conditions are fulfilled in the transition from Kepler's laws to Newton's theory, exactly these two conditions cannot be fulfilled when rival universal theories are incommensurable, because meaning variance renders them deductively disjoint, so that predictions deduced from them cannot enter formal relations required for a falsification. According to Feyerabend, by applying his model to cases of rival incommensurable theories, Popper posits formal relations (logical contradiction and logical consistency) between statements deduced from rival incommensurable theories that simply do not exist.

Seen in this way, this argument against Popper's methodology is structured to pin Popper down on "explains" and then box Popper in with Hempel. In other

[267] See Feyerabend, ERE (1962), pp. 91–95.

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words, Popper cannot simply wave Feyerabend's criticism off, by defending himself with the claim that falsificationism does not involve or provide a "formal" account of explanation, or any account of explanation, nor was ever even intended as such. It is just a formal account of empiricism (testing theories) not explanation, because Popper's formal account of testing is explicitly supposed to explain the formal sense in which a falsification has occurred such that it would also provide a formal explanation of the theory. By contrast, Feyerabend's test-model is not based on the formal relations of consistency or contradiction, but rather on incompatibility due to "the incommensurable character of the »conceptual apparatus« [268] of the two theories" (the terms used to make sense of experience and test the theories are interpreted according to incompatible rules of usage used to state the rival theories).

For Feyerabend, all that should need to be achieved in a crucial experiment between universal theories and the worldviews they are taken to imply is a genuine corroboration that provides a reinterpretation of all the confirmed predictions of the existing theory that shows the existing theory could never have discovered that it was wrong about a genuine phenomenon by itself, because its entire ontology is incorrect, so that the development of a new deductively disjoint language to re-describe the entire situation was needed to explain the otherwise anomalously occuring phenomena. Logical consistency nor inconsistency is required or even possible if the theories are deductively disjoint.

In this way, even Hempel's account of a more comprehensive explanation interpreted as normative criteria that should be used to decide when to replace one explanation with another (which is too lax in comparison to falsification which should "set the standard when there is no meaning variance) is still too restricitive in cases of meaning variance. It would be based on a pseudo-agreement that seems to be a logical consistency because of the shared form of a sentence, not because of genuine logical consistency of content. The idea is that "*A* is *A*" is logically consistent with "*A* is *A*", if and only if in both cases, the theories used to interpret *A* are commensurable. Otherwise, agreement about *A* is just a pseudo-agreement and disagreement about *A* is just a pseudo-disagreement, which can lead into Wittgenstein's foggy territory. In this way, Feyerabend allows for the pseudo-disagreement, that conflates "Brownian motion" (as anomalous) with "Brownian mo-

[268] Feyerabend, ERE (1962), p. 58.

tion" (as a stochastic movement caused by shaking atoms), to dissolve (like Wittgenstein's philosophical fog at sunrise), when Brownian motion was shown genuinely to corroborate the new theory.

Feyerabend's test-model for incommensurable rivals recommends trying to find a genuine corroboration from the new point of view that shows that the existing theory is completely wrong about everything, including "everything", and that it never could have found this out, without an incommensurable rival that shows that this is in fact the case (according to the new theory). The existing theory was completely wrong about everything because its concepts have zero extension, so there could be no genuine logical contradiction about anything. The novel corroboration showed that the established theory's ontological principles, that are supposed to apply to "everything", need to be replaced, because the terms used to state the theories do not refer to anything. (There is no absolute mass). While universal ontological principles should be used to connect our best universal theories to everyday experience, because they can condition all experience direct refutation becomes impossible, as they can always be protected from recalcitrant observations by *ad hoc* hypotheses. For Feyerabend, universal theories are supposed to shape our entire worldview by providing shared *a priori* principles relative to them, but we should still try to test them and replace them with better ones, even, in light of its their successes.

For Feyerabend, allowing for the invention of deductively disjoint alternatives used to test the ontological principles implied by an existing universal theory is the only way new worldviews can be born. The condition that predictions deduced from a new rival incommensurable theory should be logical consistent or logically nconsistent with predictions deduced from the established theory to justify replacing the existing theory and the worldview it implies is too strict and would act as birth control for genuinely new insights about everything. New initially seemingly strange theories and the ontological principles they may seem to imply may take some time to get used to, as for example, the idea that space can bend and stretch or the kind of non-locality implied by Bell's inequalities. Copernicus looked crazy to his contemporaries, as did Ehrenhaft. Only time can tell if something that looks crazy can turn into something credible. New ideas are hard to understand. The implications they have, or should have, for everyday language are not always clear. Universal theories do not interpret themselves, nor are their possible interpretations immediately well-understood or unequivocally accepted.

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Disputes (both pseudo and genuine) inevitably arise about what kind of new world the new universal theory implies. That is exactly why new ontological issues always inevitably arise as everything is reinterpreted according to a new universal theory in scientific revolutions, [269] according to Feyerabend's Kant-on-wheels, normative realism and his scientific conception of ontology. [270]

Feyerabend takes the last step in his criticism of Popper's methodology directly after explicitly targeting Popper and revealing that he had been motivated to write the paper by a problem with explanation in Popper's account (that also applies to Popper's account of what should count as a crucial experiment, that is, falsificationism). The passage can seem extremely cryptic, but basically Feyerabend argues that agreement about basic sentences and not the statements they make (which is all that is possible because universal theories are deductively disjoint) is simply not enough to support a genuine falsification, which would require formal relations between the content of statements deduced from deductively disjoint theories. Therefore, according to Feyerabend, Popper's account of basic statements is based on agreement about how to use some sentences (and not based on agreement about their content) so that the agreement cannot genuinely provide an empirical foundation for the justification for replacing an incommensurable theory providing a new conception of reality ("interpreted realistically", that is "as an explanation", that is as making "true" statements) [271] by another through a falsification. On this interpretation, Popper's basic statements turn out to be the empirical core of falsificationism that needs to be disinfected from the idea that they provide an agreement about content that establishes formal relations between predictions deduced from incommensurable rival theories.

Much earlier in the essay, Feyerabend had complained that:

> Nobody would dream of using the insight given by a new theory for the readjustment of some general beliefs only, leaving untouched the interpretation of the results of measurement. [...] Yet, precisely this is done when the measuring instrument is a hu-

[269] For Feyerabend's account of the relation between physics and ontology, see Feyerabend's Feyerabend, "Physik und Ontologie...".

[270] Compare Feyerabend, "An Attempt at a Realistic Interpretation...", ERE (1962), and Feyerabend, "Realism and Instrumentalism...", pp. 280–308.

[271] See Feyerabend, ERE (1962), pp. 93–95.

> man being, and the indication is the behavior of this human being, or the sensations he has, at a particular time. [272]

Feyerabend is trying to imply that a dis-infected empiricism must allow the meanings of observation statements (even basic statements) to vary with respect to progress through revolutions. What we take to be basic statements, or background knowledge, or well-understood observations should not be given a privileged status with respect to credibility. According to Feyerabend, we should be able to change the meanings of the terms used to make basic statements. Sometimes, they should be re interpreted according to a deductively disjoint alternative. In the final "Summary and Conclusion" section of ERE (1962), this complaint is applied directly to Popper's account of a falsification, given the roles that formal relations between basic statements are supposed to play in that account. Not only does falsificationism fail as a normative account of testing rival incommensurable theories empirically (because it implies that new theories should *explain* the successful predictions of its rival by being logically consistent with them), more generally, no formal or objective account of explanation is possible due to meaning variance in the course of progress through scientific revolutions, or as Feyerabend puts his final conclusion in the final sentences of the essay: a "formal or »objective« account of explanation cannot be given". [273]

Feyerabend's argument is that incommensurable theories cannot be tested by accepted basic statements that provide a "neutral" or shared observation language, because of meaning variance. Two rival universal theories interpreted the same sentences as making different statements that have different implications. When universal theories are incommensurable, the new theory cannot falsify the existing theory, but it can still be shown to be better because it is more general and it does not imply an incorrect ontology, because it can be shown to be more comprehensive than its rival by successfully predicting a *novel* corroboration based on a fact that is incompatible with the ontology of the established theory, given that the new fact can be shown that it does not imply an ontology that has also been discredited by that corroboration. Moreover, by offering new successful explanations of potentially everything, the new theory implies that the existing theory had been misdescribing all the (relevant) facts. That, as we have seen, is

[272] Feyerabend, ERE (1962), p. 37.

[273] Feyerabend, ERE (1962), p. 95, final sentence.

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how Feyerabend's test-model of a crucial experiment between rival incommensurable universal theories supposed to work without a falsification based on formal relations between corresponding predictions.

Of course, Feyerabend understood both that Popper's methodology explicitly endorses meaning variance and that it is only supposed to be a normative rule of thumb. He was very familiar with Popper's account (much more familiar than most). Either he was intentionally misrepresenting Popper's views, or he thought that his criticism of Popper still goes through even though Popper accepted meaning variance and that falsificationism is only supposed to be a normative rule of thumb. It seems clear that in ERE (1962), with respect to universal theories, Feyerabend suggests that Popper's account of basic statements cannot take meaning variance account due to incommensurability. He thought Popper was wrong about the relationship between Newton and Einstein's theories which are deductively disjoint in particular and successive universal theories more generally. It also seems clear that Feyerabend did not think that Popper could defend himself from his criticism with the reply that falsificationism is only supposed to suggest a normative rule of thumb. Popper's account of basic statements used to test theories is supposed to make meaning variance irrelevant to theory testing. For Popper, the meanings can vary, as long as there is agreement on basic statements, there can be a crucial experiment by falsification. However, directly after arguing that Popper's methodology cannot take comparing incommensurable theories into account, Feyerabend also argues that agreement of behavior about basic statements cannot guarantee agreement of content of basic sentences, which is what should be required to establish formal relations for a falsification. [274]

Put simply, Feyerabend argues that when the same measurement results are interpreted by incommensurable rivals, agreement is about how to use basic sentences, not about the content of basic statements, and without agreement on basic statements (about the content of the sentences) there can be no formal relations between them. With respect to the defense of Popper's methodology with the rebuttal that it is only supposed to be a rule of thumb, note that Feyerabend is clearly arguing that falsificationism *cannot* work for testing *any* incommensurable theories empirically, not just that it did not work that way in some specific cases. Moreover, if falsificationism is just supposed to be *a* rule of thumb for justifying

[274] See Feyerabend, ERE (1962), pp. 93–95.

replacing one theory for another, and not *the* rule of thumb, then where are the other rules of thumb for justify replacing an established theory based on empirical evidence in Popper's account? Given Popper's conception of empirical content, there does not seem to be any room for additional rules of thumb (such as he one implied by Feyerabend's test-model). [275]

We will return to these two points in the next section, to see more clearly why Feyerabend does not think that the corresponding (from the same measurement procedure) predictions of universal theories *can* enter formal relations, and to see why defending Popper's methodology by claiming it is only supposed to be a rule of thumb is, from Feyerabend's perspective, bedside his point.

To sum up this section, in ERE (1962), Feyerabend clearly argues that falsificationism requires establishing *formal relations* between predictions deduced from incommensurable (deductively disjoint) rivals for a falsification of one theory by the other that do not exist when there is meaning variance that renders them deductively disjoint or "incommensurable". Popper's account purportedly fails because it relies on formal relations that do not exist due to meaning variance in the terms used to state rival universal theories that determine the meanings of the terms in the statements deduced from them and used to test them. Falsificationism cannot take meaning variance into account. As we shall see in part 2, the fact that ERE (1962) was explicitly intended as a *criticism* of Popper's falsificationism in 1962 does not fit into the widespread (Popperian) account of Feyerabend as developing and extending Popper's falsificationism until 1967. Feyerabend continues to be misrepresented as promoting falsificationism in the period from 1960–1967 when in fact he was try to criticize it as an anti-progressive methodology that shares the same dogmatic feature that turns school philosophies into ideologies, stultifying the very progress they purport try to promote. Put simply, according to Feyerabend, falsificationism cannot work for testing rival universal theories because meaning variance in the terms used to state them renders them deductively disjoint, and so recommending trying to falsify them, when in fact new proposals can and should be allowed to be deductively disjoint from them, stultifies progress by undermining any potential legitimacy for justifying replacing an established universal theory with an incommensurable proposal. Falsifications are not how new worldviews are born.

[275] As we shall see in the next section, Feyerabend's test-model relies on an incompatible conception of empirical content with respect to Popper's methodology.

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1.9 The Implications: On the Limited Validity of Falsificationism

In this section, we will consider some of the major implications of Feyerabend's criticism of Popper's methodology. We will start with the main implications it has for falsificationism itself. Then we will consider its implications for Popper's account of basic statements. This will allow us to pinpoint why Feyerabend seems to think that Popper's account glosses over the problem of explanation under investigation. Then we will see how the implications Feyerabend's criticism of Popper's methodology has with respect to Feyerabend views on empirical content and scientific progress in contrast to Popper's views on those issues. Finally, we will see that the problem with Popper's methodology according to ERE (1962) is its implications for explanation, and why Feyerabend thinks that Popper's methodology misleadingly implies that new universal theories explain the facts that were explained by antiquated universal theories. This shows how Feyerabend turns a problem with explanation in Popper's account into a normative criticism of Popper's methodology that has implications with respect to the relativism of truth and fact to framework.

The Limited Validity of Falsificationism

As we have seen, Feyerabend *explicitly* criticizes Popper's methodology in ERE (1962). In so doing, he implies that it has only a limited validity. As a model for the justification of when to replace one theory by another, it is limited to comparing commensurable theories empirically, because it cannot serve as a model for the justification for a crucial experiment between universal theories due to of meaning variance:

> It seems to me that this theory [Popper's falsificationism] *can well deal with* all the problems that arise when *T* and *T'* are *commensurable*, but [logically] inconsistent inside *D'*. It does *not* seem to me that it [falsificationism] *can deal with* the case where *T'* and *T* are *incommensurable*. [276]

Feyerabend is claiming that while falsificationism works for ("can deal with") testing lower level-laws and theories with universal theories that are commensu-

[276] ERE (1962), p. 93 [emphasis added].

rable with them (for example, testing Kepler's laws and Galileo's theory, which were falsified by classical mechanics), falsificationism *cannot* work for ("cannot deal with") cases of testing incommensurable universal rival theories (such as classical mechanics and relativity theory), as Feyerabend sees it. In other words, the main implication of Feyerabend's criticism of falsification is that it has only a limited validity. In can only work for comparing commensurable theories empirically. It cannot work for comparing incommensurable theories empirically. A falsification cannot be the ideal justification for replacing one universal theory by another in scientific revolutions.

As we have seen, Feyerabend explicitly accepts that classical mechanics falsified Galileo's theory and Kepler's laws by successfully predicting corroborated statements that logically contradict statements deduced from Galileo's theory and Kepler's laws, while also correctly predicting all of their correct predictions. Feyerabend's test-model *implies* that the corroborated predictions of relativity theory that diverge quantitatively from predictions deduced from classical mechanics (for example, at high velocities relative to the speed of light) do not falsify classical mechanics in the same way that predictions deduced from classical mechanics contradict predictions deduced from Galileo's theory of freefall and Kepler's laws falsifying them. In these cases, the theory doing the falsifying (classical mechanics) does not use meanings that vary with respect to any of the terms that are used to state or test Galileo's theory of freefall and Kepler's laws, and so the universal theory could be used to explain their successful predictions (if it is assumed to be true, given the orthodox account of explanation). The statements used to corroborate both can be and are logically consistent (making explanation of the existing theory's successful predictions by the new theory possible), while other statements deduced from both theories can and do logically contradict (making falsification of one theory by the other's predictions possible). In this case, the new universal theory (classical mechanics) is commensurable with the existing laws and theory (Kepler's laws and Galileo's theory of free-fall), and so there is no meaning variance rendering them deductively disjoint, and no problem with respect to explanation during such this episode of scientific progress.

However, when the theories are incommensurable rivals, according to Feyerabend, because of meaning variance they are deductively disjoint, so that neither of those two things are possible. The successful predictions that had been deduced from the existing theory in the domain they appear to overlap or even be

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identical with predictions deduced from its rival cannot simply be taken as evidence for the new theory. For that, first they must be reconceptualized according to the new theory.

By contrast, for Popper the evidence corroborating Kepler's laws also corroborates classical mechanics, just as the evidence corroborating classical mechanics (at comparatively low velocities with respect to the speed of light) corroborates relativity theory. However, according to Feyerabend, in the latter case, as opposed to the former, because the sentences expressing that evidence are given new incompatible meanings by the incommensurable rival used to interpret them, they are reinterpreted into different statements. This is how Feyerabend's test-model limits the validity of falsification to comparing commensurable theories empirically. Feyerabend's model tries to show why and where Popper's model breaks down. It breaks down because of meaning variance, even in the meaning of the terms in basic statements and background knowledge, when they are used to test incommensurable rival universal theories empirically, because to be used to do so, they must be deduced from the theory they are used to test. Because of meaning variance, there can be no formal relations between their predictions. That is how incommensurability limits the validity of falsificationism.

The Implications for Popper's Account of Basic Statements

Feyerabend's criticism of Popper's methodology has negative implications for Popper's account of basic statements. These implications are not always clearly stated in ERE (1962), but there are several significant hints to this effect, and by putting them together, the picture becomes clearer. Feyerabend implies that the tension between empiricism and explanation is glossed over in Popper's account of "basic statements", which is why it needs to be disinfected of its empirical core. Feyerabend's complaint boils down to the accusation that in Popper's methodology, basic statements are based on agreements of behavior (decisions) as if this established agreement with respect to their content (given that agreement of content is needed to establish formal relations between them), when in fact they must have different contents when used to test rival incommensurable theories because they must be deduced from them to be used to test them.

Feyerabend seems to think that Popper's "Basissätze" should be translated as "basic sentences", not "basic statements", because at its empirical core, that is all

that is agreed upon in Popper's methodology, agreement about how to use basic sentences, and not agreement about their content their content, which falsificationism requires, which implies that empirical content is cumulative.

Feyerabend praises Popper and Carnap for adopting causal, pragmatic accounts of observation sentences according to which observation sentences are distinguished from other sentences not by the cause of their production (neither by experience nor the world), but by the fact that their production conforms to certain behavioral patterns. These views are presented as a form of pragmatism that introduces a "subjective" element that requires us to relinquish a formal account of explanation through revolutions: [277]

> It seems to me that the changes that are necessary will make it impossible to retain a formal theory of explanation, because these changes will introduce pragmatic or "subjective" considerations into the theory of explanation. [278]

As we have seen, Feyerabend's remark in footnote 18 was a harbinger of things to come, as it prepares his criticism of Popper's model. In that footnote, Feyerabend claims that, according to Popper's methodology:

> In the case of classical physics, therefore, [according to Popper] every basic statement must either be itself a statement about relative positions of physical bodies [...] or it must be equivalent to some basic statement of this "mechanistic" [...] kind. [279]

This is what Feyerabend is rejecting when he targets Popper's account, and the reason he rejects it is basically the same as why he criticizes Bohr for suggesting that classical statements will forever always remain irreplaceable (due to complementarity and the correspondence principle). Feyerabend's criticism of Popper's methodology challenges the implication that such mechanistic statements about relative positions of physical bodies are forever irreplaceable. For Popper a basic statement is any statement that can serve as a premise in an empirical falsification, which takes the singular existential form "There is an *X* at *Y*" so that basic statements can act as potential falsifiers that can formally *contradict*

[277] For a contemporary Kuhnian form of pragmatism, see Hasok Chang, **Realism for Realistic People: A New Pragmatist Philosophy of Science**, Cambridge University Press, Cambridge 2022.

[278] Feyerabend, ERE (1962), p. 92.

[279] Feyerabend, ERE (1962), pp. 35–36.

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universal statements as illustrated by the example: [280] In space-time region *k* "there is an apparatus which is a perpetual motion machine", [281] which contradicts the law of the conservation of energy, and would, if true, falsify it. Notice that Popper's example of a basic statement is extremely indicative with respect to Feyerabend's counter-example to falsificationism in ERE (1962), as this is exactly how Brownian motion appears before Einstein's predictions, which resulted in the need to limited the validity of the second law of thermodynamics. In other words, Popper alludes to the Brownian motion example to illustrate how basic statements should work.

The problem with Popper's account, according to Feyerabend's argument is that according to Popper, the potential explanatory success of a new proposal should match and exceed the potential explanatory success of an established theory by potentially explaining all its correct predictions and in addition some further predictions. If this is the case, then the established theory should be replaced by the new proposal, because it has *more empirical content,* because it makes *more* correct *predictions,* in a falsification, there can be no "Kuhn-loss". Comparing theories empirically is based on a quantitative comparison of the respective sizes of the sets of successful predictions that can be deduced from them. [282] Feyerabend implies that this puts an undue and unjustifiable restraint on the free advance of science, which may need to revise our understanding of such "basic statements", and the roles they play, or even how to test theories empirically without them, allowing for the possibility of science without experience.

To see this more clearly, consider how Feyerabend had hinted earlier, that he was going to go challenge Popper's basic statements for exactly this reason:

> Observational statements proper are such qualitative statements as "pointer *A* coincides with mark *B*", or "*A* is greater than *B*"— and these statements will not change, or

[280] As we shall see, Bohm was suggesting a shift from Cartesian to topological understanding of basic concepts, illustrating Feyerabend's point.

[281] Popper, **The Logic of Scientific Discovery...,** p. 48. For an overview of the role of basic statements in Popper's account, see Stephen Thornton, "Karl Popper", in: Edward N. Zalta (ed.), **The Stanford Encyclopedia of Philosophy** 2023, https://tiny.pl/q4m6tss7.

[282] See Popper, **The Logic of Scientific Discovery...**

> be eliminated, whatever the development of the theory, or of the methods of measurement. [283]

Feyerabend does not explicitly ascribe this view to Popper, but he implies that this is a consequence of Popper's account of basic statements by comparing it to Bohr's Bohr's complementarity which he often criticized for implying the irreplaceability of classical concepts. Without a test-model that can take meaning variance in basic statements into account, testing basic statements becomes a problem for Popper's methodology. Basic statements are always taken to be better tested than the theories they are used to test unless a new theory replaces it by providing a revolutionary new test that preclude formal relations because of meaning variance. Feyerabend is suggesting that for that reason, in progress by falsificationism, basic statements tend to become irrefutable sentences that lose their content.

As we shall see, this turns out to be a bitter irony, as Popper tries to criticize Feyerabend (and perhaps Bohm) for promoting the myth of the untestable framework, when in fact, according to Feyerabend it is a pitfall of Popper's own account that framework theories cannot be tested, because he has only one test-model that cannot take them into account. By contrast, Feyerabend's criticism of falsificationism implies that rival basic statements are expressed by the same sentence. Even the way Feyerabend presents Popper's idea of basic statements makes it clear that he does not share Popper's views about it. In the footnote attached to this quote mentioning "basic statements", [284] Feyerabend remarks: "*Popper himself does not restrict his characterization to the observation statements of classical physics*". [285] In other words, Feyerabend is implying that in Popper's methodology, "mechanistic" basic statements are supposed to play an *irreplaceable* role in *all* empirical sciences, which Feyerabend is clearly. On the interpretation of the essay suggested, Feyerabend very clearly wants to distance himself from Popper by representing Popper's views in this way.

Feyerabend is implying that the empirical core of Popper's account, his "basic statements", is supposed to supply a single, somehow content neutral observation

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[283] Feyerabend, ERE (1962), p. 51.

[284] Feyerabend is citing Popper, **The Logic of Scientific Discovery…,** p. 103.

[285] Feyerabend, ERE (1962), fn.18, p. 36 [emphasis added].

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language applicable for testing all theories empirically, regardless of meaning variance in the terms of the rival theories used to predict and explain them. After all, the idea of using basic statements to test rival theories according to formal relations between statements deduced from them was developed to make meaning irrelevant to testing theories empirically, whereas Feyerabend is clearly trying to criticize the implication of Popper's methodology that, "the meanings of (observational) terms are invariant with respect to [...] explanation". [286] By contrast, according to Feyerabend, meanings vary when the same terms are used to re-explain the same sentences with new revolutionary theories. As Feyerabend explains:

> if it is correct, as has been argued all the way through the present paper, that the meanings of observational terms depend on the theory on behalf of which the observations have been made, then the observational material referred [287] to in this modified sketch of explanation must be presented in terms of this theory also. [288]

The tension in Popper's methodology, according to Feyerabend, is clearly between the contextual theory of meaning, which results in meaning variance rendering universal theories deductively disjoint, and Popper's account of the role of explanation in a falsification according to formal relations. According to Feyerabend, using theories as explanations should have ontological implications: just as new universal theories are corroborated by reinterpretations of the facts, new universal theories also explain new facts that have new meanings, not the facts that the established theory was thought to have explained with the same sentences that were used to make different statements. Feyerabend claims any attempt to explain Popper's "explains" in the quote [289] cited by Feyerabend to illus-

[286] Feyerabend, ERE (1962), p. 43.

[287] Note, Feyerabend did not adopt Popper's term "basic statements" here distancing himself from Popper on basic statements (as he had already implicitly done, as we have just seen, in an earlier footnote that lumps Popper's view of basic statements in with Bohr's contention that classical concepts are irreplaceable, which Feyerabend also criticized for a similar reason. On this reading, Feyerabend is signaling that he disagrees with Popper about basic statements, and Popper's whole strategy for dealing with meaning variance by introducing "basic statements", which to play their role, must either logically be consistent with or logically contradict other basic statements to be used as "empirical evidence" to test them, which is exactly what Feyerabend has been arguing against all along.

[288] Feyerabend, ERE (1962), p. 94.

[289] See Feyerabend, ERE (1962), pp. 92–93.

trate Popper's methodology in ERE (1962) must be interpreted *informally*, or it will imply, according to the orthodox account of explanation that new theories explain the antiquated observation statements that express antiquated facts, which follows from erroneously assuming that there can be formal relations between the corresponding predictions of incommensurable theories based on basic statements. Feyerabend's ultimate conclusion in the very last sentence of ERE (1962), makes this very clear: "We have to conclude, therefore, that a formal and »objective« account of explanation cannot be given". [290] He means that a formal account of the explanation of the facts once explained by a former universal theory cannot be given by the contemporary theory that replaced it. It is impossible to give a formal account because meaning variance in the terms used to state the rival theories renders them deductively disjoint so that their corresponding predictions cannot enter formal relations. The new theory should be used to reinterpret everyday observation sentences and basic sentences into new statements, and not to formally explain former observation statements or the purported facts they stated.

Feyerabend's criticism of Popper's methodology implies that, in the case of incommensurable rivals, basic statements are supposed to function as agreed upon neutral arbiters, without really being neutral arbiters (covertly they have different meanings), nor can they even function as neutral arbiters for the purpose of formal relations as they should according to falsificationism because meanings should not be invariant, even of the basic statements used to test universal theories because of meaning variance in the terms used to explain what those basic sentences mean according to the theory from which they are deduced and used to test. In the closing page of ERE (1962), Feyerabend explicitly rejects this kind of neutral, or non-theoretical language approach, when he explains the reason that the criterion of predictive success cannot be completely objective:

> What would such an objective criterion be? It would be a criterion which is either based upon behavior that is not connected with any theoretical element — and this is impossible (cf. my criticism of the theory of sense data above) — or it would be behavior that is tied up with an irrefutable and firmly established theory-which is equally impossible. [291]

[290] Feyerabend, ERE (1962), p. 95.

[291] Feyerabend, ERE (1962), p. 95.

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Feyerabend's criticism of Popper's methodology implies that Popper's account glosses over the problem meaning invariance poses for falsificationism by not recommending deducibility of content as a condition for testing the theories against each other, which implies that agreement between uninterpreted sentences suffices for the new theory to informally "explain" the successful predictions of the established theory. [292]

Feyerabend was well aware that according to Popper:

> Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement or other which we *decide to accept*. If we do not come to any decision, and do not accept some basic statement or other, then the test will have led nowhere. [293]

According to Feyerabend, Popper correctly acknowledges where the empirical core of science lies (in decisions about agreement of behavior and not content), but in so doing Popper undermines his own account of the role basic statements are supposed to play in his test-model as applied to incommensurable universal theories because formal relations are not possible due to meaning variance in the terms used to state them.

It seems likely that Popper did not agree with Feyerabend's emphasis on the subjective implications of Popper's methodology with respect to testing, explanation and truth, as Popper was moving in exactly the opposite direction at exactly that time. In **Conjectures and Refutations** (1963), Popper integrates the concepts of truth and content into "truthlikeness" or "verisimilitude", according to which one theory can be said to be more truthlike or to have more "verisimilitude" than its rival, [294] which he continues to explicate according to the size of the set of the testable consequences that can be deduced from them. Popper had always emphasized his commitment to objective knowledge long before his 1972 book. [295] By contrast, Feyerabend was arguing for an irreducible subjective ele-

[292] Feyerabend, ERE (1962), pp. 94–95.

[293] Popper, **The Logic of Scientific Discovery...**, p. 86 [emphasis in the original].

[294] For Popper's account of verisimilitude, see Popper, **Conjectures and Refutations...**; for an overview, see Thornton, "Karl Popper...".

[295] See Karl R. Popper, **Objective Knowledge. An Evolutionary Approach**, Oxford University Press, Oxford UK, New York 1972. On Feyerabend and Popper's changing attitude toward truth in

ment in criterion of empirical success. Feyerabend's conclusion in ERE (1962) implies that issues of truth and explanation should be left completely out of the picture with respect to characterizing scientific progress through scientific revolutions, because truth and explanation are relative to the frameworks to which they belong, just as Popper was trying to bring truth into the picture. As we shall see, there can be little doubt that Popper's trend toward accepting truth contributed to Feyerabend's view of the extent to which he disagreed with respect to Popper's views on methodology as he struggled to clarify his own views between 1960 and his "awakening" in December 1967. From the 1960 draft of ERE, they diverge in opposite directions with respect to truth and objectivity, until Feyerabend suddenly realized just how much his own methodological views differ from Popper's when he finally awoke from his Popperian slumber.

It is important to emphasize that Popper explicitly allows basic statements to be tested and replaced, [296] and that for Popper, the empirical core is never definitively secure. Testing is ultimately based on provisional decisions not to question them further (for the moment), which are necessary for testing rival theories. This is where, according to Feyerabend, the tension lies, agreement not to question the meaning of sentences does not establish that it has the same content. Popper's infamous swampy metaphor is illuminating. According to Popper, theories are like buildings held up by piles driven deeper and deeper into a swampy ground:

> The piles are driven down from above into the swamp, but not down to any natural or "given" base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being. [297]

Feyerabend is suggesting that we stop pretending that the buildings are standing on piles fixed in one location, instead of emphasizing that they are free floating, and it is rational to choose whatever boat best floats no matter where it may take you, before Feyerabend decided it is better to choose to believe whatever best floats your boat.

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this context, see especially Luca Tambolo, "A Tale of Three Theories: Feyerabend and Popper on Progress and the Aim of Science", *Studies in History and Philosophy of Science. Part A* 2015, Vol. 51, pp. 33–41.

[296] Popper, **The Logic of Scientific Discovery...**, p. 86.

[297] See Popper, **The Logic of Scientific Discovery...**, p. 94.

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Empirical Content and Progress in Science

For Feyerabend in ERE (1962), the relevant question should be:

> whether newly invented theories *should b*e consistent with, or contain, those of their predecessors with whom they overlap in empirical content. [298]

According to Feyerabend, Hempel and Nagel explicitly state, or at least imply, that they should. The long quote illustrating the transitivity of evidence in Popper's methodology in ERE (1962) is intended to show that Popper's methodology also recommends that ideally they should. Falsificationism provides an account of how the new theory can explain the successful predictions deduced from the established theory. Feyerabend does not think that this is the case when rival universal theories are incommensurable. Feyerabend is trying to say that their corresponding predictions should not have to be logically consistent even in the domain in which they overlap (that is even when they result from one and the same measurement procedure), but Feyerabend needs to introduce a new account of empirical content to make sense of his idea, which as far as I can see, is incompatible with aspects of Popper's account of empirical content. For this reason, Feyerabend's claims about incommensurable theories increasing each other's empirical content can seem very strange from Popper's perspective, as for Popper, a theory's empirical content is determined by the statements that can be deduced from it and thereby used to test it, while for Feyerabend, incommensurable theories have no such formally comparable consequence. [299] If they have no such formally comparable consequences, then how can they share any empirical content, let alone increase each other's empirical content?

Feyerabend is trying to make several points with respect to empiricism and empirical content. First, introducing an incommensurable rival increases its empirical content by making otherwise unavailable facts available for testing established theories. That is what the Brownian motion is supposed to illustrate as it functions in Feyerabend's methodological argument for theoretical pluralism. Moreover, these new facts are used to undermine the ontology implied by a realistic interpretation of the established theory, not by establishing formal relations

[298] Feyerabend, ERE (1962), p. 47.

[299] For an overview of Popper's views on empirical content, see Thornton, "Karl Popper…".

between their corresponding predictions and showing the new proposal can explain everything correctly predicted plus more, but because there is a better corroborated rival (because it explain a novel phenomenon) that is based on terms that have inconsistent rules of usage with respect to the ontology they imply, which is what makes incommensurable rivals incompatible, while potentially re-explaining everything. The established theory should be replaced because the concepts used to state it have zero extension according to the new proposal, not because it makes quantitatively inconsistent predictions, but because its ontology does not have the resource to explain the novel phenomenon. As we are about to see, in this way, Feyerabends conception of empirical content in ERE (1962) is both very Popperian and very un-Popperian at the same time. It is very Popperian in that empirical content increases when testability increases. Feyerabend's conception of empirical content is clearly very un-Popperian because "it does not restrict empirical content to the consequences that can be deduced from the theory (the backbone of, Popper's methodology). Instead Feyerabend claims that incommensurable theories and their successful predictions are part of the empirical content of their deductively disjoint rivals. To see how, recall that Popper defines empirical content according to the predictions that can be deduced from it so that empirical content of a theory increases with its universality and precision as it becomes better corroborated. The empirical content of a theory is determined by what experience the theory forbids (the experiences that would count as a refutation if they can be predicted) so that the more possible experiences a theory forbids, the greater its empirical content. For Feyerabend in ERE (1962), this is also how incommensurable rival theories are part of the empirical content of their rivals. They are part of the experiences the new theory forbids interpreted realistically according to the new theory. Understanding what a theory forbids does not just require understanding what experiences are formally inconsistent with predictions deduced from it, but also what experiences it forbids based on other possible interpretations of those same experiences as deduced from incommensurable rivals. The empirical content of an established theory can be increased by an incommensurable rival because the new proposal can make new evidence available that allows the ontology of the established theory to be tested. This is why theories are better understood in contrast to each other. I contrast to each other they have more empirical content than by themselves, because experiencing an antiquated worldview realistically is exactly part of the set of experiences that is ruled out by the new theory on Feyerabend's account.

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These remarkable differences in Feyerabend and Popper's accounts of empirical content are yet another clear indication that Feyerabend no longer subscribes to Popper's views on method in ERE (1962), as Popper's conception of empirical content is the backbone of falsificationism. Put another way, Feyerabend's test-model requires a revision to the very backbone of Popper's account of empiricism. Feyerabend implies that Popper's account of empirical content cannot take testing universal theories into account. Popper *formally* defined his notion of empirical content in **The Logik**, which is very clear, but also very limited, from Feyerabend's perspective. [300] Popper's account of comparing empirical content is limited because it relies solely on logical relations (consistency and contradiction) between statements deduced from the competing theories to compare them empirically. There is no way for conceptually incompatible frameworks which are deductively disjoint to contribute to each other's empirical content.

For Popper, the basic idea is that the empirical content of a theory is not the statements it predicts correctly (verificationism), but all the potential falsifiers (experiences that contradict statements deduced from the theory) that it correctly rules out. In ERE (1962), Feyerabend sketches a different kind of account of empirical content in contrast to Popper's account, because Feyerband's account of empirical content should support his test-model in contrast to Popper's test-model. According to Feyerabend in ERE (1962), the use of a rival incommensurable theories,

> will also improve our understanding of each [...] by making it clear what is denied by the theory accepted in the end. Thus, it seems to me [Feyerabend] that our understanding of Newton's somewhat obscure notion of absolute space and of its merits greatly improved when we compare it with the relational ideas of Berkeley, Huygens, Leibnitz, and Mach, and when we consider the failure of these latter ideas to give a satisfactory account of the phenomenon of inertial forces. Also the study of general relativity will lead to a deeper understanding of this notion than could be obtained from a study of the *Principia* alone. [301]

In some, with respect to empirical content, comparing and contrasting how deductively disjoint universal theories solve problems leads to a better under-

[300] For specific references, see Thornton, "Karl Popper...".

[301] Feyerabend, ERE (1962), p. 67 [emphasis added].

standing of all of them. Feyerabend is suggesting that deductively disjoint theories are part of each other's empirical content. Feyerabend's reasoning on this point is very Popperian, as it draws on the same intuition as Popper's account of empirical content, but Feyerabend's suggestion is bewildering from Popper's perspective because empirical content is defined by deductibility and for Feyerabend deducibility by Popper disjoint theories and their successful predictions are part of each other's empirical content. This is not the only clear indication that Feyerabend is no longer working within the framework of Popper's methodology in ERE (1962). The consequences that Feyerabend's test-model have for how science should be seen to be correctly described (to serve as a normative guide) specifically with respect to characterizing scientific progress through universal theory replacement (scientific revolutions) also clearly contributed to driving a wedge between the account of progress that results from Popper's logic of scientific justification and Feyerabend's attempt to disinfect empiricism. Fortunately, that particularly long and complicated story has already been very well told. [302]

As for Feyerabend's conception of scientific progress, Feyerabend's test-model in ERE (1962) implies that progress should be best characterized as an ever-increasing ocean of alternatives forcing each other into greater articulation and should not be characterized as better approximation to the truth. This idea of progress remains basically the same from the implications of ERE (1962) "described as "an ever-expanding ocean of alternatives" in 1965, through to **Against Method**:

> A scientist who wishes to maximize the empirical content of the views he holds and who wants to understand them as clearly as he possibly can must therefore introduce other views, that is, he must adopt a pluralistic methodology. He must compare ideas with other ideas rather than with "experience" and he must try to improve rather than discard the views that have failed in the competition [. . .] Knowledge so conceived is not a series of self-consistent theories that converges towards an ideal view; it is not a gradual approach to the truth. It is rather an ever-increasing ocean of mutually incompatible (and perhaps even incommensurable alternatives), each single theory each fairy tale, each myth that is part of the collection forcing the others into greater articulation and all of them contributing, via this process of competition, to the development of our consciousness. [303]

[302] See Tambolo's excellent, "A Tale of Three Theories…".

[303] Feyerabend, **Against Method…** (1975), p. 30. Compare Feyerabend, "Problems of Empiricism…", pp. 224–225.

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In this way, from ERE (1962) on, Feyerabend and Popper parted ways with respect to truth and progress. Feyerabend's notion of truth relativizes truth to the *a priori* principles that construct that framework so that there is no truth simpliciter, and so it does not make sense to say that progress better approximates truth. Truth is always truth according to some theory, and it should always be truth according to some theory *in contrast to* truth according to its rivals, and never just truth *simplicter*. Popper tries to show that the growth of knowledge is objective given that empirical content increases as more and more possible observations statements are ruled out while more and more experiences are correctly predicted.

In ERE (1962), Feyerabend argues that universal theories are not falsifiable, because they are deductively disjoint. That is why falsificationism cannot take testing deductively disjoint universal theories empirically into account. But Feyerabend is still an empiricist. In his early philosophy of science, he is trying to show "how to be a good empiricist" [304] by understanding science as "an attempt at a realistic interpretation of experience". [305] According to Feyerabend's disinfected empiricism, universal theories are not falsifiable, and methodology should not recommend trying to falsify them. It should recommend showing that the existing theory is refuted indirectly without a falsification, by showing that a novel phenomenon that could *never* have been discovered by established theory by itself, that in fact refutes it, just because the existing theory makes the wrong ontological assumptions, which can be replaced by changing the rules of usage of the terms used to state it. If the new theory can show the existing theory implies the wrong ontological assumptions by corroborating a novel phenomenon in this way, then because ontological assumptions are universal principles, actually shows that the theory is wrong about much more than just this "corroborator". It is wrong about everything, which it never could have found out, without a deductively disjoint rival that shows why it was wrong about the corroborator (and everything else). This is exactly how Feyerabend's novel corroboration test-model for universal theories differs from Popper's falsificationism for commensurable theories.

For Feyerabend, universal theories are not falsifiable, but they are still empirically comparable by crucial experiments that are decisive, at least temporarily

[304] Title, see Feyerabend, "How to Be a Good Empiricist...".

[305] Title, see Feyerabend, "An Attempt at a Realistic Interpretation...".

though not definitively once and for all, by crucial experiment based on novel corroborations as set out by Feyerabend's test-model. In this way, in contrast to Popper, Feyerabend promoted a theoretical pluralism that was both revisionary (common sense should be revised and corrected according to the new theory and its worldview which should replace its rivals) and still retentive (universal theories are superseded and should be replaced as realistic interpretations, but they should still inform current views, and continue to be fruitful even after they have been superseded and replaced as realistic interpretations. Feyerabend makes this implication of his views very clear in his "In Defence of Classical Physics", which Feyerabend wrote the first draft of in 1967 for the fourth volume of the proceedings of the 1965 London Colloquium as a critical reply to Popper's inaugural lecture "Rationality and the Search for Invariants". [306] Feyerabend's test-model implies classical physics has never been falsified, and just because it has been superseded by a deductively disjoint rival, does not imply that all of the sudden it will cease to function as a fruitful tool for discovering new phenomena and understanding new relations.

This difference between Feyerabend and Popper with respect to progress is like the difference between Popper's "we propose and nature disposes" and Feyerabend's (following Einstein and Bohm) "we propose, but we should never really dispose of what was once entirely successful within some specifiable domain or on some level". Not just because it may make a comeback, but also because it still informs our best current theory through contrast as part of the set of experiences the new theory rules out, but also because it can lead to tests of its rival that otherwise would be unavailable. In these ways, Feyerabend's account is supposed to be very different from Popper's which is later compared to the gong show and would seem to imply a succession of discarded theories, not an ever-expanding ocean of them. The established theory and its worldview should become part of the empirical content of the new theory in contrast to it, not by being explained by it. Established universal theories are not falsified. They are out-corroborated. Feyerabend's account is still revisionary, because the current worldview should be replaced and everyday language should be corrected in the process of developing and establishing a better one, while remaining retentive, but not just in the Popperian sense that they are all just conjectures so it could always make a comeback, but also because it should be seen as part of the *content* of the new theory con-

[306] See Collodel, "Was Feyerabend a Popperian…", p. 25.

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trastively as part of the set of possible experience it rules out and because it may still lead to new discoveries.

Explanation and Popper's One and Only Normative Rule of Thumb

Popper's methodology is normative (and so it seems to protect itself from refutation). In ERE (1962) Feyerabend shows the limits of its validity. Feyerabend test-model complements Popper's test-model in the sense that the two cases each only work for one of only two possibilities. Theories can only be either commensurable and Popper's model should be recommended, or they are incommensurable, in which case Feyerabend's test-model should be recommended. As we shall see in part 3, in 1961 Bohm complaned that Popper's falsificationism should be complemented by an account of confirmation and truth, which it needs to presuppose for falsificationism to work, but does not give an adequate account of. Now let's turn to the normative nature of Feyerabend's criticism to try to show why a Popperian cannot simply defend themself from Feyerabend's criticism by claiming that falsification is just supposed to be a normative rule of thumb that was never supposed to work in every case.

A Popperian is sure to have many objections to this interpretation of Feyerabend's rendition of Popper because Popper was explicitly trying to develop a normative model of testing theories that never portended to be universally applicable, nor to be a formal model of explanation and Feyerabend knew it. For this reason, these objections continue, Feyerabend's argument is not intended to target Popper's methodolology. But such objections falters for any number of reasons. Most notably, because the problem of explanation Feyerabend identifies is explicitly supposed to be a problem in Popper's rendition of Popper's own account. [307] Moreover, Feyerabend sets up his main conclusion that a formal account of explanation cannot be given by asking:

> How, then, is the above account of explanation to be modified to cover the case of incommensurable theories also? [308]

[307] The account referred to is in the long quote of Popper explaining falsificationism and using "explain" to do so, which Feyerabend uses Watkins to try to clarify, before criticizing Popper's view because it cannot take testing rival incommensurable theories into account.

[308] Feyerabend, ERE (1962), p. 94, penultimate page.

Before answering his own question with his main conclusion, (the modification necessary is to abandon a formal account of explanation given that revolutionary progress involves deductively disjoint rivals), Feyerabend explicitly makes it clear, in a footnote, that the same problem in Popper's account with respect to the explanation of successful predictions of an incommensurable rival in the domain in which they overlap also arises in the case of crucial experiments between those theories in the domain in which they diverge, that is, with respect to the purported falsifiers. This is mentioned in the footnote attached directly to the question just cited:

> As Professor Feigl has pointed out to me, this difficulty also arises in the case or crucial experiments. [309]

The point Fiegl made is only made in a footnote. It is not Feyerabend's main point of contention with Popper's methodology, which is explicitly a problem with explanation in his account.

Popper's methodology is supposed to be a model of a crucial experiment that shows the best possible reason for concluding that why one theory is better justified than the other and so should replace it based of the available empirical evidence deducible from it. Here Feyerabend implies that, as Feigl had pointed out, if successful predictions deduced from an established theory cannot be formally related to the corresponding predictions deduced from an incommensurable rival theory and thereby explain them, due to meaning variance in the terms used to state the rival theories that renders them deductively disjoint, then the unsuccessful predictions deduced from the established theory cannot be contradicted by the successful predictions deduced from an incommensurable rival either. (Incommensurable theories and their respective predictions have no formal relations). This is why Feyerabend concludes that the new theory cannot be tested by a crucial experiment based on logical contradiction because there is meaning variance, not that universal theories cannot be tested by crucial experiments. [310] As we have seen, Feyerabend's test-model in ERE (1962) was designed to show how to test universal theories empirically despite meaning variance (as was Popper's), but in a different way, by giving up on Popper's account of the role formal relations play

[309] Feyerabend, ERE (1962), p. 94, fn. 115.

[310] As we have seen, Feyerabend had already indicated that Feigl had made this point in "An Attempt at a Realistic Interpretation…".

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and Popper's account of empirical content and the linear conception of progress that it implies. However, Feyerabend's main point is that the predictions deduced from an established theory cannot be formally related to an incommensurable rival, due to meaning variance rendering them deductively disjoint, which is why the new theory cannot be said to explain the successful predictions of the existing theory in a domain in which they only appear to overlap. He is developing a problem in *Popper's methodological views* that can be seen by the limitations of Popper's account of testing and Hempel's account of a more comprehensive explanation, both of which imply that new universal theories can explain antiquated observation statements. Feyerabend concludes that incommensurability implies that there needs to be a modification of the orthodox account of explanation for scientific revolutions. The modification required, according to Feyerabend, is simply to abandon the idea that the new theories should have to explain the successful predictions of their rivals. It is not to abandon the idea that there can be crucial experiments between rival universal theories.

Normatively speaking, according to Feyerabend, new incommensurable proposals *cannot* formally explain the successful predictions of their deductively disjoint rivals, therefore, to be justified as replacements for established theories, they should not have to. Feyerabend is arguing that by putting an overly restrictive condition on the justification of new universal theories, falsificationism stifles revolutionary advance, instead of supporting it by allowing universal theories to compete empirically based on indirect tests of their rival's ontology through a process of conjecture and novel corroboration. Feyerabend argues that falsificationism *cannot* work for testing any rival incommensurable universal theories (or any other incommensurable rivals), not that it does not work in one special case. Descriptively, Feyerabend's challenges Popper's account of the transition from classical mechanics to relativity theory. Normatively, he uses Brownian motion to illustrate how there can still be crucial experiments between universal and other incommensurable theories, despite the fact that they are deductively disjoint so that no falsification is possible. That is why the "rule of thumb" defense of Popper's methodology is beside Feyerabend's point.

According to Feyerabend:

> For the thinker who demands that a subject be judged "according to its own standards", such criticism is of course impossible; he will be strongly inclined to reject any interference and to "leave everything as it".

citing Wittgenstein as an example of this defeatist attitude in the footnote. [311] By contrast, Feyerabend's theoretical pluralism explicitly emphasizes (with typical Feyerabendian flair) that dispute, and not consensus, should be considered the healthy state of development:

> cessation of dispute is not to be regarded as a sign that now we have finally arrived at the truth, but rather as a sign of fatigue in those originally participating in the dispute (just as the more recent return to religious beliefs is a sign of fatigue and of despair in the capabilities of reason). [312]

Feyerabend's test model is illustrated by an example that supposed to corroborate a novel methodological account of a crucial experiment that limits the validity of falsificationism as part of a criticism of Popper's conception of testing, his conception of empirical content and his conception of progress. According to Feyerabend views, Popper's account seems to gloss over the problem with the role of explanation in his own account without addressing it. That is the gist of Feyerabend's immanent criticism of Popper, as can be seen in the following passage directly before Feyerabend turns explicitly to Popper's form of contemporary empiricism:

> Now it is clear that a theory which is satisfactory according to the criterion just pronounced [when the new theory is incommensurable with the established theory] will not be capable of functioning as an explanans in any explanation or reduction that satisfies the principles put forth by Hempel and Oppenheim or Nagel. Paradoxically speaking: explanations cannot use satisfactory theories as *explanantia*. And satisfactory theories cannot function as *explanantia* in Hempel-Oppenheim explanations. [313]

After showing the same problem applies to Popper's methodology as Hempel's, Feyerabend concludes, for this reason:

> it seems perhaps advisable to eliminate altogether considerations of explanation from the domain of scientific method and to concentrate upon those rules which enable us to compare two theories with respect to their formal character and their predictive success and which guarantee the constant modification of our theories in the direction of greater generality, coherence, and comprehensiveness. [314]

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[311] Feyerabend, ERE (1962), pp. 61–62, and fn. 72.

[312] Feyerabend, ERE (1962), p. 73.

[313] Feyerabend, ERE (1962), p. 92.

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Truth is notably not on this list. For Feyerabend, new universal theories have greater generality because they explain a novel phenomenon and rule out more potential experiences (including those that corroborate its rival). When one universal theory replaces another, one cannot be said to be closer to the truth, because truth is always relative to one of the frameworks, either the established one or the new proposal, and so cannot be used to characterize the relationship between them.

Ontological Relativism and The Non-Autonomy of Facts

For Feyerabend, a new universal theory should be allowed to be deductively-disjoint with the existing universal theory, otherwise the new proposal would always have to be merely a modification of the existing theory, and not a potential revolutionary replacement of it. Feyerabend emphasizes that in revolutions the established theory and its ontology are *replaced*, not absorbed.

> we must admit that it is impossible to relate the kinetic theory and the phenomenological theory in the manner described by Nagel, or to explain all the laws of the phenomenological theory in the manner demanded by Hempel and Oppenheim on the basis of the statistical theory. Again replacement rather than incorporation, or derivation (with the help, perhaps, of premises containing statistical as well as phenomenological concepts), is seen to be the process that characterizes the transition from a less general theory to a more general one. [315]

Feyerabend's account of Popper's falsificationism clearly implies that progress by falsificationism is *cumulative* with respect to correct predictions, and therefore it is cumulative with respect to the facts that it can be used to explain. Feyerabend cites Popper's claim that in a falsification, the successful predictions of the established theory corroborate the new theory, so that the evidence is transitive. The successful predictions of a falsified theory (or law) in the domain in which the rival theories overlap carry over and count as corroborative evidence for the new theory. [316] By contrast, according to Feyerabend, progress through revolution cannot be by falsification (due to meaning variance that renders them deductively disjoint), so that with respect to content, progress is not cumulative, its revision-

[314] Feyerabend, ERE (1962), p. 92.

[315] Feyerabend, ERE (1962), p. 78.

[316] Feyerabend, ERE (1962), p. 93.

ary. New incommensurable theories require redescribing the facts as new kinds of facts expressed with the same observation sentences. They replace the established theory and the existing facts that were erroneously believed to be explained by it with a new incompatible explanation of the new kinds of facts that replace the existing ones, but are still expressed with exactly the same sentences (for example, "The ball fell" implied that it was pushed by a force from within before it implied that the ball was pulled by an external force).

Feyerabend thinks that progress through revolutions, in contrast to progress by falsification, is not cumulative with respect to the facts explained. Established views are *replaced* with respect to their role in providing a realistic interpretation of experience. In a revolution, even the measurable (empirical) evidence in everyday life should be reinterpreted, changing what counts as the facts. The old facts are abandoned with new ones replacing them, as the new universal theory acts as new relativized *a priori* principles that provide a new framework for understanding and further investigating these new facts, while we, in our everyday language, continue to say “the sun rises” based on antiquated ontological principles from the pre-Copernican worldview. Statements of these new facts are deductively-disjoint with the statements of the old facts using the same sentences, that is, even when exactly the same sentences are still used to state these new facts.

In ERE (1962), Feyerabend’s view is that *revolutions revise what counts as the facts*, so that facts are relative to the frameworks with which they can be stated and do not exist independently of them. Some kinds of facts can only be stated and explained with the help of new frameworks, which is why incommensurable proposals should always be welcomed (as we shall see, even if they are a*d hoc,* like Bohm’s), because they could lead to new kinds of facts that show the ontology of the established theory needs to be replaced.

The new theory cannot be said to be somehow truer, or a better approximation to truth, which would imply that they have a better correspondence to independently available facts, because the facts are not independently available. The kinetic theory was needed to establish the fact that Brownian motion is stochastic movement caused by shaking atoms. This fact could not have been discovered without the invention of an alternative to the classical phenomenological theory that does not share its faulty energeticist ontology (because the stochastic motion is caused by" so it says atoms). Feyerabend endorses the idea that truth is relative

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to the *a priori* principles provided by universal framework theories. Feyerabend's views imply that truth reflects coherence of a statement within a universal framework, and not a unique corresponds to a set of fixed unchanging facts that exist independently of that framework. Experience of the world and the world experienced are "co-constituted" together. [317] The framework allows the facts to be stated, but there is no guarantee that all the facts relevant to testing that framework can be stated within it and every reason to be believe that very likely not all of them can.

In 1968, directly after he announced his break in December 1967 (see part 2), Feyerabend expanded on his criticism of Popper's methodology as anti-humanitarian in "Science, Freedom and the Good Life". [318] Here, for the first time in print, Feyerabend begins to doubt the positive humanitarian function of science itself, lamenting that it is failing to promote human happiness. He explicitly blames this on the disjointed character of intellectual life and finds Popper partially to blame for it, given his strict separation between science and non-science. Kuhn is also partially culpable for promoting dogmatism as an integral feature of normal science, instead of something diametric opposed to good science. After the 17th of December 1967, Feyerabend was no longer presenting himself as a Popperian. He was talking in the past tense about his Popperian past. Eventually, Feyerabend became seen as a chief critic of Popper. As we shall see, by 1967 in letters and by 1970 in print, Feyerabend explicitly generalized the main implication of his argument against falsificationism to the conclusion of his most infamous methodological argument. It is not just that falsificationism can only have a limited validity (limited to testing commensurable theories), all methodological rules *can* have only a limited validity, or in other words, the only universally applicable methodological rule is "Anything goes!".

Postscript. Wittgenstein, Ontology and Empiricism

There is one final implication of Feyerabend's criticism of Popper's methodology left to consider. Feyerabend's test-model has implications for Feyerabend's

[317] For this terminology as used to explain Kuhn's "Kant-on-wheels-realism", see Paul Hoyningen-Huene, **Reconstructing Scientific Revolutions. The Philosophy of Science of Thomas S. Kuhn**, University of Chicago Press, Chicago 1993.

[318] See Feyerabend, "Science, Freedom, and the Good Life..", pp. 127–135.

criticism of Wittgenstein's alleged quietism. [319] On first reading, there is a rather awkward sounding passage, where Feyerabend is responding to a potential objection that he cannot be serious about the potential dangers of conformism, when it comes to the use of words like "explanation":

> It may be that the word "explanation" sounds beautiful to some ears-but who would think it reasonable to start a war for, or against, its elimination? [320]

On the interpretation suggested by this essay, the awkwardness is lessened once you realize the context (see section 1.4 The framework) and the severity of his criticism of Popper's falsificationism. Feyerabend's argument implies that Popper's account of falsificationism is just another overly restrictive school philosophy or dogmatic ideology. In the context of Popper's infamous dispute with Wittgenstein, he is implying that it was caused exactly by the kind of genuinely philosophical conflation that Popper did not even seem to understand by objecting to Wittgenstein's views with examples of genuine problems that are philosophical.

The poker incident is an event that was interpreted in radically different ways by those who witnessed it first-hand (let alone all of those who read Popper's account, or other various first-hand accounts, or a book about them that tries to compare them), which should be one of the main lessons of the book **Wittgenstein's Poker**, which is about it. [321] Popper, for his part, claims to have felt genuinely threatened by what he interpreted as Wittgenstein's *violence* about a dispute about explanation and "genuinely philosophical" problems that ERE (1962) is explicitly and implicitly intended to dissolve by distinguishing between genuine philosophical problems and genuinely philosophical "foggy" problems (pro-Wittgenstein) whose dissolution can have a genuine role in scientific progress by becoming ontological problems that could lead to decisive tests between universal theories.

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[319] It seems worth noting that Feyerabend's early review of Wittgenstein's **Investigations** clearly contributed to establishing the "Myth of the Quietist Wittgenstein", see Moyal-Sharrock, "The Myth of the Quietist Wittgenstein...".

[320] Feyerabend, ERE (1962), p. 61.

[321] See Edmonds and Eidinow, **Wittgenstein's Poker...**

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Feyerabend explained how ontology starts anew after each revolution in physics in "Physics and Ontologie". [322] Ontology explicitly becomes part of the empirical content of a theory according to Feyerabend's test-model in ERE (1962). Taken all together, in ERE (1962), Feyerabend criticizes Wittgenstein's alleged quietism, while adopting his views on meaning and ontology, just as he criticizes Popper's falsificationism from within a critical rationalist framework. In this way, Feyerabend's synthesis of Wittgenstein on meaning with Popper on method tries to take the best from both, while criticizing the worst in each — a strategy Bohm would later try to use with respect to the conceptual apparatus of quantum theory and relativity theory in his search for a new conceptual order for physics based on topology.

Feyerabend criticizes Wittgenstein for suggesting that philosophy leaves everything as it is by showing the legitimate role illegitimate (pseudo-) disputes play in the spread of knowledge from universal theories to everyday language *and in the advance of science itself*. Feyerabend embraces the idea that there are genuinely philosophical problems caused by antiquated ontological principles in which a "whole cloud of philosophy [is] condensed into a drop of grammar". [323] Suns rise? These kinds of foggy problems can raise ontological issues that can inspire crucial experiments between universal theories that dissolve the dispute, by resolving the problem empirically (at least for the time being, that is, definitively for the moment, but not indefinitely so). For this, new incompatible rules of usage need to be developed for the same terms used to state the rival theories without losing the ability to predict the same observation sentences. In this way, Feyerabend's test-model gives dissolving pseudo-disagreements a role in promoting

[322] See Feyerabend, "Physik und Ontologie...". The account undercuts to some extent the suggestion that Popper may have been "lying" about feeling threatened at the time in a self-aggrandizing version of himself as the hero, with Wittgenstein his arch nemesis in philosophy, so rudely departing from the field in dejected defeat. Surely, Popper felt threatened by Wittgenstein both physically (and intellectually). Feyerabend in ERE (1962) would seem to suggest that Feyerabend was aware that Popper had felt threatened by Wittgenstein's violence in 1960, when it was written. So if it is a retrospective fable by Popper, that he felt threatened by Wittgenstein's violence, as the authors of **Wittgenstein's Poker** seem to suggest, than it is one Popper started telling at the latest before 1960, and not just in his 1976 autobiography reflections. See Karl R. Popper, **Unended Quest: An Intellectual Autobiography**, Routledge, London — New York 1976 for Popper's version of the story. Popper was definitely a self-aggrandizer, but he does not seem to have been a liar. He did however think that he somehow owned every idea he had ever written down, which is somehow *perpopperous*.

[323] Wittgenstein, **Philosophical Investigations...**, p. 222.

scientific advance in support of his view that correcting everyday language with our best theories would promote progress.

In other words, Wittgenstein's genuinely philosophical "foggy" problems can become part of science as part of "an attempt at a realistic interpretation of experience" within the framework provided by a universal theory. Incompatibility between ontological principles can be used to try to devise experiments to try to test them through the corroboration of novel phenomena. That is what Popper and Einstein were trying to do with their thought experiments. They were trying to find a way to make metaphysical speculations testable, and indeed these thought experiments led to empirical tests with astounding results. This is how clarifying philosophical fog can motivate ontological disputes, according to Feyerabend's view of "scientific philosophy" in the Viennese tradition. For Feyerabend, ontology is where metaphysical speculation goes to become empirical. Clarifying genuinely philosophical "foggy" problems becomes part of the process of scientific advance, just as part of science becomes dissolving pseudo-disputes by finding ways to settle metaphysical issue empirically, as for example, with a genuine corroboration of a novel phenomenon of the type Feyerabend envisions as illustrated by the Brownian motion example; that is, one that shows its rival is completely wrong about everything and could never have discovered this by itself.

In the same vein, Wittgenstein's genuinely philosophically "foggy" philosophy helped Feyerabend correct Popper on testing universal theories in ERE (1962), with his own purportedly universally applicable test-model for empirically testing all rival universal theories as a genuine improvement in methodology, in contrast to the falsificationism that it corrects by limiting its validity to comparing commensurable theories empirically. By making genuinely philosophical "foggy" problems part of his test-model as prelude to genuine corroborations of a universal theory by genuinely novel kinds of phenomena, Feyerabend is trying to explain how ontological principles can become testable, and why new proposals should be used to try to re-interpret all of experience, while showing how this inevitably results in disputes about new genuinely philosophically "foggy" problems, until some new Einstein comes along with a new crucial experiment that shows how to deduce a new kind of phenomenon from a new theory that uses the same terms with incompatible rules of usage so that the experiment dissolves the dispute by solving an empirical problem, which is exactly what has happened according to

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Feyerabend's test-model recommends happened and should happen in scientific revolutions.

Feyerabend was trying to incorporate the resolution of Wittgenstein's genuinely philosophically "foggy" problems into the process of scientific advance by allowing meanings to vary, while at the same time *refuting* Wittgenstein's contention that philosophy must leave everything as it is because it can be neither empirical nor offer explanations Philosophy can help *change* meanings. This is part of how Feyerabend tried to synthesize the best of Wittgenstein and Popper's ideas into Feyerabend's test-model and the idea of testing rival incommensurable universal theories that it is used to illustrate. From the vantage point of a better worldview, in retrospect, it should seem that everything that was accomplished was accomplished even though they did not understand what kind of world they lived in according to the new worldview.

Moreover, using concepts that turned out to have zero extension in foggy discussions about universal ontological principles purportedly implied by some universal theory (or not) can help to accomplish quite a lot, as it should seem like that is just what must happen in the lead up to crucial experiments that make genuinely revolutionary advances. Clarification of pseudo-disagreements and nonsense can play important roles in scientific progress. [324] Recognizing differences in the systems of classes used by incommensurable universal theories is an essential step in testing them, just as allowing for the possibility of subsequent changes to the system of classes itself that would render our contemporary worldview completely obsolete is exactly the risk one takes when playing the game of science, as Feyerabend's test-model of conjectures and *novel* "corroborations" tries to make clear. Switching back and forth between rival incommensurable theories as predictive devises, according to Feyerabend, is routine in science. Different theories with incompatible assumptions are used to solve different kinds of problems. Whatever works. However, from Feyerabend's perspective, merely recommending looking clearly and thereby getting on with it from right back from where you

[324] See for example Aristides Baltas, "Nonsense and Paradigm Change", in: Léna Soler, Howard Sankey, and Paul Hoyningen-Huene (eds.), **Rethinking Scientific Change and Theory Comparison: Stabilities, Ruptures, Incommensurabilities?**, *Boston Studies in the Philosophy of Science*, Vol. 255, Springer, Dordrecht 2008, pp. 49–83; and Eric Oberheim, "Understanding Nonsense, Wittgenstein and Conceptual Change", in: Léna Soler, Howard Sankey, Paul Hoyningen-Huene (eds.), **Rethinking Scientific Change and Theory Comparison: Stabilities, Ruptures, Incommensurabilities. Boston Studies in the Philosophy of Science. Vol. 255**, Springer, Dordrecht 2008, pp. 69–81.

started before the fog set in, while accepting the futility of trying never to get so trapped again, is not very helpful. It may help to improve the situation by developing better concepts. Feyerabend also tries to provide a new explanation of testing universal theories in the form of a novel test-model that is corroborated by the Brownian motion example.

Feyerabend's account can seem somehow just as scary as Wittgenstein's views can seem. Witgmestein seems to imply that because of our grammar, we are inevitably, perpetually, vulnerable to genuinely philosophical confusions and conundrums and there is nothing to be done about it, accept periodically to try to let the fly out of the bottle, and just accept that it may get trapped again. Yet in a way, Feyerabend's account is even creepier, because it raises the possibility that not just words (which are just short-hand statements for Feyerabend as we live in worlds of facts not things), but also entire sets of *identical sentences* as actually used around us all the time in the same ways, may be being interpreted to mean something different than we think, *and this goes completely unnoticed* in the fog of everyday language, where pseudo-agreement and pseudo-disagreement reign. What looks like agreement and disagreement about trivially true or false empirical statements are not genuine agreements or disagreements because the same sentences are *covertly* interpreted according to incompatible rules of usage as implied by the rival ontologies of incommensurable theories. It's as if when we use everyday language, we covertly agree not to be able to disagree.

Rivals could be just caught in a cloud of fog caused by incompatible rules of usage that give different and incompatible meanings to all of the sentences about which they purportedly agree and disagree, because exactly the same sentences actually make radically different statements (that break Kuhn's no overlap principle) without either party or anyone else ever noticing it. You could wake up and realize the person in the bed next to you at night for the last twenty years does not really agree with you about anything, because you just found out they believe in some universal ontological principle that you had not expected that they believe. In fact, that they believe these principles would seem incoherent to you: How could they possibly believe that? Feyerabend's account allows for this possibility but recommends that unending quarrels of philosophers about traditional philosophical pseudo problems should give way to new foundational questions in ontology given progress in science and methodology. After he awoke, he wondered, how could I have believed in that little known form of insanity. The creepy

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possibility makes you wonder, and it can be worth wondering, are the people I think I agree with really saying the same things when we say the same sentences. Do we really disagree, or are we making different assumptions about ontological principles and so that are statements belong to different worldviews? Simply appreciating this possibility makes one less dogmatic and is, perhaps, an important step in any "Bohm dialogue" aimed at raising everyone's consciousness of the interconnectedness of the everything they are part of.

Lastly, the best way to develop the tension between Wittgenstein's views on conceptual change and Feyerabend's views on conceptual change can be seen quite clearly by introducing a distinction between stipulative and evolutionary conceptual change. [325] In much of his *early* work, Feyerabend is trying to stipulate conceptual change in the sense that he is recommending what people should think by trying to explain why they should think it. We should interpret our best theories realistically and reinterpret everyday language accordingly. Wittgenstein, by contrast, shows that conceptual change (for a variety of reasons) cannot simply be stipulated. It is a slow evolutionary process full of mutual misunderstandings.

Part 2. The Consequences: Feyerabend's Awakening, Popperstein's Baby, and the Lessing Effect

As indicated earlier, the name "Popperstein's Baby" is intended as an allusion to both Frankenstein (as a scary hybrid that took on a life of its own) and "*R*osemary's Baby" (Polański 1968) a nightmarish psychological horror film. When Feyerabend used term "incommensurable" in ERE (1962) to try to criticize Popper's methodology as sharing a dogmatic element with school philosophies so that it unwittingly perniciously stultifies the progress it purports to promote because it cannot take meaning variance into account, a hybrid monster was conceived and Popperstein's baby was born. The confusion that resulted about what Feyerabend meant and what he was trying to do with the term "incommensurable" in ERE (1962) had dramatic consequences for Feyerabend's relation to Popper and his

[325] On the insightful suggestion of a distinction between stipulative and evolutionary conceptual change in Wittgenstein's work, James C. Klagge, "Wittgenstein, Science, and the Evolution of Concepts", in*:* Jonathan Beale and Ian James Kidd (eds.), **Wittgenstein and Scientism,** Routledge, London — New York 2017, pp. 193–208.

school. In this way, “Popperstein’s baby” is a catch all family resemblance term meant to represent a set of confusions surrounding Feyerabend’s views as set out in ERE (1962), which turned into the cause of Feyerabend’s living nightmare when he awoke in December 1967. The idea behind the choice of the term is to emphasize that Feyerabend was trying to synthesize Wittgenstein and Popper’s views resulting in the confusions that led eventually to him announcing his break with Popper’s school over a dispute with respect to his criticism of Popper’s methodology that turned Feyerabend’s life into a living nightmare. This all began with ERE when he used “incommensurable” to try to criticize Popper’s methodology. Feyerabend was clearly trying to criticize Popper’s methodology with the idea of incommensurability and the test-model he tried to illustrate with Brownian motion, both of which were developed dialectically (in the sense of complementary opposition), that is, in contrast to Popper’s methodology in ERE (1962). He was not merely attempting to repeat Popper’s methodology as Feyerabend understood Popper to have implied in Popper's initial belatedly published reaction to it. Nor was he merely trying to extend Popper’s methodology by merely adding a complementary test-model to falsificationism. [326] The test model he proposes is dialectically related to Popper’s methodology, limiting its validity by rejecting an implication of one of its basic “assumptions” (that the empirical content of a theory is the set of observation *sentences* that can be predicted from it). Feyerabend's view also involves incompatible rules of usage for terms like “empirical content” and “indirect refutation”. Feyerabend and Popper never seem to have come to terms with respect to Feyerabend’s use of “incommensurable” before his awakening in 1967, nor after. And as we shall see, in December 1967, Feyerabend seems almost desperate for feedback from Popperians with respect to his criticism of Popper’s methodology, and for a chance to discuss his criticism of Popper’s methodology with them.

Appreciating that and how Bohm shaped Feyerabend’s argument for theoretical pluralism in contrast to Popper’s falsificationism is a prerequisite for appreciating Bohm’s deep influence on Feyerabend’s formative develop with respect to the views set out in ERE and his subsequent break with Popper’s school. As we shall see, Popperstein’s baby played a starring role in a horrifying nightmare that grew into the nefarious myth that Feyerabend's “incommensurable” implies “em-

[326] Because the test-model requires revisions to Popper’s definitions of both the empirical content and indirect refutation of a theory.

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pirically incomparable", or put more directly, that according to Feyerabend "incommensurable" *universal* theories are supposed to be "untestable" framework theories, which seems to be implied by Popper in **The Myth of the Untestable [Incommensurable] Framework** (1994). This was not what Feyerabend was trying to say with "incommensurable" in ERE (1962), as Feyerabend develops a test-model in contrast to falsificationism specifically to illustrate how rival incommensurable theories can still be tested against each other empirically by crucial experiments despite meaning variance in the terms used to state them that render them deductively disjoint based on novel corroborations. Put simply, according to Feyerabend in ERE (1962), while "framework" theories are not falsifiable, they are still empirically testable with crucial experiments based on novel corroborations deduced from incommensurable rivals. There can be good reasons for replacing them with respect to an attempt at a realistic interpretation of experience. New universal theories can be more comprehensive. They can have greater generality because they can take novel phenomena into account, which the theories that they replace cannot, and potentially explain everything else too. [327]

This account of the birth and life of Popperstein's, a misunderstanding of incommensurability that which this essay hopes finally to put to rest, has three sections. The first section (2.1 Feyerabend's awakening) establishes that Feyerabend's ERE (1962) criticism of Popper's methodology is in fact what led to Feyerabend's nightmare and his feeling of utter isolation in December 1967, and not *primarily* concurrent disputes with Popper about issues in the foundations of quantum mechanics. [328] For that we will turn to an as yet unpublished letter that Feyerabend sent to Watkins on the 17th of December, 1967; hereafter the "Watkins letter", [329] which tells a story that is corroborated by a letter Feyerabend sent to Lakatos on the same day. In both letters, Feyerabend announces his break with the Popperian school for the same basic reason: his rejection of Popper's

[327] Feyerabend also argues that new universal theories have greater generality than the rivals they replace, while trying to explain how to be a good empiricist (See Feyerabend, "How to Be a Good Empiricist…") by being more tolerant of alternative proposals.

[328] Compare Collodel, "Was Feyerabend a Popperian…", pp. 27–56, which makes this misleading suggestion.

[329] Feyerabend to Watkins, 17 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians…**.

methodology (because it is descriptively inaccurate and normatively undesirable). To see this, one needs to appreciate the distinction between critical rationalism as a philosophy and Popper's empiricism as methodology (aka falsificationism). As we shall see, if one conflates falsificationism with critical rationalism, then the question, "Was Feyerabend a Popperian?" becomes impossible to answer correctly, because Feyerabend was criticizing falsificationism, while defending critical rationalism. A more fine-grained account of critical rationalism is not what is needed. A distinction between critical rationalism and falsificationism does the trick. The terms "Popperian", "Popperianism" and "critical rationalism" are unclear and potentially very misleading without explicit qualifications.[330] ERE (1962) was intended by Feyerabend not as *an extension* but as a criticism of falsificationism that limits its validity, and thereby, by extension, it was intended as criticism of Popper's form of critical rationalism. Moreover, it was *Feyerabend's criticism of Popper's methodology* that led to Feyerabend's feeling of isolation (as he was breaking with Popper's school because it had, for him, become a school philosophy) and not *primarily* disputes about issues in the foundations of quantum theory.

As we shall see, by equating critical rationalism with Popper's methodology and postponing a more fine grained approach to critical rationalism latter, the basic postulates of Collodel's approach are insufficient for assessing the evidence with respect to their relationship and how it developed and why, and so his assessments of the existing accounts are inaccurate. The evidence clearly indicates that Feyerabend was trying to criticize Popper's methodology, his normative philosophy of science (aka falsificationism), within a critical rationalist framework that accepts fallibilism and the maxim to increase testability. A more fine-grained analysis of *critical rationalism* will not help. A distinction between critical rationalism as philosophical framework and falsificationism as Popper's methodology is all that is needed.

In the 1960s, Feyerabend gradually changed how he taught Popperianism (presumably as having a limited validity given his own test-model),[331] until on the

[330] See Collodel, "Was Feyerabend a Popperian…", pp. 27–56.

[331] Collodel, ("Was Feyerabend a Popperian…", pp. 27–56), suggests Feyerabend's break only became "public" in the autumn of 1967, when he had the opportunity to give Popper's course "Introduction to Scientific Method" at the London School of Economics (LSE), exactly fifteen years after he had attended the same series of lecture as a student of Popper. This is true if "public" means public

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17th of December 1967, Feyerabend announced his brake with Popper's school, because of unresolved differences with respect to his criticism of Popper on *methodology* (and not his criticism of Popper's propensity interpretation as Popper's strategy for unspooling the great quantum muddle). Watching Lakatos lecture Popper's methodology to his students, he had an epiphany. He realized just how much he disagreed with it. Not even Popper's injunction against the use of *ad hoc* hypotheses to protect theories could be defended as a helpful methodological rule. It has a limited validity and there is no way to know in which contexts to apply it in advance. By mid-December 1967, Feyerabend felt so alone, isolated and misunderstood *with respect to his views methodology* that he was looking for a new philosophical home. He no longer considered himself a "Popperian" in the sense that he disagreed with Popper's methodology. He briefly even if only fleetingly considered joining Bohm's dialectical approach. [332]

Feyerabend tries to criticize Popper's falsificationism in a draft of ERE in 1960 because it is descriptively inaccurate and normatively detrimental for scientific progress. Moreover, from that perspective, Popper's "admirable" methodology also stands in the way of progress in philosophy of science in exactly the same way our best-established theories pose a challenge to new proposals that implic-

to Popperians at the LSE who did not understand ERE (1962) as an attempt to criticize Popper's methodology as it was intended. Feyerabend tried to go public with his criticism of Popper's methodology in ERE (1962) and he explicitly emphasizes that he gradually changed how he taught Popper's methodology, not that he abruptly changed how he taught it, finally going public all at once. He says he realized how many things he thought were wrong with it, not that it was wrong, or that he changed what he thought was wrong with it or why he thought it was wrong. The criticism Feyerabend uses against Popper's views in the Watkins letter, which he uses to justify announcing his break with Popper's school, were directed at Popper's normative methodology, which he begun to criticize in that way in ERE (1962). It is descriptively wrong and normatively detrimental. The awakening in 1967 was not an abrupt change of position that Feyerabend abruptly made public all at once time. Moreover, the epiphany was a not a change to a new position in methodology. It was a realization of where he already found himself, which was with no real position, but only a "position" explicitly in "scare" quotes. Feyerabend had criticized Popper's methodology in 1962, and now, in 1967, he was breaking with Popper's school, soon to find a new basis for his new "position" that he took with him when he left in the work of Jonh Stuart Mill, which was no new "position" in methodology at all.

[332] Like many lessons Feyerabend learned in philosophy, it took some time for this possibility to gesticulate, as initially he seems to have considered, but then decided against this possibility, although in the long run his philosophy can be seen to increasingly converge with Bohm's in many significant respects. Note that Feyerabend, apparently alluding to Kuhn, also criticized "some very implausible (because Hegelian) notions concerning the »growth« of knowledge" (Feyerabend, ERE (1962), p. 68, fn. 82), which he thought also apply to Bohm's methodological views.

itly use incompatible rules of usage, as for example with respect to the term "inkommensurabilität", just as the idea that Feyerabend was a falsificationist until the late 1960s stands in the way of understanding his theoretical pluralism in contrast to Popper's as a criticism of it. The new proposal looks incoherent without a revision to the basic postulates used to state the rival accounts (such as "empirical content" and "indirect refutation") as does equating critical rationalism with falsificationism.

According to Feyerabend, this is part of why everyday language should be revised, and why there should be more tolerance so as to allow science to progress unimpeded by it, so that there is progress not just in physics proper, but also in ontology and methodology according to his scientific conception of philosophy. [333] As we shall see, some proponents of the account that Feyerabend was a Popperian who broke with Popper and his school in 1967 use some of the same evidence (the Watkins letter) to come to a very different conclusion. Because there is no distinction between critical rationalism and falsificationism, as just one possible form of it, they begin with the wrong basic (Popperian) postulates in philosophy and methodology for understanding Feyerabend's views, which results in a distorted picture of the evidence and distorted assessments of what happened based on it. [334]

As indicated, our first goal is to show that it was Feyerabend's criticism of Popper on empiricism and not his criticism of foundations of quantum theory that primarily drove the wedge between Feyerabend, Popper and Popperian school

[333] Kuhn emphasized, at the time, that normal commensurable scientific progress is necessary for revolution to a new incommensurable theory, presenting the essential tension between tradition and innovation as a necessary dialectic for scientific progress. Bohm posits many more dialectical relations to explain progress. Feyerabend rejected any such purportedly necessary conditions on scientific progress throughout his early philosophy, periodically taking shots at such absolute "transcendental necessities". For Feyerabend, necessity, like truth, is relativized to the universal theory and the worldview used to experience.

[334] Collodel claims that his "adopted analytic strategy recommends distinguishing these different facets carefully" (Collodel, "Was Feyerabend a Popperian…", p. 31), but at the same time he continues to take Popper's critical rationalism to be synonymous to Popper's methodology broadly speaking when assessing the evidence for the different accounts and those accounts: "The phrase "critical rationalism" as used in this paper is to be understood as synonymous with "Popper's epistemological approach", characterized, among other features, by its fallibilism, the endorsement of criticism as a progressive attitude in the search for truth, and a normatively *falsificationist methodology of science*. *This broad outline is sufficient for the aims of this study*" (Collodel, "Was Feyerabend a Popperian…", p. 31, fn. 5 [emphasis added]). I beg to *differ*.

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between 1962 and 1967. [335] Then, in the next section (2.2 Popperstein's Baby and the Lessing Effect) we will turn to Popperstein's baby (the Popperian misunderstanding of Feyerabend's use of the term "incommensurable") to understand how this happened: How Popper's misunderstanding of "incommensurable" was born and the starring role that misunderstandings of Feyerabend's attempt to criticize falsificationism with the idea of incommensurability limiting its validity played in the genesis and subsequent development of Feyerabend's nightmare, [336] which then spiraled out of control into Feyerabend's increasingly inappropriate *ad hominem* attacks. The third and last section of part 2 will be to explain how ERE (1962) set the stage for Feyerabend's transition from the limited validity of Popper's methodology to **Against Method** and its "Anything goes!" when he broke from Popper's school in 1967. The postscript returns to Einstein's remarks on scientific method and Popper's perception of **Against Method** as a Lessing Effect, 2.0.

2.1 Feyerabend's Awakening (December 1967)

While disputes in the foundations of quantum theory undoubtedly contributed to the nightmare, for Feyerabend, they were not at the heart of it, at least not with respect to how his relationship to Popper developed from ERE (1962) until he awoke from his Popperian slumber into a living nightmare in December 1967. He was clearly worried that he may have enraged Popper with his latest draft criticizing those views that he had sent to him. Feyerabend made this very clear in his correspondences with Lakatos and Watkins, in both of which he announced and tried to explain his break with Popper's school on the same day. [337] The Watkins

[335] If this account is correct, the growing unease that Feyerabend could not explain why he felt with respect to Popper and his views in the 1950s may have been partly due to his (perhaps initially only latent) gradual awareness that his methodological views were incompatible with *Popper's account* of crucial experiments by falsification, which he only dared to address in ERE (1962) (after meeting Bohm). He clearly did not like his perceived role as having contributed to the establishment of Popperianism as a philosophical school. See Collodel's extremely informative analysis of that aspect in Collodel, "Was Feyerabend a Popperian...", pp. 27–56.

[336] See Feyerabend, "Science, Freedom, and the Good Life..", pp. 127–135.

[337] See the "Watkins letter" which was dated 17 December 1967 and compare it to the letter he wrote on the same day to Lakatos also announcing his break with Popper. See the Appendix and Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperi-**

letter can seem cryptic, [338] and it has become contentious as Collodel cites it to support a different view (that the tension between Feyerabend and Popper was primarily about foundations of quantum mechanics).

> [O]ne of the reasons why I was mad at Popper was that his paper did not pay any attention to my criticism of 1962. Maybe he had not read my paper (which I sent him); maybe he did not like it. But I thought I had refuted him and not a word from him on this matter." ([...] Feyerabend to Watkins, December 17, 1967). [339]

It does not seem definitively clear to me to which paper Feyerabend intended to refer, but by the end of the letter it turns out that Feyerabend is clearly trying to say that the dispute relevant to his break with Popper's school was about Popper's empiricism as both descriptively inaccurate and normatively detrimental, and not primarily in their dispute about the foundations of quantum theory.

To understand Feyerabend's letter to Watkins, one should try to imagine the question Watkins had asked Feyerabend to which Feyerabend is trying to reply. It has to be imagined, because there is no record of it, as the letter itself implies that their epistolary discussion takes up where their face to face discussion had left off, after Feyerabend returned from his visit with Watkins in London. [340] On my interpretation of this letter, the question in question seems to me, to be quite clear and so simple that it almost goes even without having to say: *Should you not have announced your changes of positions with respect to Popper's views on the foundations of quantum mechanics and Popper's methodology more clearly*?

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ans...

[338] Collodel, "Was Feyerabend a Popperian...", pp. 27–56 uses this same letter to support his "Popperian" rendition of what happened in 1962 between Feyerabend and Popper. There is no mention of the tension due differences on *methodology* in 1962 in contrast to those in foundations of quantum theory as set out in this same letter in that account. This essay uses the same letter to support the opposite conclusion about what was happening and why the nightmare was unfolding: The nightmare was due to Feyerabend's criticism of Popper's methodology in ERE (1962) as descriptively incorrect and normatively detrimental, and not his ongoing quarrel over issues in the foundations of quantum theory.

[339] Collodel, "Was Feyerabend a Popperian...", p. 50.

[340] The remark: "the day after our big meal at Bertorelli's (for which, I am sorry to say, I had to pay myself) I took a look at the sky, packed my suitcases, and hopped on the next plane available" (Feyerabend to Watkins, 17 December 1967), seems to take a jab at his host for not covering the cost of the meal. See Collodel, "Was Feyerabend a Popperian...", pp. 27–56 for more historical detail surrounding these events.

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The letter opens with a story that serves as a metaphor for his account of how he awoke from his Popperian slumber into a living nightmare: [341]

> the day after our big meal at Bertorelli's (for which, I am sorry to say, I had to pay myself) I took a look at the sky, packed my suitcases, and hopped on the next plane available — which I caught at a margin of 10 minutes. I had a marvelous flight — 10 hours and not a second more to Los Angeles, a whole row of seats to myself but when I arrived in San Francisco I was greeted by a storm of 80 miles/hour and 25 degrees temperature. The fence in front of my house is blown to pieces, in the back a big eucalyptus has fallen over and has smashed the platform. Ludwig's fountain at the university is frozen — a historical event unknown to the present generation and to its predecessors also.

This sets the tone *and* (retrospectively, that is after having read the letter) serves as a metaphor for the story he is about to tell. Feyerabend, the performer at work as a writer, seems to be comparing the state of his home after the storm to the nightmare into which he awoke from his Popperian slumbler, [342] which is completely unsurprising given Feyerabend's dramatic style. [343]

Watkins opens his reply with: "Happy New Year, and thank you for your »nightmare« letter", [344] to which Feyerabend responds in his next letter in January 1968 to Watkins (after the initial storm had passed), by beginning his letter with:

> you deserve being decorated for answering letters promptly, in detail, and charmingly. Having a hangover, a house without a bathtub and a toilet (workmen all over the place in order to repair things as a result of a storm) I needed cheering up — and I was cheered up. So, here my reply (and tell that other gentleman in the department, the one concerned with induction, that he should take an example from you). [345]

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[341] Eilenberger emphasized this point in his talk (mentioned in the next footnote).

[342] This point was made very forcefully by Wolfram Eilenberger with respect to the letter Feyerabend sent to Lakatos on the 17th of December 1967 (Eilenberger had not seen the Watkins letter) at the ETH Zürich, "100th Birthday of Paul Feyerabend workshop" on February 22. His inspiring and insightful talk brought the fact that these two letters were sent on exactly the same day to my attention. I had thought of them more generically as both sent in mid-December 1967.

[343] Eilenberger's wonderful presentation was to some extent incomplete, given what can be learned about what happened from the Watkins letter sent on the same day, and the Watkins correspondence more generally, which also shows that Watkins understood a draft of ERE (1962) to be intended as a criticism of Popper's methodology.

[344] Watkins to Feyerabend, 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

He seems to be suggesting that Watkins try to get Popper to respond to him more promptly.

In "the Watkins letter" on 17 December 1967, Feyerabend distinguishes and clarifies at least *two* changes of positions: The first one mentioned is his "*alleged* change from Bohm to Bohr". However, now that he had a chance to look at his papers more closely, he is going to amend that. As we shall see, he is also going to recount an epiphany that he had when he awoke from his Popperian slumber, while watching Lakatos lecture his students on Popper's methodology as part of correcting how his position had changed with respect to Popper's methodology. Feyerabend had decided he had been wrong about what he had said to Watkins about his change of position about Bohm and Bohr as well, as he continues directly: "For I discovered to my surprise that I know my own writings as badly as you do." He is saying that he misdescribed that transition to Watkins, and now he is going to tell him what really happened, as Feyerabend then proceeds to do just that in the Wakins letter:

> There exist articles in 1959, and there is especially my long article "Problems of Microphysics" of 1962 where my position is as follows: Bohr's interpretation of quantum theory is the best interpretation on existence; it is better than Popper's; but the attempt to show that it is not only excellent, but also the only possible interpretation must be rejected. [346]

Feyerabend then confirms: "So there was a change of position" with respect to Popper's views in microphysics, which Hanson was shocked about at the time: "Hanson recorded »surprise, and, indeed, a great shock« at my change of position." As I understand it, Feyerabend means his change of position away from Popper, toward Bohr. [347] Feyerabend is clarifying what happened by claiming he

[345] Feyerabend to Watkins January 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians…**

[346] Feyerabend seems to mean his "Complementarity…", so perhaps he was still somewhat hazy about when this issue started in his publications. For more details, consult Matteo Collodel, who has looked closely at the how Feyerabend's major papers developed from their drafts to their published versions.

[347] According to Feyerabend, Bohm considered his own view closest to Bohr's views. As I understand that transition in foundations of quantum theory, it was in support of Bohm's work in the foundations of quantum theory (and not Bohm's dialectical *methodology*, which Feyerabend explicitly rejected in favor of *falsificationism* (and not critical rationalism) in 1960. As far as I can see, this is the last time Feyerabend explicitly endorses falsificationism unqualified in this way in print. In

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did clearly announce himself in the case of his dispute with Popper, and his change of position on the foundations of quantum theory, *and it was noticed at the time*. He even calls his 1962 paper on the foundations of quantum theory ("Problems of Microphysics") his "Anti-Karl" paper, presumably because the main claim in that paper, as he describes it in the Watkins letter, is that Bohr's strategy is better than Popper's. It is important to understand the context: Bohr's early strategy is also Feyerabend's own strategy, although Feyerabend did not adopt it from Bohr, but initially used it to try to criticize Bohr with it (following Popper's inaccurate account of Bohr instead of Bohr). [348] (Feyerabend inadvertently initially "lessened" Bohr in 1957, see part 3). Feyerabend then explains that he even repeated some passages about this criticism on the foundations of quantum mechanics verbatim in his 1959 paper [349] and his 1962 microphysics paper, and he is doing it again in a paper he is now working on that he had sent Watkins and Popper asking for comments. Then Feyerabend seems to change the subject abruptly, at least for the moment, by suggesting that this is:

> another fine occasion to say that I say something, but now only for the third time: Do you think I am doing *such things* only in "Problems of Empiricism"? [350]

ERE (1962) and after, Bohm is chastised implicitly and explicitly for aspects of his dialectics, and in ERE (1962), although Feyerabend then explicitly significantly qualifies some of his objects to some aspects of Bohm's dialectical views in Paul K. Feyerabend, "Dialectical Materialism and the Quantum Theory", *Slavic Review* 1966, Vol. 25, Vol. 3, pp. 414–417, https://doi.org/10.2307/2492853.

[348] This was a "lessening" of Bohr by Feyerabend (see section 3.2 below). Feyerabend found out his mistake at the Colston conference. He may have felt dupped by Popper, who was promoting the "myth of the Copenhagen interpretation". See Don Howard, "Who Invented the »Copenhagen Interpretation«? A Study in Mythology", *Philosophy of Science* 2004, Vol. 71, No. 5, pp. 669–682. How embarrassing for Feyerabend it must have been to learn of his "lessening" of Bohr *after his talk*? This, together with an earlier (unjust from Feyerabend's perspective) accusation of plagiarisms with respect to Paul K. Feyerabend, "Eine Bemerkung zum Neumannschen Beweis", *Zeitschrift für Physik* 1956, Vol. 45, pp. 421–423, may help explain Feyerabend's negative attitude toward Popper on these issues. In any case, their situation was extremely complicated due to many such factors.

[349] Compare Feyerabend, "Complementarity..." and Feyerabend, "Problems of Microphysics...", as well as Paul K. Feyerabend, "On the Quantum-Theory of Measurement", in: Stephan Körner and Maurice Henry Lecorney Pryce (eds.), **Observation and Interpretation: A Symposium of Philosophers and Physicists, Proceedings of the Ninth Symposium of the Colston Research Society**, Butterworths Scientific Publications, London — New York 1957, pp. 121–130.

[350] See Feyerabend, "Problems of Empiricism...".

Introducing the topic of empiricism in this way (as yet another topic where he had repeated himself three times) seems to imply that he had also tried to explain some *such* other change of position (concerning Popper with respect to empiricism) for the third time in "Problems of Empiricism" *in contrast to* repeating his criticism of Popper's views on the foundations of quantum theory three times. This hypothesis is corroborated by Feyerabend's criticism of Popper's empiricism (it needs to be disinfected of its empirical core) in ERE (1962) in a big, misunderstood reveal, in the simplified summary of ERE (1962), "How to Be a Good Empiricist" in 1963 very explicitly, and then again in "Problems of Empiricism" in some of exactly the same words in 1965. On issues in the foundations of quantum theory, Feyerabend's 1962 microphysics paper was already the second time he criticized Popper, [351] who he had been criticizing in the foundations of quantum theory since 1957, with the third one in progress, which eventually (during their break up) became Feyerabend's mammoth two-part tirade "On a Recent Critique of Complementarity", Parts 1 and 2. [352] "Problems of Empiricism" was published in 1965, and repeats the view Feyerabend had earlier used to criticize Popper's empiricism in ERE (1962), and was his third go at that, after "How to be a Good Empiricist" (1963), [353] which Feyerabend says is a simplified summary of both ERE (1962) and "Problems of Microphysics", which indicates that Feyerabend had meant to disinfect Popper's empiricism in ERE (1962). [354]

In other words, Feyerabend tried to announce his "change of position" with respect to Popper's empiricism in ERE (1962), which he then *repeated twice* (just

[351] Following Feyerabend, "Complementarity…".

[352] See Paul K. Feyerabend, "On a Recent Critique of Complementarity: Part I", *Philosophy of Science* 1968, Vol. 35, No. 4, pp. 309–331 and Paul K. Feyerabend, "On a Recent Critique of Complementarity: Part II", *Philosophy of Science* 1969, Vol. 36, No. 1, pp. 82–105.

[353] Feyerabend, "How to Be a Good Empiricist…".

[354] Feyerabend, "Problems of Microphysics…". Compare Feyerabend, "How to Be a Good Empiricist…", and Feyerabend, "Problems of Empiricism…". Feyerabend noted in a note added in 1980 to ERE (1962) as republished as Feyerabend, "Explanation, Reduction and Empiricism..." (1981); that the same ideas are repeated in the sixth section of that paper ("Problems of Empiricism…") and *were not Popperian.* Compare "Galileo, Faraday, Boltzmann and Einstein brought about progress by the skillful application of alternatives. In many cases *no other way was possible* (cf. again my "Problems of Empiricism", esp. section 6)" (Feyerabend, **Realism, Rationalism and Scientific Method...**, p. 108, fn.14). Note Feyerabend had also developed the same ideas without the Brownian motion example and not as a criticism of Popper in Feyerabend, "An Attempt at a Realistic Interpretation…" loosely based on his 1951 dissertation.

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like in the foundations of quantum theory). The first time Feyerabend tried to criticize Popper's *empiricism* was in 1962 in *ERE*, which represents the culmination of Feyerabend's early views (drawn together from the four areas of his research: formal logic and its history, analytic philosophy, foundations of quantum theory and general methodology) and introduces his theoretical pluralism and his methodological argument for it based on the (Bohmian) Brownian motion example, which becomes the centerpiece of his methodological views through to **Against Method** (1993). Feyerabend is saying that he had been trying, but failing (in Popperian eyes) to announce his change of position on empiricism away from Popper's empiricism to a disinfected normative account of empiricism since 1962. [355] To put it another way, Feyerabend also thought he had changed positions and (announced it) with respect to Popper's empiricism in ERE (1962), *in contrast to* his change of position in microphysics that he had already made earlier *in a 1959* publication, [356] and again for the second time in "Problems of Microphysics" (1962), with the third still being prepared. [357]

That is what Feyerabend seems to be alluding to: two separate changes of position: one in methodology and one in the foundations of quantum theory, both criticism of Popper's views, and both for the third time, neither of which had been resolved with respect to Popper and the Popperian school. Now the very next thing Feyerabend says is also quite significant: "All these things I had forgotten when we discussed the matter". This implies that Feyerabend had forgotten to tell Watkins about these points with respect to these *changes* of positions with respect to Popper in 1962, that is, both with respect to microphysics and *Popper's position on empiricism in* ERE (1962). Again, no wonder Feyerabend admits that he does not know his own papers as well as Watkins. Now the next sentence in the

[355] Collodel, "Was Feyerabend a Popperian...", pp. 27–56 correctly emphasizes the normative critical rationalist framework within which Feyerabend was working in this period but does not recognize ERE (1962) as an attempt to criticize Popper's methodology. He presents that as part of a *backwards projection* after their break as a reinterpretation of ERE (1962) (similar to his Ehrenhaft proposal as a rediscovery or retrospective reattribution in this respect), see Matteo Collodel, "Ehrenhaft's Experiments on Magnetic Monopoles: Reconsidering the Feyerabend-Ehrenhaft Connection", *International Studies in the Philosophy of Science* 2022, Vol. 35, No. 1, pp. 69–94, DOI: 10.1080/02698595.2022.2123099 and the postscript to part 3.

[356] Feyerabend, "Complementarity...".

[357] Feyerabend had sent Watkins a draft for comments, corrections, and suggestions, of his new third criticism of Popper on the foundations of quantum theory, see Feyerabend, "On a Recent Critique of Complementarity: Part I..." and Feyerabend, "On a Recent Critique of Complementarity: Part II...".

letter is somewhat trickier and, by itself, is rather ambiguous: Feyerabend writes: "I can even tell you that *one of the reasons* why I was mad at Popper was that his paper did not pay any attention to my criticism of 1962" (italics inserted). [358] It is not all that clear which criticism in 1962 he means or if he means both, but as it will turn out, it does not seem to matter much.

Collodel's reading of this letter is at least partially correct, as Watkins clearly took this to mean at least that Feyerabend was mad about Popper for not responding to Feyerabend's "Problems of Microphysics", as he replies in his next let-

Popper had not provided critical comments on the draft of what became Feyerabend's, "On a Recent Critique of Complementarity: Part I..."and Feyerabend's, "On a Recent Critique of Complementarity: Part II...", neither by mail, nor during the 1967 fall term, while Feyerabend was in London teaching at the London School of Economics, where he gave Popper's introductory lecture course, see Paul K. Feyerabend and Karl R. Popper, "The Feyerabend–Popper Correspondence (1948–1967)", in: Matteo Collodel and Eric Oberheim (eds), **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper,** *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Venice — Berlin 2024, pp. p. 419, fn. 4 [59–422]. Popper's only appeared much later in Karl R. Popper, **Quantum Theory and the Schism in Physics. Volume III** of the **Postscript to The Logic of Scientific Discovery**, W. W. Bartley III (ed.), Hutchinson and Company, London — Rowan — Littlefield — Totowa 1982 in response to Jeffery Bub's (Bohm's close colleague) development of Feyerabend's view, see Jefrey Bub, "Popper's Propensity Interpretation of Probability and Quantum Mechanics", in: Grover Maxwell and Robert M. Anderson (eds.), **Induction, Probability, and Confirmation, Minnesota Studies in the Philosophy of Science, Vol. VI,** University of Minnesota Press, Minneapolis 1975, pp. 416–429 and to Max Jammer's remarks on Feyerabend and Bub's criticisms of Popper's views in Max Jammer, **The Philosophy of Quantum Mechanics: The Interpretations of Quantum Mechanics in Historical Perspective**, Wiley, New York 1974, see pp. 450–452 and compare Popper, **Quantum Theory and the Schism in Physics...**, p. 71 fn. 63, pp. 72–73 fn. 65, and p. 153 fn.2.

[358] Popper had not provided critical comments on the draft of what became Feyerabend's, "On a Recent Critique of Complementarity: Part I..."and Feyerabend's, "On a Recent Critique of Complementarity: Part II...", neither by mail, nor during the 1967 fall term, while Feyerabend was in London teaching at the London School of Economics, where he gave Popper's introductory lecture course, see Paul K. Feyerabend and Karl R. Popper, "The Feyerabend–Popper Correspondence (1948–1967)", in: Matteo Collodel and Eric Oberheim (eds), **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper,** *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Venice — Berlin 2024, pp. p. 419, fn. 4 [59–422]. Popper's only appeared much later in Karl R. Popper, **Quantum Theory and the Schism in Physics. Volume III** of the **Postscript to The Logic of Scientific Discovery**, W. W. Bartley III (ed.), Hutchinson and Company, London — Rowan — Littlefield — Totowa 1982 in response to Jeffery Bub's (Bohm's close colleague) development of Feyerabend's view, see Jefrey Bub, "Popper's Propensity Interpretation of Probability and Quantum Mechanics", in: Grover Maxwell and Robert M. Anderson (eds.), **Induction, Probability, and Confirmation, Minnesota Studies in the Philosophy of Science, Vol. VI,** University of Minnesota Press, Minneapolis 1975, pp. 416–429 and to Max Jammer's remarks on Feyerabend and Bub's criticisms of Popper's views in Max Jammer, **The Philosophy of Quantum Mechanics: The Interpretations of**

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ter to Feyerabend: "you are quite justified in feeling sore at Karl's ignoring of your criticism of his Propensity interpretation of Quantum Theory", [359] however even this may have been meant to imply that this is not the case with respect to his other 1962 criticism of Popper's methodology. It does not matter that much if it was intended as ambiguous or specifically just to one, because this is explicitly just *one of the reasons* he was mad. If this was not *the* reason that he was mad at Popper in 1962, but just one of the reasons, then what were the other reasons?

Feyerabend's formulation "one of the reasons" makes you wonder what those other reasons are, and how significantly he thinks they were relative to each other. And at this point, Watkins's interest must now really be peaked. First, Feyerabend admits he was wrong about what he had told Watkins about announcing his changes of positions with respect to Popper in 1962, and now he is about to tell more reasons he was mad at Popper. Moreover, as we shall see, it is about to get dramatic, and very personal, as by the end of the letter, it turns out that Watkins himself plays a starring role in Feyerabend's living nightmare. Watkins clearly understood Feyerabend on *this* point: as in his reply he begs to put off the discussion until after Christmas as: "after all, I'm one of the figures in your nightmare". [360] Moreover, it will turn out that the reason for this according to Feyerabend is that Watkins himself is *still* so stuck in Popper's methodology as an "ideology" that he is unwilling to try to see how Feyerabend's criticism of Popper's empiricism is supposed to work. That is why Feyerabend feels like he is losing his friend (Watkins). Feyerabend *was* mad at Popper, while he seems rather sad about Watkins.

On this interpretation, Feyerabend feels completely isolated because he cannot even get through to his friend John, because he (John) is stuck in a Popperian fog with respect to Popper's empiricism as an ideology (as illustrated by his *unwillingness* to continue to try to understand Feyerabend's criticism of Popper), and not because of his criticism of Popper's view in the foundations of quantum

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Quantum Mechanics in Historical Perspective, Wiley, New York 1974, see pp. 450–452 and compare Popper, **Quantum Theory and the Schism in Physics...**, p. 71, fn. 63, pp. 72–73, fn. 65, and p. 153, fn.2.

[359] Watkins to Feyerabend, 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

[360] Watkins to Feyerabend, 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

theory. Ever since the first draft, Watkins simply could not see how ERE (1962) hits its intended target (Popper's empiricism). It seemed more like a "lessening" of Popper than a criticism of his methodology (just as Popper seems to have seen it). [361] Watkins understood that Feyerabend's argument in ERE was *intended* as a criticism of Popper even before it was published (even if some others may seem not to have noticed this), but Watkins could not understand how it was supposed to hit its target. It clearly seems to him more like a repetition, not even as, some sort of extension that can be used to fix a minor problem with Popper's account.

In stark contrast to his rejection of Feyerabend's attempt to criticize Popper's empiricism, Watkins was clearly still undecided about the potential legitimacy of Feyerabend's criticism of Popper's propensity interpretation:

> Whether it really does mean that the particles obey the conservation laws only statistically I should like to see thrashed out — he [Popper] says (in the *Postscript*) that his Indeterminism leaves the conservation laws intact. Why not a seminar on this next summer — it ought to be more fruitful than that heart-to-heart you wanted last term? [362]

This key passage suggests that a semester earlier, Feyerabend had wanted a heart-to-heart on his criticism of Popper's empiricism and Watkins had turned him down! This implies that Watkins was no longer interested in continuing to beat *that* dead horse. Their dispute about Feyerabend's argument as a criticism of Popper's methodology, which clearly looked much more like a repetition of Popper's views than a criticism of them to Watkins, had begun in 1960 well before ERE (1962) was published. In this sense Popperstein's baby was conceived in 1960 and was a pre-mature birth. It seems safe to assume that Popper thought something similar. Popper seems to have thought that Feyerabend was (a) stealing his ideas and (b) then trying to present them as criticisms of his views (and so of course failing to criticize them). As we shall see, in his response a decade later, Popper implied (a) and did not even deem it worthy to mention (b).

That this dispute in methodology had never been resolved is reflected by the fact that Feyerabend still wanted to have a "heart-to-heart" about six years later,

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[361] See Watkins to Feyerabend 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

[362] Watkins to Feyerabend, 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

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but which Watkins had declined. This is what directly set the stage for his awakening (Watkins's turning down his suggestion to "heart-to-heart" to heart on his criticism of Popper's empiricism). This suggests that the heart of the matter for Feyerabend, which Feyerabend had asked for a "heart-to-heart" on, has to do with *empiricism* and not Feyerabend's criticism of Popper's propensity strategy in foundations of quantum theory, which Watkins takes to be potentially more fruitful than Feyerabend's alleged criticism of Popper's empiricism.

This is how Watkins played his starring role in the unfolding drama. Watkins got that message loud and clear (that he was part of the problem in this way). In his reply, Watkins suggests that they "revisit that nightmare scene in the springtime when things tend to look jollier anyhow". [363] This follow-up letter also implies that Watkins had understood that Feyerabend meant that the nightmare scene has to do with empiricism and not Popper's propensity interpretation of quantum indeterminacy. In his response to Feyerabend on 30th December 1967, with respect to Popper's empiricism, Watkins encourages Feyerabend not to keep trying "to burn it or stamp it out," but rather, to "try to improve it," as: "Despite your opposition to it you still find it the »most acceptable« philosophy at present available". Plainly, Watkins understood very clearly that Feyerabend's criticism of Popper's empiricism was intended to do something much more negative (burn it/stamp it out) than simply to repeat, or improve or extend it, and that the dispute he is talking about was about empiricism and not the foundations of quantum theory, as Watkins is clearly not suggesting that Feyerabend try to improve Popper's propensity interpretation of quantum theory.

Moreover, Watkins is clearly trying to convince Feyerabend *not* to leave the Popperian school, by inviting him to run a seminar together on his criticism of Popper's propensity interpretation of quantum theory. (By changing the subject to something they can fruitfully discuss in contrast to his attitude toward Feyerabend's criticism of Popper's empiricism). And Watkins's reaction to Feyerabend explaining his motivation for leaving the Popperian school shows that he still disagrees with Feyerabend about his criticism of Popper's empiricism, and also that he has not understood how Feyerabend's criticism of Popper's methodology as "anti-humanitarian" (because it is anti-progressive, and so dogmatic and dangerous) is supposed to work, thereby demonstrating Feyerabend's point, from Feyer-

[363] Watkins to Feyerabend, 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

abend's perspective. Watkins responds to Feyerabend's criticism of Popper's empiricism as antihumanitarian while announcing his break: "Popperianism »is also antihumanitarian« — balls". [364] Quite the comeback to the second prong of Feyerabend's two pronged argumentative strategy criticizing Popper's empiricism. On this interpretation, Feyerabend is saying to Watkins, what I said before at the restaurant was wrong, I did announce my changes of positions both with respect to Popper on methodology, and with respect to the dispute about the foundations of quantum theory, both of which I have repeated more than one publication. Moreover, it is (at least in part) because you (my friend John) still do not understand how (not that) [365] ERE (1962) is supposed to be a criticism of Popper's empiricism that makes me (Feyerabend) feel so alone and at a loss. Watkins motivated Feyerabend's step out of the Popperian school to his "Anything goes!" approach in this way, when Feyerabend tried to show that his argument undermines not just falsificationism, but general methodology more generally.

Directly after telling Watkins that *one of the reasons* he was mad at Popper was his lack or response to his 1962 Feyerabend continued: "Maybe he [Popper] had not read my paper (which I sent him); maybe he did not like it. But I thought I had *refuted* him and not a word from him on this matter". This could apply equally to both 1962 criticisms of Popper's views. The Feyerabend-Popper correspondence indicates that from Feyerabend's point of view, he and Popper had not come to terms with either issue. More specifically, they had not come to terms in their discussion of Feyerabend's incommensurability, which he had tried to use to criticize Popper's empiricism:

> I am sorry that I could not discuss with you certain matters in a little greater detail, especially the matter of the incommensurability of theories. I quite agree that the earth rotates like a wheel both in classical physics and in relativity, but my problem is the relation of the classical wheel to the relativistic wheel. Perhaps the best thing would be to give a talk about it in your seminar sometime next year. [366]

[364] Watkins to Feyerabend, 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...** With "Popperianism »is also antihumanitarian«", Watkins is citing Feyerabend back to Feyerabend. The complete passage reads: "Popperianism »is also antihumanitarian« — balls; reminds me, distantly, of the title of a paper by Bernard Williams (German Existentialist): »Popper leads to Auschwitz«".

[365] Watkins had explicitly suggested that he sees ERE (1962) as compatible with Popper's views in comments on a draft in 1960, implying he does not see how it is a criticism of it.

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On my reconstruction of Feyerabend's argument this is *exactly* at the heart of their unresolved dispute that led to their break up. According to Feyerabend, what counts as a wheel changed, so observation statements about the classical wheel cannot be explained by observations of the relativistic wheel. This indicates that Feyerabend was actively trying to explain his idea of incommensurability to Popper in the spring of 1967, which implies Feyerabend believed that Popper had not yet understood it, and that it had explicitly been an open issue. In other words, Feyerabend was asking for a chance to explain his criticism of Popper's empiricism based on his idea of incommensurability to both Popper in the spring of 1967, and to Watkins in 1967 in a co-run seminar, and neither seems to have been interested. As we shall see, Feyerabend also seems to have harbored some trepidation of the impending confrontation with Popper. A Popperian will read this passage and insist that it cannot be intended to characterize Feyerabend's criticism of Popper's normative methodology. Feyerabend cannot mean to have "refuted" Popper's empiricism in the Watkins letter, because as Feyerabend emphasizes in ERE (1962), it is normative. Normative accounts cannot be "refuted", and Feyerabend surely knows this. This can be seen in the tender ridicule in Popper's response to Horgan:

> I launch my gotcha question: "*Is your falsification concept falsifiable?*" Popper glares at me, then his expression softens, and he places his hand on mine. "I don't want to hurt you", he says gently, "but it is a silly question". [367]

But to understand Feyerabend, we need to try to interpret him according to his framework, and from his framework that is what Feyerabend thought he did with his signature two-pronged argumentative strategy. It is descriptively false (of the accepted cannon), and normatively undesirable. Feyerabend used this strategy to criticize and thereby "refute" any number of normative methodological proposals, all on account that they are descriptively inaccurate of progress that has been made, and they would stifle progress (and so are potentially dangerous) if taken to be understood as universal normative rules, often pointing to the Brownian motion example, and arguing for his theoretical pluralism in this way.

[366] Feyerabend to Popper, 21 April 1967 in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 418.

[367] Horgan and Popper, in: John Horgan, "The Popper Paradox", *Cross-Check. John Horgan's Free Journal* 2023, https://tiny.pl/dxzx3 [03.03.2025].

In this sense, Feyerabend thought he had "refuted" Popper's empiricism, and others as well. In any case, in the Watkins letter, Feyerabend continues:

> Of course, there still remains the problems, discussed by us in the restaurant, whether changes of position *should be announced*.

So, now we know it looks a lot like we got the question Feyerabend was answering at the restaurant correct, and that we are on the right track, and that there is still more to come on the issue of announcing changes of "position" to help clarify what Feyerabend is trying to say.

Feyerabend briefly begins to contrast between publishing in friendly and unfriendly journals (to which he will return shortly), while agreeing with Watkins's view about announcing change of positions in some specific situations (in contrast to other specific situations), as "Friends, after all, have a right to know," (well maybe not a right, "but they may reasonably expect to be told"). Feyerabend's point can seem a bit unclear, but it seems very clear that next he is alluding to the fact he was *invited* by Mary Hesse [368] *to withdraw his submission* to the special issue in *The British Journal for the Philosophy of Science* on Popper's philosophy with respect to what became "On a Recent Critique of Complementarity" [369] with the unconvincing excuse that they needed to "limit the number of papers". [370] Nice, an invitation to withdraw his invited submission to a special issue on Popper. That could not have helped relieve the tension between Feyerabend and Popper and the Popperian school, and Feyerabend seems clearly to be suggesting that this is part of what was going on. Then Feyerabend finally makes incontrovertibly clear what change of position he is now talking about in contrast to his change of

[368] Watkins asked him not to blame her: The *BJPS* decision on your paper was very wrong — but all complaints to Körner, please" (Watkins to Feyerabend, 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**).

[369] And Feyerabend, "On a Recent Critique of Complementarity: Part II..." as Feyerabend's reaction was to rewrite and considerably enlarge the paper in the late winter of 1968 following suggestions made by Bub and an anonymous referee, which was and then published in two parts as Feyerabend, "Science, Freedom, and the Good Life..", pp. 127–135 and Feyerabend, "On a Recent Critique of Complementarity: Part II...". See Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 420, fn. 4.

[370] See Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 420, fn. 4.

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position with respect to the foundations of quantum theory in "Problems of Microphysics":

> This is the reason why I told the class *whose last meeting you attended* why I could no longer follow certain tenets of Popperianism (students of Popperianism I regard as friends in a wider sense). I told them what my problems were and why I believed that Popperian *methodology* did not solve them" (italics inserted).

Bingo! In this letter, Feyerabend is explicitly explaining to Watkins why he did not announce his change of position on *Popper's methodology* (Popper's empiricism) more clearly in 1962 and that, regardless of what he said earlier, he had announced his change of position on Popper's methodology three times. Apparently, Feyerabend had suggested to Watkin's something about why he had not clearly announced his changes of positions in microphysics and methodology, and now is correcting what he had said. He did announce himself clearly. Again, this also seems to suggest that Feyerabend was also mad at Popper for not having come to terms with him with respect to his criticism of Popper's methodology since ERE. As we shall see, except for the "Bibliographic Note" added to the republication of his "The Aim of Science" (1957) in 1972, Popper's response to what he took to be Feyerabend's views on methodology was only published much later in 1994, and even then, Feyerabend was completely ghosted, [371] only indirectly referred to by Popper, who first criticized and rejected Kuhn's views on incommensurability, and then spent pages and pages criticizing *others* who have propagated the myth of untestable framework theories by claiming there are "incommensurable" framework theories that are *untestable* without explicitly mentioning Feyerabend by name. While Popper seems to have meant Feyerabend (who else?), as we have seen, Feyerabend's claim was that incommensurable framework theories are unfalsifiable, not untestable. [372]

As we have seen, Feyerabend had clearly been trying to engage with Popper through some Popperians on methodology since the spring of 1967, as he also

[371] Popper had a rule never explicitly to criticize his former students in print, see Collodel, "Was Feyerabend a Popperian…", pp. 27–56. Is it just me or does this rule seem condescendingly arrogant, as if they could never have a valid criticism or be treated on equal terms? As we shall see, Popper broke this rule with respect to Feyerabend, when he republished "The Aim of Science", which just poured more fuel on the fire, given Feyerabend's reaction to how he responded.

[372] Note Feyerabend's complaint against Bohm's methodology was that nature forces us to speak the truth, which would make theories untestable. See part 3.

asked Watkins to run a seminar together on Popper's methodology and Feyerabend's criticism of it the semester before he announced his change of position on the 17th December 1967, which Watkin's had evidently turned down. That is why Feyerabend blames Watkins for his role in Feyerabend's nightmare. Watkins is too stuck in his Popperian ideology, that he refuses to engage with criticism Feyerabend's of it — proving Feyerabend's point that Popper's methodology had hardened into a dogmatic school that dogmatically dismisses criticism of it.

Together with the last remaining letter in their correspondence, Feyerabend sent Popper a draft of Feyerabend what became Feyerabend "On a Recent Critique of Complementarity". In the letter, Feyerabend says he realizes he may need to tone down his criticism of Popper in the draft, given as Popper had pointed out to him, he had been overly-harsh with Kuhn: [373]

> On rereading it [Feyerabend's draft of what became "On a Recent Critique of Complementarity"] I find that on various occasions I have expressed myself rather harshly [...] but I don't think this will do any harm. At any rate I am very curious to hear what you think of it. I am also very much looking forward to seeing you again. [374]

It seems it may have done more harm than he thought it would, or more likely he was being somewhat sardonic. To mix a bunch of metaphors, for Popper this may have been the straw that broke the proverbial camel's back, with Feyerabend burning the bridge behind him as he vaulted out of the gate when leaving the Popperian school while slamming the door shut behind him as he left. Feyerabend

[373] Feyerabend is referring to a draft of what became Paul K. Feyerabend, "Consolations for the Specialist", in: Imre Lakatos and Alan Musgrave, (eds.), **Criticism and the Growth of Knowledge**, **Proceedings of the International Colloquium in the Philosophy of Science, Vol. 4, London**, Cambridge University Press: London — New York 1970, pp. 197–230. The draft (and ensuing published version) draws heavily directly from letters that Feyerabend had sent to Kuhn on a draft of Kuhn's **The Structure of Scientific Revolutions...**, see Paul Hoyningen-Huene, "Two Letters of Paul Feyerabend to Thomas S. Kuhn on a Draft of **The Structure of Scientific Revolutions**", *Studies in History and Philosophy of Science* 1995, Vol. 26, No. 3, s. 353–387 and Paul Hoyningen-Huene, "More Letters by Paul Feyerabend to Thomas S. Kuhn on **Proto-Structure**", *Studies in History and Philosophy of Science A* 2006, Vol. 37, No. 4, pp 610–632. These letters, and Feyerabend's pointed and personal criticisms of Kuhn in them, played a significant role in the deterioration of their relationship. See "Introduction" in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**

[374] See Feyerabend to Popper, 4 October 1967, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 419.

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was to wary to approach Popper himself directly about it and even unsure whether Popper would even see him, see him:

> I am writing because at my prompting Paul tells me he very much wants to see you, especially to hear from you about a paper of his, and he does not know how to approach you. He knows how busy you are, and maybe I know too, but unlike him I think he can let you judge *whether or not you will see him*. Now, of course, normally I should let him decide whether to request you or not. But right now I do not feel at peace with this counsel of inaction. I think you have comments on his paper which you can convey to him with no preparation and at no loss of time, and I think whatever your view is, it will be preferable all round if you tell him what it is. [375]

According to Gilles's firsthand account of their relationship around that time, Feyerabend was anything but looking forward to seeing Popper in this period. Quite to the contrary, Feyerabend was skulking around the halls trying to avoid Popper, who could be heard in heated disputes with Lakatos over lunches from which Feyerabend was conspicuously absent, *about whether Newton's theory had ever been falsified*, which is exactly the point Feyerabend thought that Lakatos had "lessened" him on (by treating him as a Popperian, who was not adequately citing Popper, on methodology), [376] while adopting Feyerabend's own argument against Popper and turning it against Popper and Feyerabend as if it were his own. So the dispute about methodology (it needs to be disinfected of its empirical core given a problem with explanation), which had begun in 1960 on a draft of ERE (1962) had remained unresolved, and Feyerabend took Lakatos to be adopting his own objection to falsificationism while unjustly painting himself (Feyerabend) as a Popperian on methodology and criticizing him with that objection, although Feyerabend had been criticizing Poppers methodology on this point since 1962. Watkins had had enough of all of that, and Feyerabend finally burned the bridge behind him (unwittingly or not) by returning to his criticism of Popper on the foundations of quantum theory in a much harsher way, in a draft of what became "On a Recent Critique of Complementarity" that he had enclosed with what turned out to be his last letter to Popper a few months before he announced his break from the Popperian school due to his rejection of Popper's methodological views in December 1967.

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[375] Agassi to Popper, 6 November 1967, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 420, fn. 5 [emphasis added].

[376] See Collodel, "Was Feyerabend a Popperian...", pp. 27–56.

In other words, with respect to ERE (1962) and Popper's methodological views, Feyerabend's answer to Watkins's question implies, if you read between the lines, that he did not make his criticism of Popper's falsificationism in ERE (1962) more clearl, because look at what just happened with his draft (of what became "On a Recent Critique of Complementarity"). He had just been invited to withdraw it. [377] His excuse, however, does not seem to be finished, as what he says next is finally *very* informative with respect to what had happened:

> But I, right now, am at a complete loss. I have been teaching Popperianism for now about eight years, over 10 years when I include my time in Bristol. *I gradually changed as I went on.* [378]

Notice that with this formulation, he is clearly talking about Popper's empiricism (his methodological views) and not Popper's propensity strategy in foundations of quantum theory. And the next sentence in the letter is crucial:

> And now when I finally heard Imre's crystal clear lecture I realized why I *had* changed. I had *a curious feeling of utter unreality*. Here one was supposed to hear something about scientific theories about *Einstein's theory of gravitation, about its relation to Newton, Kepler, etc.* And not a single thing was correct.

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[377] With respect to Popper's influence of the genesis of Feyerabend's "Explanation, Reduction and Empiricism..." (1962) before it was submitted for publication, Feyerabend would seem to have been stuck between a rock and a hard place. Feyerabend could not be too openly critical of Popper. He had to give Popper a draft and get his approval before submmitting it, lest he be accused of ambushing him with his criticism later, and Popper (infamously) did not take criticism well. This may explain the set-up of the essay as a big reveal, and why Feyerabend seems to be so subtle with respect to not more explicitly stating the negative implications of his criticism of Popper's falsificationism as an anti-progressive methodology in ERE (1962). This subtlety gradually disappears in his "How to Be a Good Empiricist...", and "Problems of Empiricism...", and then completely from his "Explanation, Reduction and Empiricism..." (1981), where Feyerabend makes it very clear that he is trying to criticize Popper's methodology with incommensurability. As we shall see in part 3, Feyerabend never published the paper "Observationally Complete Theories: Some Observationson Quantum Theory (1958)" (now in: Matteo Collodel and Eric Oberheim (eds), **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper**, *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Venice — Berlin 2024, pp. 487–492) because Popper had convinced him not to, arguing it was worthless, even embarrassing. In sum, Feyerabend had to credit and praise Popper profusely, so as not to be accused (once again) of plagiarizing his ideas, while he was trying to use his ideas to criticize others (Hempel and Nagel), before turning the same criticism against Popper himself, which would perhaps explain the big reveal structure that was such a big flop.

[378] Feyerabend to Watkins, 17 December 1967, see the Appendix.

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So Feyerabend had had an epiphany: He suddenly realized why he had changed the way he had been teaching falsificationism gradually over the years. At this point there can no longer be much doubt about what Feyerabend is talking about. He is talking about methodology, specifically Popper's empiricism. The description of his criticism matches exactly the structure of the argument in ERE, which leads to the same conclusions. As we have seen, Feyerabend argued in ERE (1962) that falsificationism cannot take exactly this series into account, because Popper misrepresents the justification for the transition from Newton's theory to Einstein's, treating it as if it were just like the case of the justification for the transition from Kepler to Newton one of which is a transition between commensurable theories, and the other which is not. That is what Popper gets it wrong according to ERE (1962) by conflating commensurable cases with incommensurable cases, which falsificationism cannot take into account, because the latter are deductively disjoint.[379] Recommending scientists strive to falsify their theories, when this cannot be done in a scientific revolution, impedes progress by implying that there is only one rule of thumb that reflects the empirical method impeding progress is dangerous. Feyerabend had claimed that Popper's methodology shares a dogmatic element with school ideologies (through an unwitting implicit commitment to meaning invariance due to his use of formal relations that make corroboration transitive through revolutions), and is therefore normatively undesirable (see part 1). The main difference to Watkins in the letters is that he shortens the normative claim into a single word. Popper's methodology in the Watkins correspondence is characterized as "antihumanitarian" instead of anti-progressive which is dangerous as implied in ERE (1962).

In this way, Feyerabend's excuse to Watkins for not being more open about his rejection of Popper's methodological views in 1962 seems to be something like: I tried to announce my change of positions with respect to Popper's empiri-

[379] Notice too that by taking the two different kinds of cases to be the same, Popper makes the very first mistake Feyerabend attributes to contemporary empiricists in the first footnote. The first footnote, in this way, clearly intended to target Popper (his specific name was made *more general* in the transition from an earlier draft) specifically for this mistake (as described in the first footnote), as that is precisely the mistake Popper makes, according to Feyerabend, by treating testing commensurable theories empirically (Kepler to Newton) "as the same as testing incommensurable theories empirically (Newton to Einstein) because testing them *seems* to have exactly the same logical structure, but in fact involve two different and incompatible test-models, which leads to Popper's failure to take the difference into account. In sum, in ERE (1962) Popper turns out to be Feyerabend's ultimate target to whom Feyerabend alludes in footnote one.

cism and his propensity interpretation of quantum theory in 1962, and what did I get for it? My criticisms of Popper have been snubbed even by you, and I am now completely lost and alone. It is a veritable living nightmare. The unspoken (or unwritten) implication is look what happens when you openly criticize Popper. Can you really blame me for not announcing my change of position with respect to falsification more clearly back in 1962, given how Popper reacts to criticism? Can you really have expected me to have done it even more explicitly?

It is important to notice the italicized *had* in "when I finally heard Imre's crystal clear lecture I realized why I *had* changed" in the context of Feyerabend's earlier admission that he had somehow *forgotten* or repressed *all of this* when they (Watkins and Feyerabend) discussed his changes of positions face to face in the restaurant. Feyerabend may be suggesting that he somehow had not noticed the extent to which he gradually had changed the way he taught Popper's methodology, because he had forgotten that he had concluded that Popper's methodology is both descriptively incorrect (it cannot take incommensurable theories into account), and normatively undesirable in ERE (1962) (because it cannot take incommensurable theories into account), until he was *reminded* that that is how he *had* gradually changed how he had lectured on it while listening to Lakatos's lecture, when he came face to face with the realization he had been disagreeing with everything he heard Lakatos say about Popper's falsificationism long before December 1967, during a Popperian slumber. His epiphany was his realization of just how far he had strayed from the pack with respect to Popper's methodology and the Popperian school. He did not all of a sudden change his mind about Popper's methodology, or get a new idea about why it is wrong, or even what was wrong with it. In the letter, Feyerabend is very clear which implications for Popper's methodology he means, as Feyerabend continues:

> Moreover, a change to a more Popperian face would most likely affect science in a very undesirable manner. For example, it would make it impossible for us to retain refuted theories with the help of *ad hoc* hypotheses until a new interpretation of experimental results is available that shows that they are right after all.

It is the limited validity of Popper's methodological rules. This is what ERE (1962) implies on the interpretation developed in this essay with respect to falsificationism. It has little to do directly with the foundations of quantum theory, although in that discussion he was forced to retract the universality of his own realism, also preparing the ground for his generalization. Feyerabend concludes:

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"Considering *all this* Popperianism seems now to me like a *nightmare*, not at all better than Hegelian philosophy of Nature". [380] This also implies that the root cause of the nightmare was his dispute with Popper's normative methodology, not his dispute over foundations of quantum theory, which undoubtedly *also* contributed to the nightmare.

Feyerabend then directly continues:

> What now? In addition to being unrealistic the Popperian philosophy of science is also antihumanitarian: it stops midway between human interest and professional interest. [381]

Surely, this is not directed at Popper's propensity interpretation, but at his empiricism, as that is what Feyerabend's criticism of contemporary empiricism (Popper's included) implies ever since ERE (1962). The question "What now?" becomes "Whither to Feyerabend?" in the response from Watkins and the ensuing letter exchange. The question is understood as "where can Feyerabend find a new home, if he is leaving Popper's school?" as they continue to discuss Feyerabend's "nightmare". This can be clearly seen in Watkins's direct response to Feyerabend on the 30th of December 1967, which includes a recommendation that Feyerabend should not go to Bartley, because:

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[380] Feyerabend is alluding to Bohm's view. See the last sentence of his review of Bohm's book, Paul K. Feyerabend, "Professor Bohm's Philosophy of Nature" [Review of David Bohm, **Causality and Chance in Modern Physics**, Routledge & Kegan Paul and D. Van Nostrand, London 1957], *The British Journal for the Philosophy of Science* 1960, Vol. 10, No. 40, p. 321–338, where he takes sides with falsificationism against Bohm: "I cannot accept Bohm's methodology of caution [...] I prefer [...] the methodology of falsification as it has been developed by Popper" (Feyerabend, "Professor Bohm's Philosophy of Nature...", p. 338). In ERE (1962) and at least until 1967, Feyerabend did not accept Bohm's "dialectical' philosophy, as Feyerabend adamantly rejected any such transcendental necessities on how science should be able to be seen to grow. As indicated earlier, for Feyerabend, science is what we make it, and what we make of it. Feyerabend's first love in philosophy was Dingler, who made this very clear to him, and volunteerism permeates his philosophy from start to finish: "Dingler impressed me with his clarity, his confidence, and the way in which he built science from decisions. For years afterward I defused facts with suitably chosen *ad hoc* hypotheses" (Feyerabend, **Killing Time...**, p. 30). See also letter Feyerabend to Bechler, 1 January 1976, Paul K. Feyerabend, "Autobiographfical Remarks 1976", in: Matteo Collodel and Eric Oberheim (eds), **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper,** *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Venice — Berlin 2024, pp. 19–44.

[381] Watkins to Feyerabend, 30 December1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**.

> After two gods (Christ, Popper) had failed him, he [Bartley] should have begun to suspect that there are no gods, not that it is just possible that he is God III. [382]

Here and elsewhere in subsequent exchanges with Watkins, Watkins clearly understood Feyerabend was announcing his break from Popper's school and that he was looking for a new home, as they continued to discuss Feyerabend's nightmare into 1968.

In the Watkins letter, Feyerabend emphasizes: "All these things are not arguments, but series of motives. Popper's *philosophy* is to me the one most acceptable". [383] Feyerabend then continues in the same sentence, "and yet it [Popper's philosophy, more specifically, Popper's empiricism] now turns out to be just another nightmare. So, for the time being, I am lost". [384] The *nightmare* is resulting from his criticism and rejection of Popper's *methodology*.

Feyerabend concludes his point with a big finish:

> and [I, Feyerabend] can only look with admiration to people such as you [Watkins] who are *lost only locally* but whose *ideology* is otherwise quite safe. [385]

Ouch. Feyerabend tells Watkins that he is lost in his Popperian ideology (Popper's methodological views). This clearly implies that Feyerabend thinks that Watkins (who is a Popperian), who *still* does not get Feyerabend's criticism of Popper in ERE (1962) and is no longer interested in discussing it, is lost *only locally*, because he is secure in his Popperian ideology. In this way, Feyerabend uses a (whimsical?) back-handed compliment to drive home the point home. Feyerabend's ERE (1962) was supposed to expose Popperianism for pretending to be *the* logic of scientific justification as a dogmatic ideology, and Watkins still could not see how. Moreover, Watkins was caught up in it to the point that he did not ac-

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[382] Watkins to Feyerabend, 30 December1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

[383] Again, clearly Feyerabend clearly means Popper's views on empiricism, and not Popper's views on the foundation of quantum mechanics.

[384] "It is of course an advantage to have been a Popperian. For the other philosophies are not only inadequate, they are also quite definitely sickening (having returned to Berkeley I just got a little inkling of how bad philosophy really can be)". Feyerabend to Lakatos, 17 December 1967 in: Lakatos and Paul K. Feyerabend, **For and Against Method: Including Lakatos's Lectures...**, p. 121.

[385] Watkins to Feyerabend, 30 December1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

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cpet Feyerabend's invitations to continue to try to explain his criticism of it to him in the jointly run seminar he had proposed. Watkins wants Feyerabend to develop his idea within the framework of Popper's methodology instead. That is partially why Feyerabend feels so completely isolated, and why he blames the situation partially on Watkins. Feyerabend then remarks: "In my present mood I think the best thing will be to write the historical books" implying it may be best to try to avoid normative *methodology* all together, given how misunderstood he felt with respect to his criticism of Popper's normative methodology. [386]

Popperians tend to read Feyerabend as Popper did, *from Popper's perspective*, (go figure), as a "Lessing Effect" in methodology (more on this to come). They see the break in December 1967 as resulting primarily from disputes in the foundations of quantum theory caused by Feyerabend's unruly criticism of it. However, as Feyerabend pointed out, disputes about issues in the foundations of quantum theory had been going on for years. In 1957, at the Colston conference, Feyerabend outlined his own Bohrian strategy, in contrast to Popper's propensity strategy, which he presented on Popper's behalf (and seems to have less than half-heartedly defended), to show that quantum indeterminacy is compatible with classical laws. This strategy had been published in 1957 and Feyerabend says he criticized Popper in foundations of quantum theory in a 1959 publication well prior to what happened with ERE (1962) on *methodology*. Moreover, Feyerabend had been highly critical of Popper's propensity interpretation to Popper in letters even before the conference, in which Feyerabend told Popper that he thought that his propensity interpretation showed little if anything and was of no help to dealing with the real issue of quantum indeterminacy.

However, the Watkins letter shows that *from Feyerabend's perspective*, it was his 1962 failed attempt to criticize Popper on methodology *and not his criticism of Popper on foundations of quantum theory* that had begun even earlier that was chiefly responsible for this nightmarish development. [387] As we shall see in more

[386] See Feyerabend, **Philosophy of Nature...**, and a book he planned to write on the history of physics.

[387] The two issues are intricately connected in several significant respects with intolerance toward Bohm's proposals in the foundations of quantum theory exemplifying Feyerabend's argument for the need for more tolerance in general methodology. Feyerabend broke with Popper's propensity interpretation even before Feyerabend delivered it to the public for the very first time on Popper's behalf at the infamous Colston conference in 1957. It is infamous, because it is one of the first, if not the first, post-war conference on the foundations of quantum theory, which had been

detail later, the saga of Feyerabend's relationship to Popper's propensity interpretation and Feyerabend's own (Niels Bohrian) strategy [388] for dealing with the measurement problem dates back to Bohr's reaction to learning of Bohm's hidden variables proposal in 1952, which Feyerabend witnessed first-hand. It is also intricately interwoven together with Feyerabend's relation to Popper and Bohm and what happened when Feyerabend tried to break with Popper on methodology with ERE (1962), see part 3. [389] In any case, by 1967, given his criticisms of Popper's methodoloigcal views since 1962, he now felt so completely isolated from Popper and Popperians, so that by 1967 he felt completely lost and alone. Lakatos was lessening him by treating him like a Popperian while criticizing Popper's methodology for the same reason that Feyerabend was (Newton's theory has never been falsified), and Watkins no longer seemed willing even to try to understand his criticism of Popper's methodology that falsificationism needs to be disinfected of its empirical core because it cannot take testing rival universal theories into account due to incommensurability.

Watkins clearly understood that Feyerabend *intended* to criticize Popper from the start. In 1960, Watkins had already misunderstood Feyerabend's account as compatible with Popper's account, and Feyerabend knew this even before the publication of ERE (1962), as he said so in a letter commenting on a draft of it: "I

dormant due to the prevailing attitude, exemplified by Rosenfeld and the "Copenhagen school" ("Shut up and calculate!"), which Bohr himself did not share. For Feyerabend, Bohr was not a Borian in this sense, just as Bohm was not a Bohmian (see part 3), similarly Feyerabend was not a Popperian falsificationist *after* 1962, on this interpretation.

[388] Bohm's proposal would *explain* why there is the measurement problem on the quantum level caused by analosous processes at a lower-level. Feyerabend tried to show that quantum indeterminacy *cannot be* an empirical problem for the quantum theory.

[389] In a letter to Hempel in 1968, Feyerabend explained very explicitly just how far he had travelled from Popper with respect to his views on the foundations of quantum theory and its relation to classical physics, describing his paper (Feyerabend, "Problems of Microphysics…") as a "reply to a paper by Karl Popper in which he [Popper] said that in science the search for invariants was not everything and that one should permit irreversible, imprecise, indeterministic laws to enter the body of science. I say the very opposite, that is, I want our laws to remain precise, reversible, deterministic but want to put the imprecision and sloppiness into the method rather than into the laws. And I argue that the Copernican Revolution was made "my" way, rather than Karl's way. I also argue that classical physics is far from finished and that we have not yet really exploited it to the full)" (Feyerabend to Hempel, 20 April 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical…**, p. 201). As we shall see, in many respects, Feyerabend was proceeding *Bohm's way*.

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[Watkins] also accept your account of scientific progress which seems entirely compatible with P [Popper]. Yet you rather suggest your account reveals a serious inadequacy in P [Popper]". [390]

This is how Watkins saw Feyerabend: as lessening Popper, not criticizing him. Feyerabend also thanked Watkins in ERE (1962) for helpful suggestions (and especially for pointing out the implications of Hempel's consistency condition that he targets in Hempel's account of a more comprehensive). So Watkins also played a starring role in the genesis of Feyerabend's criticism of Popper's methodology by calling attention to the consistency condition, shared by Hempel and Popper, which is exactly what is incompatible with meaning variance according to Feyerabend.

Watkins, however, did not agree with Feyerabend's representation of Popper, nor did he agree that Feyerabend had actually managed to criticize Popper, complaining that by "explanation" in Popper account of testing (falsificationism) Popper is speaking loosely, not formally. [391] Feyerabend tried to nail Popper down exactly in response to Watkins complaint in ERE (1962), in an attempt to clarify how his view does criticize Popper's with respect to Watkin's complaint. Watkin's complaint was that he (Feyerabend) had not shown his view is incompatible with Popper, as far as Watkins can see from the draft he was commenting on, because Popper was speaking loosely not formally in the passage cited. In ERE (1962), Feyerabend tried to undermine Watkins's complaint by citing the long quote from Popper claiming that in a falsification the evidence is transitive.

For Watkins, Feyerabend is clearly targeting a red herring with his knit picking about Popper's (purportedly informal) use of "explain" in the passage cited in ERE (1962). Moreover, to Watkins, Feyerabend seems to be "lessening" Popper, as Popper had emphasized that methodological rules are just rules of thumb, so showing their limited validity is by no means a criticism. Feyerabend disagreed with Watkins's assessment that it is a red herring, and tried to show that is exactly how Popper glosses over the tension between deducibility as a condition for testability and meaning variance, by retreating to agreement of behavior about

[390] Watkins to Feyerabend, 19 August 1960, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

[391] See Watkins to Feyerabend, 19 August 1960, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

basic sentences instead of agreement of content about the of those sentences. The latter would be required for them to be transitive as evidence as in the Kepler case, but which is impossible due to meaning variance in the terms used to state rival universal theories.

To make a long story shorter, from Feyerabend's perspective, Popper's account has an internal problem (Feyerabend's criticism is immanent) that he calls "a problem of explanation" in Popper's account because the successful predictions of theories that are replaced would be "explained" by the new theory, when in fact, as Popper himself sometimes seems to imply that, they should be replaced. Feyerabend's point is that for Popper, if they can be taken as transitive collaborations because they are logically consistent, then they can also be formally explained by the new theoiry. But they cannot, so something is wrong with Popper's account. It needs to be disinfected of its empirical core is thus code for Popper's methodology needs to take meaning variance into account by admitting that there can be no formal relations between the earlier successful predictions and the predictions deduced from the new rival universal theory by accepting that rejecting the consistency condition should not apply to scientific revolutions.

For Feyerabend, by contrast, the statements expressed by the sentences that were confirmed as predictions used to corroborate the theory from which they were deduced cannot be explained by the new theory because they can have no formal relations as measurment. New statements replace old statements that look like the same statements deduced from the new theory because they are expressed by the same sentences, but they are not the same statements, as they have different implications because they get their meanings from the theories from which they have been deduced and so make different statements with the same words.

Popper's account is explicitly designed to try to allow us to replace just those predictions that should no longer be considered correct measurements, for example, by reducing the margin of error in the measurement procedure or developing new ones. According to Popper, as new improved measurement procedures are developed, they are used to correct and replace incorrect observation statements. In this way, according to Popper, the discrepancies in their predictions can be clarified ever more precisely, serving to further test the new theory and so prevent the new theory from appearing simply like an *ad hoc* maneuver designed to take just the available evidence. Nifty *and* admirable. But this does not address

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Feyerabend's criticism, which is based on Popper's acceptance of the principle of formal consistency of predictions deduced from rival incommensurable theories which is incompatible with a contextual theory of meaning. If predictions are deduced from classical mechanics and those predictions are to be formally consistent with predictions deduced from relativity theory, then the same terms used in the predictions must have the same meaning, regardless as to what the rivals may think they are agreeing about. Simply agreeing that these predictions are logically consistent or that they logically contradict, does not by itself make it so. It is an empirical question that can be investigated by considering their implications, and why they should have which implications. For Popper, the established evidence cannot be both transitive and replaced unless he conflates sentences with statements. For Feyerabend, the evidence should be replaced all at ones, when the new theory is established, and so the evidence is cannot be transitive. The evidence for the old theory is not the same evidence as the evidence for the new theory. (The wheels that are spinning according to classical mechanics are different kinds of wheels when predictions about them are used to test the relativity theory.)

According to Feyerabend, in this way, Popper's "agreement" of behavior glosses over the problem caused by the tension between formal relations and meaning variance, by misleadingly providing the illusion that accepted basic sentences provide a neutral observation language, when in fact identical accepted basic sentences make different statements as can be seen by their very different implications. Agreement of behavior about how to use sentences cannot be used to establish formal relations of content, and pretending that it can and should, and thereby can and should provide an empirical core as part of the logic of scientific justification will not promote progress. Instead, it hinders progress in understanding methodology which needs a better test-model for universal theories, and it stifles progress in science by putting overly restrictive conditions on the legitimacy of new proposals.

On the interpretation of ERE (1962) proposed in this essay, Watkins clearly does not understand Feyerabend's idea of incommensurability insisting that "If no relationship — we even think of incompatibility — existed between T and T' there could be no crucial experiment between them". So Watkins, like Feigl, seems to be suggesting Feyerabend's views makes crucial experiment impossible without a specification of the incompatibility between incommensurable theories, which Feyerabend locates in the incompatibility of the rules of usage of the terms used

to state the theory, and the incompatibility" of the ontological principles from which they follow.

The gist of Watkin's complaints are quite clear, as he anticipates several central contentious challenges to Feyerabend's idea of incommensurability that developed subsequent to its 1962 publication. According to Watkins, Feyerabend has not shown Popper to be wrong because Popper was speaking informally with "explains". Moreover, there can be no crucial experiments if rival theories are not incompatible in some way. If they or their corresponding predictions are not formally consistent or inconsistent, then in what sense are they even rivals? If they have no comparable consequences (given the condition of deductibility and the contextual theory of meaning), then how can they be? The question then arises, how can we even know incommensurable theories are even talking about the same things?

These questions reflect just how perplexing Feyerabend's idea of incommensurability as a criticism of falsificationism was to Popperians at the time, just as , to some, it continues to be perplexing today. For a Popperian, Feyerabend's criticism just does not make sense on so many levels. Some of the issues that come up are just Wittgenstein fog. For example, how do we know that they are talking about the same things and how do we even know that incommensurable theories are rivals? We can point. Feyerabend is claiming that they are universal theories, so they are both purporting to talk about the same things everywhere the same everything. Realistically interpreted, they are clearly rivals as they imply incompatible ontologies, as the new proposal implies that the concepts used to state the established theory have zero extension. It is Wittgensteinian fog because to resolve the problems, you just look at the situation, and realize what you already knew. Obviously they are rivals and maybe you don't need formal relations for a crucial experiment. Moreover, the rivals may have no trouble realizing that they disagree about how to understand what is happening unless someone is unable to learn how to understand one of the theories in its own terms. Of course, they can also just point at some phenomenon and agree to disagree about that. But because universal theories reconceptualize everything according to incompatible rules of usage of the terms used to state the rival universal theories, they and predictions deduced irrelevant to them cannot enter formal relations no matter how basic and agreed upon the sentences might appear to be to them.

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Feyerabend adamantly accepts Popper's insight that the empirical core can be ultimately based on nothing more than agreement of behaviors that are not secure, [392] but for Feyerabend this is not enough to establish formal relations between statements deduced from incommensurable rivals, which is why Feyerabend characterizes himself as a kind of *pragmatist*, who wants to make explanation from questions about testing in contrast to Popper. (At the same time, for Feyerabend, by treating a theory as an explanation, that is, as an attempt at a realistic interpretation of experience, according to the theory, it is, at least provisionally, accepted as true):

> It seems to me that the changes that are necessary will make it impossible to retain a formal theory of explanation, because these changes will introduce pragmatic or "subjective" considerations into the theory of explanation. This being the case, it seems perhaps advisable to eliminate altogether considerations of explanation from the domain of scientific method and to concentrate upon those rules which enable us to compare two theories with respect to their formal character and their predictive success and which guarantee the constant modification of our theories in the direction of greater generality, coherence, and comprehensiveness. I shall now give a more detailed outline of the reasons which have prompted me to adopt this pragmatic point of view. [393]

But for Feyerabend, agreement about behavior does not guarantee agreement about content required for formal relations to establish a falsification. The difference, from Feyerabend's point of view, is that Popper assumes that agreement on basic sentences is agreement on basic statements and thereby tries to smuggle in agreement about the content of the sentences as statements (from Feyerabend's perspective). Unless they are making the same statement, identical sentences are not logically consistent. In this way, according to Feyerabend, Popper's (and Carnap's) insight into the core of empiricism as ultimately nothing more than agreement of behavior runs deeper than Popper thought, which is why Feyerabend is trying to disinfect Popper's methodology of its empirical core by removing the consistency condition together with considerations about explanation from issues concerning theory testing.

Popper's insight is based on Popper's conception of empirical content as the set of basic statements a theory forbids. For two theories to compete empirically,

[392] See Feyerabend, ERE (1962), pp. 92–95.

[393] Feyerabend, ERE (1962), p. 92.

they have to make consistent predictions, and also for a falsification, inconsistent predictions about some of the same testable claims accepted by both parties. Otherwise, they cannot compete empirically and there can be no falsification. Feyerabend has a very different view, according to which universal theories compete without formal relations because they are deductively disjoint so that they make different statements with the same sentences.

Given all the fog Feyerabend had created with ERE (1962), in 1972, Popper tried to clarify this point: "We can compare theories with respect to testability only if at least some of the problems they are supposed to solve coincide". [394] To coincide their needs to be agreement about a basic statements. Feyerabend's account, by contrast, allows theories to compete while being deductively disjoint. Feyerabend's account implies that Popper's conspicuous use of "coincide" is ambiguous with respect to agreement of behavior and agreement of content. As Feyerabend sees it, according to Popper's account, testing requires only the former, agreement about sentences. A falsification, however, should require the latter (agreement of content). Two scientists using rival incommensurable theories, confronted by the "same" anomaly readily seem to agree about what it is they think that needs to be explained even if they propose deductively disjoint potential explanations of it. ("It's the tiny moving particle that never seems to stop. Explain *those* measurement results, by interpreting that data. Explain *that* phenomenon").

The problem is not that they do not realize that they disagree. The problem is the ambiguity of "*that*" and "*those*", which seem to coincide, but do not, because they interpret the same observation as describing incompatible processes with the same sentence while pointing. One encounters something strange as yet unexplained, while the other sees a stochastic motion caused by shaking molecules that make up the surrounding medium. They can see that they disagree, but in some cases, it may take some time and work (and patience) for them to realize that they are using incompatible conceptual apparatuses and exactly how due to conceptual differences in their rival theories. Bilingualism is the solution, but it can easily lead to conflations that bring on the fog if the languages are not kept separate. The fog can even be productive. Yet keeping two incompatible concep-

[394] Popper, **The Logic of Scientific Discovery...**, p. 132, ("Addendum, 1972"). See also Popper, **Objective Knowledge...**

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tual apparatus separate is exactly what physicists must learn when they train how to solve different kinds of problems with a set of ontologically mutually incompatible theories that interpret the same measurement results as different kinds of evidence. This is not always easy, given that measurements results only become empirical evidence for or against a theory if the theory predicts something about them so that the same measurement results can state two incompatible sets of evidence. For example, the evidence for the claim that sun orbits the earth and the evidence for the claim that the earth orbits the sun can be represented by the same table of measurements, but what those measurement results state depends on which theory is used to interpret them.

By now it should be very clear that. Feyerabend *intended* to argue there *can* be crucial experiments between incommensurable ("deductively disjoint") rivals without the possibility of establishing formal relations between them or any predictions deduced from them, and Feyerabend tried to illustrate how that should work with his test-model based on Brownian motion where the two theories posit incompatible ontologies, in contrast to falsificationism, which is based on formal relations between predictions, which Feyerabend argued cannot take transitions between universal theories, such as from Newton's to Einstein's theory, into account. Feyerabend clearly *tried* to criticize Popper's falsificationism, and Watkins saw this clearly. However, for a variety of significant reasons, Watkins could not see exactly how the criticism was supposed to work.

Watkins's reaction to the draft of ERE (1962) is very similar to how Popper seems to have initially reacted to Feyerabend's ERE (1962), which is also how Feyerabend seems to be, by many, still seen today. The idea of incommensurability in ERE (1962) is still seen as a confused attempt to develop Popper's methodology, instead of as an attempt to criticize it, and develop a test model in contrast to it. The next section tries to explain how these confusions led to Feyerabend arriving at his "position" on methodology in **Against Method** (1970), according to which "Anything goes!", in December 1967. In other words, when Feyerabend finally left Popper's school in December 1967, he may have felt lost, but he was taking his new "position" on methodology with him. He did not suddenly change his mind about Popper's empiricism in 1967. His epiphany was not *that* falsification was anti-progressive or anti-humanitarian. His epiphany was the realization of just how far from Popper's school he had come since ERE (1962) with respect to Popper's methodology given his criticism of it. On this reading in ERE Feyerabend

argued for the limited validity of falsificationism, which he tries to criticize, but he did not manage to make his criticism clear o Popper and Watkins. To both Popper and Watkins, ERE looked like a repetition more than a criticism. In 1965, Feyerabend's skeptical phase started when he was confronted with the poverty of general rules like falsification given their limited validity when they are not directly connected to the context of a specific problem. They are empty because you have no idea when to apply them. Then in 1967, Feyerabend realized how far he had come given that this holds not just for falsification and Popper's injunction against *ad hoc* maneuvers (and even his own realism), but for all methodological rules, concluding all general rules are empty and so "Anything goes!".

Feyerabend's criticism of falsificationism led directly to **Against Method**. Feyerabend generalized from incommensurability as a criticism of falsificationism according to which it has only a limited validity and the limited validity of his own realism which had to be given up for empirical reasons, to the view that all methodological rules can have only a limited validity, including the injunction against protecting a theory from evidence with a*d hoc* maneuvers.

2.2. Popperstein's Baby as A "Lessing Effect"

There are usually two very different sides of the story to any breakup and this case is no exception. From both perspectives, the draft of what became Feyerabend's diatribe against Popper's views in the foundations of quantum theory seems to have been the proverbial straw that broke the camel's back. [395] "Against Method" was published about three years later. As we have seen, Feyerabend seems to have suspected that with the harshness of his criticism of Popper's views on the foundations of quantum theory, he may be crossing a line (again), as he had earlier in his criticism of Kuhn. On the 4th of October 1967, he sent Popper his last letter, with a draft of his criticism of Popper's views on the foundations of quan-

[395] See Feyerabend, "On a Recent Critique of Complementarity: Part I..." and Feyerabend, "On a Recent Critique of Complementarity: Part II...". For an excellent and detailed account of Popper's side of the story, see Collodel, "Was Feyerabend a Popperian...", pp. 27–56.

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tum mechanics enclosed with it. [396] It is just one paragraph. Here is how the Feyerabend-Popper correspondence ends:

> On rereading it I find that on various occasions I have expressed myself rather harshly (remember that you had to tell me to make my Kuhn-paper a little more friendly, which I then did — but I don't think this will do any harm. At any rate I am very curious to hear what you think of it. I am also very much looking forward to seeing you again. [397]

Note the timeline, that is, this is just six weeks prior to announcing he was breaking with Popper's school because of the dispute initiated by his criticism of Popper's methodology in a draft of *ERE* in 1960. It is also worth noting that Feyerabend may have been more apprehensive about seeing Popper again than looking forward to it, which can be seen in a letter Agassi sent to Popper about a month later on the 6th of November 1967:

> I am writing because at my prompting Paul tells me he very much wants to see you, especially to hear [from] you about a paper of his, and he does not know how to approach you. He knows how busy you are, and maybe I know too, but unlike him I think he can let you judge whether or not you will see him. Now, of course, normally I should let him decide whether to request you or not. But right now I do not feel at peace with this counsel of inaction. I think you have comments on his paper which you can convey to him with no preparation and at no loss of time, and I think whatever your view is, it will be preferable all round if you tell him what it is. [398]

However, as we have seen, from Feyerabend's perspective, the conflict that ultimately drove a wedge between himself and the Popperian school concerned his criticism of Popper's methodology, which led to his awakening from his Popperian slumber into a living nightmare. Popper was notoriously initially "tolerant" of "silly" points from his students, but if after he had explained to them why the point is silly, and they continued to press the point, then they were sent from the room. [399] In this way, Popper's seminars underwrite the well-known joke that his

[396] See the draft: "On a Recent Critique of Complementarity" (KP 423.4). See Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 419.

[397] See Feyerabend's last letter to Popper, 4 October 1967 in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 419.

[398] Agassi to Popper, 6 November 1967, KP 266.26, reproduced in Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 420, fn. 4.

[399] See Horgan, "The Popper Paradox...".

book should have been called "The Open Society *by One of* Its Enemies". This context helps explain why Feyerabend and Popper never directly came to terms with Feyerabend's criticism of falsificationism based on incommensurability due to meaning variance, and why Feyerabend was clearly trying to get feedback about it indirectly through Watkins, Lakatos, and Agassi.

After the initial critical reception of **Against Method** (1975), when Feyerabend was depressed because he felt so misunderstood, at some point wishing he had never written that "damn book", he wrote a paper: "The Lessing Effect in the Philosophy of Science: Comments on Some of my Critics". [400] According to Feyerabend, the Lessing effect is a form of intellectual plagiarism involving the misappropriation of someone's ideas, and then criticizing them for holding the opposite view:

> Gotthold Ephraim Lessing, the German playwright, historian and literary critic composed a series of essays called *Rettungen* (rehabilitations) designed to save the reputation of well-known but unjustly maligned authors. His procedure was simple: he compared the rumours with the texts the authors had left behind. He found that traditional accusations which had been repeated with an air of disdain by scholars and the "educated public" alike were not just incorrect, but obviously and blatantly so - the evidence was there for all to see and it said the opposite of what appeared in the orthodox accounts. [401]

Such projections appear to be widely used in politics and among intellectuals, and among academic philosophers of science. [402] Feyerabend's critics, or so he claims, were what I have been calling "lessening" him in this way (making "less" of him, like what happened to Lessing according to Feyerabend's rendition of the "Lessing effect"). [403]

Here is Pooper's stinky reaction to ERE (1962) in a "Bibliographical Note"

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[400] Paul K. Feyerabend, "The Lessing Effect in the Philosophy of Science: Comments on Some of my Critics", *New Ideas in Psychology* 1984, Vol. 2, No. 2, pp. 127–136.

[401] Feyerabend, "The Lessing Effect in the Philosophy ...", p. 127.

[402] Seems Shameless, but it often occurs inadvertently. Sometimes because rivals use the same sentences to say incompatible things, or because of poor assumptions (such as de Brogie's assumption that von Neumann ruled out his pilot wave theory, which he explicitly did not), as perhaps was the case with Popper's lectures leading to Feyerabend's inadvertent "lessening' of Bohr in 1957 at the Colston conference.

[403] See Feyerabend, "The Lessing Effect in the Philosophy ...".

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(read "Notice") attached to the republication of his "The Aim of Science" (1957) as republished in **Objective Knowledge** in 1972, [404] breaking his own "rule" not to criticize former students in publications — later as evidenced by his ghosting Feyerabend in **The Myth of the Framework**. [405] To Feyerabend, as we are about to see, Popper was basically accusing him of stealing Popper's ideas, something Popper had been doing since the 1950s, while *ignoring* that it was presented as a criticize of Popper's views. Moreover, to Feyerabend Popper is complaining (whining?) that he is not getting enough due credit for the views of his that Feyerabend took from him and tried to criticize him with. Seriously. Notice too, there is no mention or even whiff of a suggestion, that Feyerabend was *trying* to criticize Popper's methodology, whether Popper thought Feyerabend had succeeded or not.

> The same idea of *mine* was also the "starting-point" (as he puts it on p. 92) of P. K. Feyerabend's paper "Explanation, Reduction and Empiricism" [...] whose reference [66] is to the present paper (as first published in *Ratio*, 1, 1957). *Feyerabend's acknowledgement seems to have been overlooked by the authors of various papers on related subjects*. [406]

As we shall see, in response, Feyerabend suggests that Popper seems to have overlooked that his starting point was not intended to be the conclusion. The important point for now is that Popper did not even present ERE (1962) *as an attempt* to criticize them, apparently because he do not think Feyerabend's criticism applied to his views, not that he had not tried to criticize him with them (assuming if Watkins noticed the intent to criticize so too did Popper). As we have seen, Watkins understood that Feyerabend was *claiming* to be criticizing Popper's methodology in a draft of ERE (1962) in 1960, he just could not see how the criticism actually applied to Popper's methodology. Popper advocated for meaning variance and Popper had emphasized that methodological rules are just rules of thumb. Therefore, to Popper, it must have looked like Feyerabend was "lessening" him, by adopting Popper's position that methodological rules can only ever be rules of (they can only have a limited validity) by emphasizing meaning variance

[404] See Popper, **Objective Knowledge...**

[405] Karl Popper, **The Myth of the Framework: In Defence of Science and Rationality**, M. A. Notturno (ed.) Routledge, London 1994.

[406] Popper, **Objective Knowledge...**, p. 205.

in the terms used to state rival theories, and then presenting these ideas as a criticism of Popper's own views which need to be disinfected of their empirical core. Put directly, Popper thought Feyerabend had lessened him with respect to his argument for theoretical pluralism and his criticisms of Hempel and Nagel's accounts in ERE (1962), and this only became much worse as Feyerabend took up the "position" that "Anything goes!" because of the limited validity of all methodological rules. To Popper, it seemed that Feyerabend was becoming internationally infamous by (mis-)appropriating Popper's views and then presenting them as criticism of them. In other words, with **Against Method** (1970), from Popper's perspective, Feyerabend began "lessening" him a second time.

As we shall see, the view expounded by some is that Feyerabend *only* later tried to make it look like ERE (1962) had been an attempt to criticize Popper's methodology after Feyerabend and Popper broke in late 1960s, by removing praise of and credit to Popper from the 1981 republished version in Feyerabend's collected papers. Furthermore, according to some, Feyerabend originally admitted ERE (1962) was just a repetition of Popper's methodological views, but then he later tried to hide it, by removing the passages where he had explicitly said so, doctoring his papers as it were. [407] That has become the "standard" account. To quote from Watkins, my response is "Balls". Feyerabend clearly tried to criticize Popper's methodology in the original 1962 version, and then again in 1963 and 1965 for the same reason and this led to his break with Popper in December 1967.

Here is one of Feyerabend's responses to Pooper's stinky reaction to ERE (1962) as set out in his "Bibliographic Note". In 1980, Feyerabend added a very long passage to the end of footnote 6 to the version of ERE to be republished in his first volume of collected papers in 1981. [408] Why buried at the end of footnote 6? Two reasons: First because it is attached to the passage which was supposed to identify Feyerabend's main goal as stated by the last sentence of the introduction:

> It is the purpose of the present paper to develop and to defend the outlines of such a *disinfected empiricism* (as in the original).

[407] See Preston, **Feyerabend: Philosophy...**, who first drew attention to the fact that Feyerabend had "doctored" his papers to make them look less Popperian.

[408] See Feyerabend, **Realism, Rationalism and...**

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By putting the long addition precisely here, Feyerabend is pointing out exactly how he was trying to improve empiricism by disinfecting it (of its empirical core) by criticizing and correcting Popper's account (by "disinfecting" it) not by abandoning empiricism. He wassuggesting that he was not *repeating* Popper's views, he was trying to criticize them in the original. Feyerabend is implying that the need to be disinfected was supposed to apply to Popper's views in the original. The footnote before the long addition (footnote 6) begins (also as in the original):

> As will be shown in section 2, the empiricism of the thirties was disinfected in the sense desired here. However, later on *modern empiricism* re-adopted some very undesirable principles of traditional philosophy. [409]

Does it say "logical" empiricism? That would have been Hempel, Nagel and Carnap, but not Popper. Does Feyerabend exempt Popper? No, he does not. As we have seen, it is quite clearly supposed to be a criticism of "Hempel" (explanation), and "Nagel" (reduction), and "Popper" (empiricism) (just add Dorothe's "Oh my!" and we're off to see the wizard). As we shall now see, in the text after "Added 1980", Feyerabend is clearly trying to suggest that *the modern empiricism* he was criticizing had meant to include Popper in the original.

> *Added 1980.* This paper owes much to discussions with D. Bohm, H. Feigl, S. Körner, T. Kuhn, G. Maxwell, H. Putnam, E. Tranekjaer-Rasmussen [not Popperians]. Popperians, who would gladly assert that they invented the multiplication table, if they could get away with it, have described it as a rehash of Popper's own ideas. Thus in his **Objective Knowledge** [...], Karl Popper sadly notes that after 1962 people started referring to me instead of him when discussing matters of explanation and theory comparison while Imre Lakatos proclaimed, with his usual propagandistic flair, that Popper's theses "were given wider circulation" by my paper which, he added, was "not as good (and definitely not as clear) as Popper's and Duhem's original exposition". [410]

Feyerabend rejected Popper's implication that it was plagiarism and a repetition, implying that it was *intended* as a criticism of Popper's methodology. This was not the first time Popper had accused Feyerabend of plagiarism. Lakatos had even made a list for Popper of all the passages in Feyerabend's publications with respect to which he thought Feyerabend had not adequately given Popper his due

[409] As in original ERE (1962) [emphasis added].

[410] Feyerabend, "Explanation, Reduction and Empiricism..." (1981), p. 47, fn. 6.

credit. [411] That must not have helped smooth over the tension. Popper had already (unfairly from Feyerabend's perspective) privately accused Feyerabend of plagiarism in the foundations of quantum theory with respect to his "Remark on the von Neumann proof", [412] insisting Feyerabend immediately add a note giving himself and Agassi all the credit for it. [413] Popper seems to have thought he owned the idea quantum indeterminacy is not different in kind than classical indeterminacy. [414] In any case, Feyerabend had initially agreed to write to the editors and add a note of acknowledgment to Popper and Agassi, as Popper was insisting, but then he thought better of it, and told Popper it was too late (which it was not). [415] By adopting Popper's strategy in foundations of quantum theory, however briefly, Feyerabend (so it seemed to Popper), was getting out ahead of what Popper's propensity interpretation strategy intends to show. ERE must have seemed like just another case of Feyerabend adopting his ideas as if they were his own. In any case, it was a short-lived reversal for Feyerabend, who immediately decided he had been mistaken with 1956 his remark on von Neumann's proof anyway, and returned to what he had been arguing before, that is, that von Neumann had shown that there is a special problem of quantum indeterminacy, while proposing

[411] See Collodel, "Was Feyerabend a Popperian…", pp. 27–56.

[412] See Feyerabend, "Eine Bemerkung…".

[413] See Collodel, "Was Feyerabend a Popperian…", pp. 27–56 and Collodel and Oberheim, **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper…**

[414] A close look at their correspondences shows that Feyerabend excitedly sent Popper two consecutive letters outlining the four main points of the short remark, suggesting Popper read Furth, whose complex considerations were to be replaced by more straightforward considerations in the "Remark". That is the Remarks explicit goal. These letters raise some questions about the legitimacy of Popper's accusation of plagiarism in that case. See Feyerabend to Popper, 30 January 1956 in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 217 and Feyerabend to Popper, 6 February 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 220. Feyerabend brought Fényes derivation of quantum theory based on the theory of Markoff-processes into the discussion, which is what Feyerabend used in the his "Eine Bemerkung…" (pp. 421–423) to clarify what Popper claimed to be his own point, as well as the widely used dice example used to illustrate the point in the "Remark". Note, Popper was just one of many others who used such an example to clarify their views on determinism and indeterminism. For example, Einstein had famously used the example to illustrate his objection to indeterminism, and Bohm was using the example to illustrate his views on determinism (within) and indeterminism (in the overlap between) levels (sub-quantum, quantum, and classical/relativistic level).

[415] See Collodel, "Was Feyerabend a Popperian…", pp. 27–56 and Collodel and Oberheim, **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper…**

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his own strategy to unmuck it. [416] In any case, at the time, there was already a history of Popper accusing Feyerabend of plagiarism being fueled by Popperians, so it no wonder that that is exactly how Feyerabend interpreted Popper's reaction with his "Bibliographical Note":

> Now the ideas which Popper and Lakatos claim for the Popperian school (Popper: "the same idea of *mine* [...]" **Objective Knowledge**, my italics) are neither Popper's own (as Lakatos soon realized) nor are they the ideas I had in mind when writing my paper. Popper correctly remarks that his **Aim of Science** was a "starting point" of my paper: he overlooks that it was a starting point of criticism, not of repetition: Popper, repeating Duhem, emphasizes that high level theories often conflict with established laws and thus *inspire* crucial experiments. I, on the other hand, was interested in cases where alternatives not only inspire such experiments *but are necessary for producing the corresponding evidence* (cf. the Brownian motion example in section 6 and the more detailed discussion of the case in ch. 8, and ch. 3 of *AM*; the Brownian motion example was suggested to me by David Bohm). [417]

Feyerabend, obviously, did not agree with Popper's assessment. [418] Moreover, here we see a clear indication that according to Feyerabend, ERE (1962) and **Against Method** (1975) argue the same point with respect to the Brownian motion example suggested by Bohm. Also, note Bohm was also first on the list including H. Feigl, S. Körner, T. Kuhn, G. Maxwell, H. Putnam, E. Tranekjaer-Rasmussen of those credited with helping him develop the ideas in ERE (1962) in contrast to Popperians, who Feyerabend is claiming do not seem to understand that those ideas criticize of Popper's methodology. Moreover, this note added to the end of fn. 6 in 1980 continues:

> Lakatos (**Philosophical Papers**, 1, 109, n.5) is aware of the purely psychological (or, as he calls it, "catalytic") function of alternatives in Popper's methodology, regards it as insufficient, includes me in his criticism and ascribes to himself the view I actually hold in the present paper (and explain in greater detail in ch. 6.1) viz. that "alterna-

[416] Compare Paul K. Feyerabend, "Determinismus und Quantenmechanik", *Wiener Zeitschrift für Philosophie, Psychologie, Pädagogik* 1954, Bd. 5, H. 2, pp. 89–111 and Paul K. Feyerabend, "Zur Quantentheorie der Messung", *Zeitschrift für Physik* 1957, B. 148, pp. 551–559.

[417] Feyerabend, "Explanation, Reduction and Empiricism..." (1981), p. 47, footnote 6.

[418] Compare Stefano Gattei and Joseph Agassi, "Introduction", in: Stefano Gattei and Joseph Agassi (eds.), **Paul K. Feyerabend. Physics and Philosophy. Philosophical Papers Volume 4**, Cambridge University Press, Cambridge 2016, p. xxix, fn. 38. [x-xxix].

> tives are *necessary* parts of the falsifying process". [419]

That is exactly the difference between Feyerabend and Popper, if Lakatos's "falsifying process" is replaced by an indirect "refutation process" according to Feyerabend's new test-model in ERE (1962), that is, *indirect refutation without falsification*. In other words, Feyerabend claims Lakatos "lessened" him on his criticism of Popper's falsificationism. To Feyerabend it seemed unfair that Lakatos was able to distance himself from Popper with Feyerabend's point (that Newton's theory was never, be falsified), while Feyerabend himself, according to Lakatos, was just supposed to be repeating Popper with that same point. Feyerabend then concludes by emphasizing the main points of ERE (1962) in contrast to, and as criticism of, Popper's methodology, while mocking Lakatos:

> He [Lakatos] also overlooks that the paper [ERE 1962] introduces incommensurability and ends up by arguing that explanations inevitably contain "subjective" elements. Small wonder that a paper with such *unPopperian* tendencies should look "not so clear" to an author who reads it as a repetition of Popper's repetition of Duhem. Or do Popperians claim to have invented subjectivism and incommensurability as well? [420]

Here, Feyerabend clearly implies his idea of incommensurability was not supposed to be a rehash of Popper's views, which also implies it is not supposed to be Popper's idea of *inkommensurbalität*.

As we shall soon see, this may have been at the root of the confusion. Feyerabend had concluded there is an irreducibly subjective element in knowledge, because it is always merely knowledge according to the experience as explained by some particular theory, in contrast to objective knowledge. This is so clearly "un-Popperian" because Popper went exactly the other way in developing his views, as for example in his **Objective Knowledge** (purportedly without going anywhere at all). In other words, Popper says he did not change his account from **The Logik** but just popularized and updated it in light of Quine's work on metalanguage. For some readers, one beautiful aspect of Popper's **The Logik** was lost in this way, as one of Popper's great achievements was to explain theory choice without recourse to some metaphysical notion of truth. Truth had been restricted

[419] Feyerabend, "Explanation, Reduction and Empiricism..." (1981), p. 47, fn. 6.

[420] Feyerabend, "Explanation, Reduction and Empiricism..." (1981), p. 47, fn. 6.

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to statements about particulars in Popper's methodology and was not applicable to theories.

Popperians read Feyerabend like Popper did, and apparently, some continue to do so. According to the editors of the fourth volume of Feyerabend's own collected papers in the philosophy of physics, Popper's account of their break-up is the correct one, and Feyerabend's side of the story is brushed under the rug by them, indirectly misrepresenting that break-up. There is no mention that ERE (1962) was intended as a criticism of Popper, nor any mention that Feyerabend denied and rejected Popper's accusation (that those editors so prominently display). At least, Feyerabend is owed the courtesy of them pointing out that Feyerabend says he was trying to criticize Popper's methodology (as Watkins had clearly recognized from the start), if not an explanation of why Feyerabend purportedly failed with his in his attempt to criticize Popper's methodology as they understand it. Instead, we are told that Feyerabend theoretical pluralism:

> *was but an extension* of ideas Popper had already formulated in **The Logic of Scientific Discovery** and elsewhere. In the 1962 original version of "Explanation, Reduction, and Empiricism" [...] *Feyerabend readily acknowledged this*; later he withdrew the acknowledgement — after Popper had called attention to it. See Popper, **Objective Knowledge** [...] p. 205; and Feyerabend, "Explanation, Reduction, and Empiricism", in [...] **[Philosophical Papers, Vol. 1]**, pp. 44–96: p. 47, footnote 6. [421]

Notice the "but" which means "just" in this context, and the similarity to Collodel's 2016 account, where there is also no mention even that Feyerabend's argument in ERE was intended as a criticism of Popper's account. [422] So according to the editors of Feyerabend's own collected papers, Feyerabend's theoretical pluralism in ERE (1962) was originally intended as nothing *but* an extension of Popper's views. [423] They make it look like Feyerabend accepted Popper's side of the story, without even mentioning that this is not Feyerabend's side of the story. Moreover, the passage cited by them does not support the interpretation given by them.

[421] Gattei and Agassi, "Introduction...", p. xxix, fn. 38.

[422] See Oberheim, **Feyerabend's Philosophy...**, which already argues this point and for the same reason: Feyerabend's incommensurability is incompatible with *falsificationism* (not with critical rationalism).

[423] Paul K. Feyerabend, "The Concept of Intelligibility in Modern Physics" (1948), trans. Daniel Kuby and Eric Oberheim, *Studies in the History and Philosophy of Science. Part A* 2016, Vol. 57, pp. 64–66.

Where exactly in the original ERE (1962) does Feyerabend admit his theoretical pluralism is "but an extension of ideas Popper had already formulated"? As we have seen, Feyerabend argues that Popper's methodology cannot take comparing incommensurable theories into account so it needs to be disinfected of its empirical core in 1962. (See ERE 1962!)

The note in ERE that the editors cite to support their bogus claim says exactly the opposite of what they imply. It says *he was not just repeating Popper's views with his theoretical pluralism*. [424] He was trying to criticize Popper's methodological views, and not, as they seem to be implying, admitting that he had been just repeating Popper before he removed the alleged admission. Popper's model suggests that statements should be deduced from each theory some of which are logically consistent and some of which logically contradict for a falsification. In this sense, each theory is tested by predictions deduced from it, which are then compared to the predictions deduced from its rival. Feyerabend was two models. Popper's for commensurable theories within frameworks and his own test-model for universal theories (applicable to scientific revolutions). In Feyerabend's test-model, there is only one set of statements deduced from one theory that are confirmed, corroborating the new theory, which then refutes the other theory *indirectly* (not by contradicting something deduced from it) but by undermine the ontological principles that give meanings to the terms used to state the theories when they are realistically interpreted. Brownian motion corroborated the kinetic theory according to which it is a stochastic process caused by shaking molecules. Nothing has been, nor need be, deduced from the phenomenological theory to show that if this explanation is correct, then classical phenomenological thermodynamics must be wrong, because according to that worldview there are no molecules. It follows that the one theory is wrong, not because of a logical contradiction with respect to a confirmed basic statement deduced from it, but because a confirmed prediction deduced from a rival shows that Brownian motion is a stochastic process caused by shaking molecules. This prediction could never have been deduced from the existing theory by itself, nor could the existing theory, by itself, ever have correctly described the phenomenon, nor even have discovered this fact (that they never could correctly describe it) without a deductively disjoint alternative that shows it. If falsificationism based on formal relations is a condition for the justification that a new proposal replace an existing theory,

[424] Gattei and Agassi, "Introduction…", p. xxix, fn. 38.

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then Brownian motion would be forever doomed to haunt classical phenomenological thermodynamics without ever being able to falsify it, because a rival deductively disjoint theory is needed to show what was wrong with classical phenomenological thermodynamics (its ontology), by explaining the phenomenon according to a deductively disjoint rival that implies all of the concepts of the established view have zero extension according to incompatible rules of usage of the terms used to state the rival theories.

In this sense, the established theory is out corroborated by the confirmed prediction of a novel phenomenon deduced from its rival, without a falsification based on formal relations. One theory is out corroborated by the other's deductively disjoint confirmed predictions, because they are corroborating a theory with an incompatible ontology. The predictions of one do not contradict the predictions of the other, nor the established theory. The new theory is incompatible with its rival's ontology, with the implications it has about everything. In other words, the corroboration of the new theory shows that the established theory is stated with concepts that have no extension. It has the wrong ontology. As a form of theoretical pluralism, Popper's methodology is monistic in an important sense. There is only one test-model. Falsificationism is a one size fits all account of testing competing theories with empirical evidence. Falsificationism is the one and only rule of thumb for testing theories empirically (as opposed, for example, comparing them with respect to simplicity). Here Popper cannot really hide from Feyerabend's point. Or would Popper also be willing to accept other ways of comparatively evaluating theories on the basis of empirical evidence? If so, why is there no mention of these other rules of thumb? Popper clearly implies that comparing rival theories empirically is merely a qualitative judgement without a falsification.

As we have just seen, Agassi and Gattei (2016) imply that Feyerabend removed his praise his Popper to hide his Popperian origins justifying of Popper's rebuke of Feyerabend that he deserves his due credit. They claim Feyerabend allegedly admitted he was *merely* repeating Popper in the original paper. However, while Feyerabend did repeat many points ascribed to Popper in the original, he also tried to criticize Popper. Moreover, Feyerabend had a much better excuse to remove the excessive praise of Popper. The effusive praise of Popper seems to have made Popper and Popperians (and potentially many others) misunderstand that was criticizing Popper as a "modern" or "contemporary" empiricist that shared a dogmatic element with school philosophies by pretending to have dis-

covered the one and only empirical method of science, and not just criticizing "logical" empiricit. Popper clearly seems to have thought Feyerabend was lessening him, which is what some still seem to be accusing Feyerabend of doing today. As we have seen, if there were a backward projection with respect to Feyerabend's criticism of Popper's methodology, it must have before 1965, when Feyerabend initially points out he has been trying to disinfect Popper's methodology of it empirical core in in "Problems of Empiricism" (1965).

Feyerabend did try to de-emphasize his dept to Popper and re-emphasize his debt to Bohm. For example: "Added 1980. This paper owes much to discussions with D. Bohm", [425] but on this interpretation, he did this not to change what he was trying to say, or to accept Popper's rebuke in any way. He had a better reason, so that it would be easier to see what he had really been trying all along: Popper's falsificationism fails to take deductively disjoint theories into account due to meaning variance in the terms used to state them. Notice, to turn his alleged repetition of Popper into a criticism of Popper, *nothing* had to be added or changed with respect to the content of the original criticism in ERE (1962). The criticism of Popper in the main text is still exactly the same: Falsificationism cannot take comparing deductively disjoint universal theories into account. One just had to read the original paper according to its dramatology, to see that it was intended as a criticism of falsificationism that develops a test-model in contrast to it. Feyerabend's misguided attempt to build drama by not specifying his ultimate target explicitly until the end, written in an overly complex and cryptic style, seems to have led some to overlook that Popper's empiricism was his ultimate main target in ERE (1962).

Popper was not done criticizing Feyerabend's idea of incommensurability (or rather, Popperstein's baby, his notion of it based on his misunderstanding of it). Popper implicitly tries to rebut Feyerabend's subjectivism and criticize Feyerabend's notion of incommensurability in **The Myth of the Framework** (1994). [426] Here, Popper begins by targeting explicitly what many mistook to be Kuhn's notion of incommensurable (which is not Kuhn's), which implies *incomparable*. Popper disagrees: "we must demand of any better theory [...] that it can be *compared* with [its rival]", or in other words, that "the two theories are *not* "incommensu-

[425] Feyerabend, "Explanation, Reduction and Empiricism..." (1981), p. 47, fn. 6.

[426] See Collodel, "Was Feyerabend a Popperian...", pp. 27–56.

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rable", to use a now fashionable term, introduced in this context by Thomas Kuhn". [427]

Note, Popper does not try to take credit for the idea of incommensurability in Feyerabend and Kuhn's work given his own earlier use of the term "*incommensurable*", most likely because he knows he meant something very different from it (or what it shares in common with Kuhn's view), and not the relative sizes of the sets of the respective falsifiers). He had already suggested in his early discussion of cultural relativism that:

> These differences are most important, and they must not be dismissed or shrugged off by a cultural relativism, or by the claim that different laws and customs are due to different standards, or different ways of thinking, or different conceptual frameworks, and that they are therefore incommensurable or incomparable, [428]

This also clearly implying that "incommensurable" implies or is is interchangeable with "non-comparable" for Popper. Later, he specifically turns to the use of "incommensurable" in philosophy of science (see above), but in a footnote, he also implicitly admits that Kuhn's incommensurable was not meant to imply incomparable. [429] "Nevertheless, I [Popper] regard the view discussed as influential" (in the philosophy of science). Why? Because he takes it to be widespread and to be Feyerabend's view. He does not explicitly mention Feyerabend by name, but just refers to: "Some people," meaning Feyerabend, "do not think that [his] assertions are correct". [430] In other words, Popper is acknowledging that Feyerabend has criticized his views on empirical comparability of theories. According to Popper (implicitly), Feyerabend is one of "[t]he proponents" of this view that incommensurability implies incomparability (here, the footnote attached makes it clear he does no longer mean Kuhn, who had clarified that he does not mean to imply incomparability with "incommensurability" after Popper had written the essay). Popper specifies in a note:

> By stating these logical criteria for progress, I am implicitly rejecting the fashionable (anti-rationalistic) suggestion that two different theories such as Newton's and Ein-

[427] Popper, **The Myth of the Framework...**, p. 54 [emphasis in the original].

[428] Popper, **The Myth of the Framework...**, p. 46.

[429] See Popper, **The Myth of the Framework...**, fn.19, p. 63.

[430] Popper, **The Myth of the Framework...**, p. 54.

> stein's are incommensurable. It may be true that two scientists with a verificationist attitude towards their favoured theories (Newtonian and Einsteinian physics, say) may fail to understand each other. But if their attitude is critical (as was Newton's and Einstein's) they will understand both theories, and see how they are related. [431]

This implies that Popper rejects the idea of incommensurability as a barrier to comprehensibility and/or communication that cannot be overcome. However, neither Kuhn nor Feyerabend were claiming either of these possibilities to be the case. Kuhn sometimes seems to overstate the barrier to communication initially posed by incommensurability. For example, in the Copernican revolution, rivals clearly understood exactly *how* they disagreed. Proponents of the established view did not misunderstand the new proposal. They just thought it was preposterous. Feyerabend often emphasized that in science overcoming challenges to communication posed by use of rival deductively disjoint theories is *routine*. One learns to keep them separate, that is, to use only one at a time to solve the problem at hand. Learning when to apply which theory is part of learning science.

Popper was very clear he rejected the notion of incommensurability as part of his creed: "Thus it [his creed] does not, like some other creeds, produce anything like the described »incommensurability«". [432] That Popper is trying to disagree with what Feyerabend has said about incommensurability and meaning variance seems very clear:

> But I assert that the fact that observations are theory-impregnated does not lead to incommensurability between either observations or theories. [433]

This illustrates that Popper understood that Feyerabend's incommensurability was intended to affect the theories and the observations statements deduced from them. It also illustrates that Popper thought his methodology showed how theories can be compared despite meaning variance. After all, that is what his "basic statements" are for. Notice with his use of "incommensurability" Popper clearly means incomparability, which is not Feyerabend's view. Feyerabend argued that universal theories are not comparable according to falsificationism, not that they are incomparable at all. As we have seen, Feyerabend's theoretical plu-

[431] Popper, **The Myth of the Framework...**, p. 26, fn. 37.

[432] Popper, **The Myth of the Framework...**, p. 56.

[433] Popper, **The Myth of the Framework...**, p. 58.

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ralism, based on Feyerband's test-model in contrast to falsificationism, tries to explain how. Popper writes:

> Ever since Heraclitus there have been revolutionaries who have told us that the world consists of processes, and that things are things only in appearance. In reality they are processes. This shows how critical thought can challenge and transcend a framework even if it is rooted not only in our conventional language but in our genetics — in what may be called human nature itself. Yet even this revolution does not produce a theory incommensurable with its predecessor. The very task of the revolution was to explain the old category of thing-hood by a theory of greater depth. [434]

Feyerabend's claim is that revolutionary theories do not *explain* antiquated categories in greater depth. They explain novel phenomenon that cannot be correctly described with the old categories which need to be replaced (with the observation statements that they were used to make), together with the antiquated facts that they thought they had explained, because they use the wrong concepts to try to explain them. With this, Popper clearly disagrees. The old concepts are *explained* in greater depth. His account is somehow conceptually cumulative in this way, not just cumulative with respect to successful predictions. With greater conceptual depth Popper seems to be implying we keep digging in the same hole, its just deeper, because now have a better shovel. Feyerabend is implying that in revolutions we dig a different hole somewhere else, with a different kind of tool, like a backhoe instead of the shovel that we had used to dig the last one.

Popper is very clear that for him:

> This is a further explanation of the asserted incommensurability. It is understandable that two ways of life and two ways of looking at the world are incommensurable. Yet I want to stress that two theories which try to solve the same family of problems, including their offspring (their problem children), need not be incommensurable. [435]

Popper is trying to say that even two incomparable ways of life need not be incomparable with respect to solving problems with science. His attitude seems to be that that is exactly what science is for, that is, finding an objective way to solve problems regardless of considerations about cultural differences and meaning variance. By contrast, according to Feyerabend, incommensurable means deduc-

[434] Popper, **The Myth of the Framework...**, p. 59.

[435] Popper, **The Myth of the Framework...**, pp. 55–56.

tively disjoint, but that does not make incommensurable rivals empirically *incomparable*. It makes them incomparable according to falsificationism, because falsificationism entails the consistency condition. In this way, Popper's use of "incommensurable" to imply "incomparable" erroneously implies that Feyerabend's claims about "incommensurable" imply imply that framework theories are untestable. Hence, the title of Popper's book: **The Myth of the Framework** (1994). The myth, according to Popper, is that framework theories (Feyerabend's "universal theories", Bohm's "framework theories") are untestable, because they are incommensurable (meaning, for Popper, empirically incomparable). However, as we have seen, according to Feyerabend they are empirically testable and comparable, although they have no formally comparable consequences even though they are not falsifiable just because they are incommensurable due to meaning variance. That is exactly what Feyerabend's test-model is designed to show: how there can be a crucial experiment between rival incommensurable universal theories based on the empirical evidence without the possibility of establishing formal relations between corresponding predictions.

How could Popper have so blatantly *misread* Feyerabend's ERE (1962), which clearly argues that Brownian motion was a "crucial experiment" without a falsification? Many reasons. How could he have read Feyerabend's "incommensurable" as implying incomparable, when this is so clearly not the case? Popper reads Feyerabend's "incommensurable" to imply incomparable, because he mistakenly reads it as his own "*inkommensurabel*", which means quantitatively empirically incomparable. Moreover, to do this Popper equates falsifiability with testability (which is exactly what Feyerabend is criticizing, that is, Popper pretending falsificationism is the only normative rule of thumb for empirically testing rival theories), just because his falsificationism implies that the test model for both kinds of cases (commensurable and incommensurable) should be one and the same. These combined reasons help to explain exactly how and why Popper misunderstands and misrepresents Feyerabend by interpreting him (Feyerabend) in his (Popper's) own terms, instead of Feyerabend's. In this way, Popper and Popperians seem not to have been able to set aside Popper's perspective, in order to understand Feyerabend's criticism of it. [436]

[436] This applies especially to Feyerabend's contrastive account of empirical content according to which incommensurable theories and the facts they purport to explain are part of each other's empirical content (for the Popperian reason that they are part of what they rule out), which no longer

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So Feyerabend's big reveal dramatology in ERE (1962) was a big flop among Popper and Popperians. ERE (1962) was dismissed by Popper and Popperians as a confused repetition of Popper's own views, which Feyerabend had tried to misappropriate as his own, while Feyerabend appeared to them to have the unmitigated audacity to try to present Popper's views as a criticism of Popper for holding the opposing view, as Feyerabend was well-aware that a falsification is an ideal scenario that was only just supposed to act as a rule of thumb. How very unfair this must have seemed to Popper and the Popperians! From Popper's perspective, this was clearly exactly an example of Feyerabend's "Lessing effect". As we have seen, this is clear given Feyerabend's response to Popper's (1972) reaction to ERE (1962) afterwhich Popper must have realized ERE (1962) was intended as a criticism of his views.

Popper likely read ERE (1962) and drafts of it. He seems to have misunderstood Feyerabend, taking it to be a confused repetition of his own views, but why did he read Feyerabend in this way in the first place? The answer seems clear enough. In the 1959 English translation (and after), Popper, who was notoriously picky with respect to translations of his work, had used "non-comparable" as an English translation for the German "inkommensurabel" in **The Logic**.[437] "Inkommensurabel" is a well-defined technical term thoroughly discussed in **The Logic**,[438] where it implies incomparable, which is clear as Popper consistently had it translated as "non-comparable" throughout the English translation.[439] Popper shows that from a formal point of view, the relative sizes of the sets of potential falsifiers of two general statements can only be *quantitatively* compared if one class is a *proper subclass* of the other. Otherwise, the two classes are "non-comparable" with respect to their relative degrees of testability, so that they can only be qualitatively and not quantitively compared with respect to how testable they

makes sense of Popperian terms.

[437] See Popper Popper, **Logik der Forschung...** in German and compare to Popper, **The Logic of Scientific Discovery...** in English.

[438] See Popper, **Logik der Forschung...**

[439] See Popper, **The Logic of Scientific Discovery...** See Collodel, "Was Feyerabend a Popperian…", pp. 27–56 who seems to draw the wrong inference from this similarity in terminology, i.e. that is shows the Popperian pedigree of Feyerabend's vocabulary, when Feyerabend was using it in a way that has incompatible rules of usage for that term with respect to Popper's inkommensurabel.

are. [440]

Feyerabend's definition of incommensurable and Popper's definitions of *inkommensurabel* can seem to somehow coincide, as for Feyerabend deductively disjoint theories share zero statements in common with their rivals, so they are also *inkommensurabel* in Popper's sense (one set of potential falsifiers is not a proper subset of the other's set of potential falsifiers), as for Feyerabend, taken as basic *statements*, they have no overlap at all. [441] The set of potential falsifiers of one is not a proper subset of the other. In fact, they do not share a single basic statement in common so from Popper's perspective they are not even rivals. On the other hand, they do share many of the same basic sentences. Moreover, Feyerabend illustrates incommensurable with the relation between classical mechanics and relativity theory (one of just a few of such transitions between universal theories), while Popper uses exactly this transition to illustrate a falsification, so it is hard to see how they could possibly mean the same thing by incommensurable and *inkommensurabel.* In other words, for Popper, classical mechanics and relativity are not "inkommensurabel", whereas for Feyerabend these rival theories are "incommensurable", which has nothing to do with the comparative sizes of their sets of potential falsifiers, but with the qualitative differences in their concepts due to incompatible rules of usage of the terms used to state the rival theories.

The Wittgensteinian fog can close in fast. For example, to Popperians, it seems that rivals using incommensurable theories must be trying to talk about different things (as they have no comparable consequences). But that cannot be right for Feyerabend, as the rivals are obviously trying to talk about the same things as part of the same "everything". They just use incompatible concepts to do so, that even give a different meaning to thinghood itself. The fog unfurls into a ghost like

[440] See Popper, **The Logic of Scientific Discovery...**, p. 98 and compare Popper, **Logik der Forschung...**, pp. 79–80.

[441] I have discussed this point with Matteo Collodel, but I come to the opposite conclusion. Popper's "incommensurable" simply has nothing to do with Feyerabend's incommensurable accept orthographically, as they do not apply to the same things, and they do not have the same implications, and they are not used to try to make a similar point. Newton and Einstein's theories are not "incommensurable" for Popper. For Feyerabend they are. This transition exemplifies a falsification for Popper, while incommensurability precludes formal relations necessary for a falsification according to Feyerabend because of meaning invariance there can be no falsification. So thing interesting thing is Popperstein's baby (in this case Popper seems to be confusing Feyerabend's incommensurability as a special case of Popper's "ink"), which would also contribute to an explanation of why he thought Feyerabend was lessening him.

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Popperstein's baby whenever Feyerabend's idea of incommensurable is conflated with Popper's idea of *inkommensurabel*, by equating testability with falsifiability, so that one fails to appreciate Feyerabend's criticism of falsificationism based on incommensurability. Popper's "inkommensurabel" does not even apply to concepts or theories, but to the relative sizes of sets of potential falsifiers (statements about particulars). Feyerabend, by contrast, applied "incommensurable" primarily directly to "concepts" and to the "theories" (in contrast to Kuhn's initial uses) those concepts are used to state, claiming theories determine the meaning of the predictions that can be deduced from them. [442]

In other words, Feyerabend and Popper do not even apply the term "inkommensurabel" (in Feyerabend's German publications) to the same things (sizes of sets of potential falsifiers vs. concepts and the theories they are sued to state), and Feyerabend and Popper were obviously not trying to say the same thing, as one explicitly implies incomparable and the other explicitly implies comparable. Popper's claim is about the relative degree of falsifiability (as measured by comparing the respective sizes of their classes of potential falsifiers using the subset relation) of any two theories, or in other words, *the relative sizes* of the two classes of potential falsifiers of those theories, that are not quantitatively, and thus only qualitatively, comparable because one does not predict all of the statements that can be deduced from the other. Popper's claim is not the theories themselves that are

[442] Kuhn, by contrast, did *not* use the term incommensurable or some cognate of it directly to apply to "theories" in the original **The Structure of Scientific Revolutions**, nor in stating his own views in the "Postscript" added a decade later. In total, Kuhn used some form of the term "incommensurable" (including "incommensurability", and "incommensurables") just *nine times* in **The Structure of Scientific Revolutions** as it first appeared in 1962. Kuhn applies incommensurable to (roughly) five different pairs of things, each of which is separated by a scientific revolution: to "ways of seeing the world and practicing science in it", on p. 4; to a new "normal scientific tradition" on p. 103; to a new "world of research" p. 112; again to "normal scientific traditions" on p. 148; to "standards [of what counts as science]" on p. 149; to "competing paradigms" on p. 150 (twice); and again to "paradigms" on p. 157, and lastly to "solutions" on p. 165. There are another five appearances of some form of the term in the "Postscript—1969" added to the second enlarged edition, **The Structure of Scientific Revolutions...**, pp. 174–210. Here, Kuhn uses "incommensurability" twice without specifying exactly what things are supposed to be incommensurable (both on p. 198). Twice he applies it to "viewpoints" (Kuhn, **The Structure of Scientific Revolutions...**, pp. 175 and 200). The one and only application of "incommensurable" directly to "theories" is used while stating critics *misinterpretations* of his views: "Only philosophers have seriously misconstrued the intent of these parts of my argument. A number of them [. . .] have reported that I believe the following: the proponents of incommensurable *theories* cannot communicate with each other at all" (Kuhn, **The Structure of Scientific Revolutions...**, pp. 198–199).

incommensurable or "non-comparable". It is the *sizes* of the sets of potential falsifiers of each theory that are "*inkommensurabel*" or quantitively non-comparable, when all of the predictions cannot be deduced from the other. That is why the term "non-comparable" is used in English by Popper (and not "incommensurable") to show where and how his quantitative method of theory comparison by degree of falsifiability applies, and where it does not. Popper uses the relative degree of falsifiability of any two general statements (based on the number of potential falsifiers they each have) as the basis for his account of how testable they are relative to each other with respect to deducing predictions from them, which is the foundation of his purported solutions to the problems of demarcation and induction, and to help disentangle fundamental issues in quantum theory (for example, he tries to assess Bohr's "complementarity" with respect to its potential testability).

In a 1972 addendum to **The Logik**, Popper later explicitly though it worth clarifying that: "We can compare theories with respect to testability only if at least some of the problems they are supposed to solve coincide".[443] Perhaps he added this just to clarify exactly what he had been saying given the discussions of this issue that had been raised by Feyerabend (and Kuhn), as according to Feyerabend's account, they are somehow the same problems, but only superficially, as they treat what looks like the same problem as very different problems that they purport to solve in very different ways. For Popper, to compete empirically they have to be commensurable (by sharing some of the same problems), otherwise they are empirically non-comparable. For Feyerabend the solved problems become different problems. Incommensurable rivals appear to try to solve the same problems, but actually they treat them as different and incompatible kinds of problems.

In Popper's account, there is no distinction between comparing commensurable and incommensurable theories. Popper accepts no such distinction, because theories that are *inkommnersurabel* cannot be quantitatively empirically compared, so no model is needed. That is just what *inkommnersurabel* for him means: Because one set of potential falsifiers is not a subset of the potentials falsifiers of the other, one cannot be said to be quantitively better corroborated than another. For as Popper so pedantically explains in **The Logik** when defining "inkommenurabel" to mean quantitively non-comparable with respect to the sizes of the

[443] Popper, **The Logic of Scientific Discovery...**, p. 132.

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sets of potential falsifier deducible from each theory, there are only a limited number of possibilities with. One set is either bigger than the other, or smaller than the other, or they are equal, or *the sizes* of the sets of potential falsifiers are non-comparable ("inkommensurabel"), because one is not a proper subset of the other. So, Feyerabend's ERE (1962), to Popper, seemed like a confused repetition of Popper's views, because Popper took "incommensurable" to mean *inkommensurabel, while equating* falsifiability with testability. In this way, Popper represents Feyerabend as having claimed that incommensurable universal theories that are incomparable are untestable framework theories, and that is how Popperstein's baby was born as the widespread myth that incommensurability implies incomparability, a myth that is still alive and well today in some circles, despite both Feyerabend and Kuhn's vocal denials that this was implied.

On some Popperian readings, such as Collodel's, Popper's "inkommensurabel" motivated Feyerabend's idea of incommensurability. Others, such as, Bschir, try to develop Feyerabend's theoretical pluralism as compatible with falsificationism and present Feyerabend's theoretical pluralism as an improvement of Popper's methodology, without considering how Feyerabend's theoretical pluralism was specifically designed to be a criticism of it, given that incommensurable means deductively disjoint. [444] "*Importing* the anomaly" is exactly what cannot happen, according to Feyerabend, as rival universal theories are deductively disjoint. The problems is that the anomally cannot be imported due to meaning variance and the resulting lack of formal relations. Trying to develop Feyerabend's views with this metaphor is misleading in this way. [445] Bschir follows Collodel's suggestion that "the central theoretical terms [in particular »incommensurable«] used in Feyerabend's paper have a traceable Popperian pedigree". [446] However, with "incommensurable" Feyerabend was not borrowing or following Popper's introduction of the metaphor or the term "inkommensurabel", he was following Einstein's use

[444] See Bschir, "Feyerabend and Popper on Theory Proliferation and Anomaly Import…".

[445] See Bschir, "Feyerabend and Popper on Theory Proliferation and Anomaly Import…", nor Hoyningen-Huene, "Paul Feyerabend and Thomas Kuhn…" and Hoyningen-Huene, "Three Biographies: Kuhn, Feyerabend, and Incommensurability…". In Feyerabend's words, "For such a demand would imply the demand to derive from correct premises what is false, and to incorporate what is incommensurable" (Feyerabend, "Explanation, Reduction and Empiricism…" [1962], p. 59). This is not meant to imply that Kuhn is legitimately a target of Feyerabend's criticism, just that the idea of importing an anomaly is not a legitimate reconstruction of Feyerabend's argument against Kuhn.

[446] Collodel, "Was Feyerabend a Popperian…", p. 40, and see especially fn. 31.

of "incommensurable" to mean something different as part of a criticism of falsificationism because it implies the consistency condition. In other words, while Feyerabend may seem to be using Popperian terms, he clearly is not in this case, and suggesting otherwise belies a deep misunderstanding of Feyerabend's theoretical pluralism and its relation to Popper's theoretical pluralism. The suggestion that Feyerabend was following Popper's use misdescribes how Feyerabend tried to use the term. Popper's use was precisely defined as part of a formal discussion of comparing theories by the relative sizes of the sets of their deductive consequences *quantitively* (in contrast to merely qualitatively) or by other epistemic values such as by simplicity. Compare Collodel:

> The metaphor of "incommensurability" was introduced by Popper through a precise definition at the beginning of chapter six of Popper (1935). [447]

Collodel notes that the German "inkommenurabel" was translated into English as "non-comparable" by Popper, [448] however this is disregarded, because "he meant it more literally, as can be seen by other sources". [449] As we have seen, it is quite clear what Popper meant with his formal definition, which is not metaphorical. Collodel's account is calling the kettle black with its claims about "disregarding" who was using what terminology in whose sense. [450] Feyerabend was not emulating Popper's terminology with "incommensurable", nor invoking Popper's idea of inkommensurbel. Collodel's account does not "complement" (Oberheim's **Feyerabend's Philosophy)** either, which does not recommend conflating Popper's use of "inkommensurabel" with Feyerabend's use of "incommensurable", and then conclude that Feyerabend's use does not make sense. [451] Feyerabend also

[447] Collodel, "Was Feyerabend a Popperian…", p. 40, fn. 31. This formulation seems to be an oxymoron".

[448] Compare Popper, **Logik der Forschung…**, p. 80 with Popper, **The Logic of Scientific Discovery…**, pp. 98–99.

[449] Collodel, "Was Feyerabend a Popperian…", p. 14, fn. 33.

[450] Reading Popper in English helps avoid getting trapped in the fog of conflating their ideas with respect to "incommensurable" (Feyerabend's deductively disjoint) and "inkommensurabel" (Popper's non-comparable).

[451] Oberheim, **Feyerabend's Philosophy…** traces the contextual theory of meaning in discussions of methodology to Duhem, **The Aim and Structure of Physical Theory…**, who did not use the term "incommensurable". The other uses of "incommensurable" that are considered used the same term to say something similar (a kind of incompatibility) to what Feyerabend also tried to say by applying

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tried to define the incommensurability relationship: two theories are incommensurable,

> if a new theory entails that all the concepts of the preceding theory have *zero extension* or if it introduces rules that *change the system of classes* itself. [452]

Feyerabend's definition of "incommensurable" does not match Popper's definition. It was never supposed to capture Popper's idea, nor was it intended to be metaphorical for either Popper or Feyerabend. It was supposed to mean non-comparable for Popper and to mean "deductively disjoint" *and nothing else* by Feyerabend. Feyerabend must have known that Popper had used "inkommensurabel" to mean quantitively non-comparable in this way, as he was thoroughly familiar with Popper's work. Perhaps Feyerabend was trying to be cute by causing confusion by not bothering to point this out, and it backfired (in Popper and Popperians' eyes) similar to how his big reveal targeting Popper at the end of ERE (1962) was a big flop.

On Collodel's interpretation, Feyerabend's *notion* of incommensurability is something "Popperian", something, following Popper's "metaphorical" use of the term, something even magical, as Collodel depicts Feyerabend as "A Sorcerer's Apprentice [who] Transmuted Popper's Critical Rationalism into Theoretical Pluralism and Got Cursed with Incommensurability". [453] Feyerabend was merely an apprentice to Popper who got cursed with a magical idea. This magical notion of incommensurability is something Feyerabend got "cursed" with, because it does not make sense, neither to Collodel, nor according to him. Collodel includes a commitment to a "normatively falsificationist methodology of science" in his use of "critical rationalism" (as well as "a progressive attitude in the search for truth" while trying to assess the evidence. If critical rationalism implies falsificationist method-

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the term to *concepts* (Fleck "disease" became incommensurable with "disease", Bohr "number" became incommensurable with "number", Köhler "mental' may be incommensurable with "material") or to qualities of the basic of rival universal theories (Einstein), and not to the relative *size* of the sets of potential falsifiers of rival theories to mean that those sizes are incomparable.

[452] Feyerabend, "On the »Meaning« of Scientific Terms…", p. 268.

[453] This is the title of Matteo Collodel's talk delivered at the 15th Congress of Logic, Methodology and Philosophy of Science on August 7, 2015, in Helsinki as part of the Symposium "Feyerabend Theoretical Pluralism vs. Popper's Critical Rationalism — Continuities and Ruptures". I did not see the talk, only the abstract.

ology and Popper's purportedly progressive attitude with respect to the search for truth, then Feyerabend was not a critical rationalist in ERE (1962) in either respect. Feyerabend concludes that truth and explanation should not be part of empiricism, of how theories should be tested empirically. Feyerabend is implying that because contemporary theories do not explain antiquated facts, they do not better approximate the truth. Facts and the truth of statements about them become relativized to a universal framework. This becomes increasingly clear after ERE (1962). Collodel does not even mention Feyerabend's criticism of Popper's methodology in ERE (1962). In fact, this (Collodel's suggestion that critical rationalism is synonymous with a falsificationist methodology together with other things) is the one and only time the term "falsification" or any derived of it is used by Collodel in his entire assessment of the evidence with respect to the available accounts of Feyerabend's development. It is used to specify how "critical rationalism" is used in the paper as "*synonymous* with »Popper's epistemological approach«, characterized, among other features, by [...] a progressive attitude in the search for truth, and [Popper's] normatively falsificationist methodology". [454]

Collodel suggests a more fine-grained approach would be necessary to address the relationship between their ideas, but, a more fined grained approach is not used to assess the evidence and the accounts that he criticizes. There is no coherent way to understand Feyerabend's deteriorating relationship with Popper between 1960 and 1967 without a distinction between falsificationism as a methodology and critical rationalism as a more general philosophy. In ERE (1962), Feyerabend, as we have seen, was trying to distinguish between critical rationalism as a philosophy and Popper's methodology. He endorsed one while criticizing the other for sharing a dogmatic element with school philosophies while it pretending to be *the* empirical method. [455] Feyerabend argues that Popper's view is unable to take comparing incommensurable rival theories into account, and so it stifles the very progress it purports to promote.

[454] Collodel, "Was Feyerabend a Popperian…", p. 5, fn. 5.

[455] Recall, according to Feyerabend: "for Popper the test procedure outlined is the empirical method" (Feyerabend, "Explanation, Reduction and Empiricism…" [1962], p. 74), or: "According to Popper this procedure of testing a theory by comparing it with experience, as seen in the light of alternatives, is identical with the scientific method" (Feyerabend, "Explanation, Reduction and Empiricism…" [1962], p. 73). This does not sound like Feyerabend is also adopting these views, nor do they sound like Popper if you do not add as normative rules of thumb, and so no wonder Popper thought he was being "lessened".

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Feyerabend criticized falsificationism as a normative methodology, without abandoning critical rationalism as a philosophy even after 1967, after having finally announced his departure from Popper's school, he felt completely lost and alone. [456]

In Collodel's assessment of the various accounts given the available evidence, all of the other uses of "falsification" or some derivative appear in citations of others, such as Feyerabend, or other accounts of Feyerabend (by Edgley and Agassi) or of Popper (by Slate) or of Lakatos (about Lakatos); or they are in citations of accounts of Feyerabend that Collodel is criticizing (most notably mine), with not the slightest hint that Feyerabend criticized Popper's falsificationist methodology as sharing a dogmatic element with school philosophies that needs to be disinfected of its empirical core from ERE (1962) to "Problems of Empiricism" (1965). [457] In Collodel's account, there is no distinction between critical rationalism and Popper's methodology (falsificationism), and without one, what Feyerabend says about himself in the quotes cited by Collodel and what Feyerabend published in 1962 with respect to his relation to Popper simply do not make sense. The accounts and evidence cannot be properly assessed, without recognizing that Feyerabend's argument for theoretical pluralism was developed in contrast to and as a criticism of falsificationism. To the contrary, Collodel claims:

> Feyerabend's theoretical pluralism [is] centre[d] around the idea [of] the degree of falsifiability of a given scientific theory. [458]

It is not. It is centered around the ideas of incommensurability and testability by novel corroboration in contrast to degree of falsifiability. In Collodel's account, no distinction between the maxim to increase testability and the maxim to increase falsifiability is made, nor is the distinction between direct and indirect refutation emphasized, nor the difference between Feyerabend's account of "indirect refutation" which is based on novel corroboration and Popper's which is a falsification. Equating testability and falsifiability is another openly Popperian misreading of Feyerabend. Feyerabend presented a criticism of falsificationism based

[456] Oberheim, **Feyerabend's Philosophy...** already claims that Feyerabend *criticized* falsificationism with incommensurability intentionally and how.

[457] See Collodel, "Was Feyerabend a Popperian...", pp. 27–56.

[458] Collodel, "Was Feyerabend a Popperian...", p. 15.

on the ideas that a theory can be tested by refuting its ontology, without establishing formal relation between predictions deduced from them. Collodel asks the question wrong ("Was Feyerabend a Popperian?"). He asks, does Feyerabend stay within or go "beyond critical rationalism and its methodological model of the growth of scientific knowledge". [459] If these are the same thing, there is no possibility for Feyerabend to remain a critical rationalist, while criticizing falsificationism and trying to develop a better methodological model of the growth of scientific knowledge that can take comparing incommensurable universal theories into account.

Feyerabend's model was not centered around Popper's model of the growth of scientific knowledge, if by that, "degree of falsifiability" is meant. It was intended as a criticism of Popper's falsificationism that limits its validity to testing commensurable theories by showing how theories can be indirectly refuted by a novel corroboration without a falsification, not an attempt to repeat Popper's methodology. Feyerabend's theoretical pluralism can only be developed properly in Feyerabend's terms, not Popper's. Reading Feyerabend in Popperian terms leads to misunderstanding Feyerabend's idea of incommensurability as misguided black magic sorcery that Feyerabend got stuck with, or just another version Popperstein's Baby. There are many potential problems with Feyerabend's views, but to understand those problems, it is first important to try to understand Feyerabend's views in their own terms and not in Popper's. Criticizing Feyerabend's views as failed attempts to repeat Popper is not helpful for understading them.

Feyerabend often published German versions of his books and papers, and in them "incommensurable" is translated as "inkommensurabel". [460] In these cases, Feyerabend and Popper use exactly the same term ("inkommensurabel" in German), in the same context (comparing theories empirically), while giving incompatible meanings to it. The Wittgenstein fog is ready to roll in, especially for those reading just these German versions of Feyerabend and Popper's works, where Feyerabend and Popper use the same term to mean very different things. Arguably, Feyerabend's use of "inkommensurabel" and Popper's use of "inkommen-

[459] Collodel, "Was Feyerabend a Popperian…", p. 15.

[460] See Feyerabend, **Realism, Rationalism and…** and Paul K. Feyerabend, **Ausgewählte Schriften. Band 2. Probleme des Empirismus. Schriften zur Theorie der Erklärung, der Quantentheorie und der Wissenschaftsgeschichte**, Vieweg, Wiesbaden 1981.

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surabel" are meta-incommensurable.[461] They belong to incompatible accounts of science that use incompatible basic postulates to recommend rival normative accounts of how best to make progress in science.

"Meta" is being used here in Quine's sense of a metalanguage, which is a language used to discuss languages as distinct from the "object language" it discusses. For example, the claim "»Snow is white is true« if and only if snow is white" is a statement in a metalanguage that contains "Snow is white is true" as its object language. This is often used to illustrate Tarski's definition of "truth". Tarski's definition of truth appears to have played a major role in convincing Popper that it is justifiable to say scientific progress is increased approximation to truth.[462] Feyerabend's criticism of Popper's methodology can be clearly stated with its help: Popper's falsificationism, according to Feyerabend's criticism, wrongly assumes that "snow is white is true" if and only if "snow is white" is agreed to be true. This is not the same as Quine's "»Snow is white is true« if and only if snow is [in fact] white". The former does not guarantee the later, nor the latter the former. In this way, the account developed in Feyerabend in ERE (1962) implies that facts do not exist independently of the theories used to understand them. As Feyerabend explained a year later, he was challenging the "autonomy principle", according to which, "facts exist, and are available independently of whether or not one considers alternatives to the theory to be tested".[463] Brownian motion, is the example he uses to illustrate this principle. According to Feyerabend, Brownian a new kind of fact, motion refuted phenomenological thermodynamics because a better theory (kinetic theory) showed that its ontological presuppositions were incorrect, not because new theory showed that some successful predictions deduced from it are logically consistent while others are logically inconsistent with predictions deduced from the phenomenological theory. That is how there can be crucial experiments between incommensurable rivals based on a novel corroboration without a falsification that implies new theories explain antiquated facts.

It is important to emphasize that Feyerabend is not always very clear about many of these points in ERE (1962). Most especially, sometimes Feyerabend

[461] See Eric Oberheim, "Scientific Progress and Incommensurability...", pp. 293–313.

[462] See Tambolo, "A Tale of Three Theories...".

[463] Feyerabend, "How to Be a Good Empiricist...", pp. 3–39.

seems to be inconsistent with respect to "inconsistency", by calling rival ontological principles "inconsistent" (unless it is read to mean incompatible in the way just suggested). For example, for Feyerabend, it may take a while before the "inconsistency" between "incommensurable" (deductively disjoint) rivals is discovered. For example:

> Such inconsistency will not be obvious at a glance; it will take considerable time before the incommensurability of *T* and *T'* can be demonstrated. However, as soon as this demonstration has been carried out, in the very same moment, the idiom of *T'* must be given up and replaced by the idiom of *T*. [464]

However, the "inconsistency" here cannot between ontological principles or the theories or the statements deduced from them, none of which can be formally consistent or inconsistent, if they follow from "deductively disjoint" theories. In ERE (1962), Feyerabend tries to make it very clear where the disagreement between incommensurable theories lies:

> To bring about the situation described above (sets of mutually incommensurable concepts), it is sufficient that they govern the use of the main terms of *T'*. In such a case *T'* is formulated in an idiom some of whose implicit rules of usage are inconsistent with *T* (or with some consequences of *T* in the domain where *T'* is successful). [465]

Feyerabend is trying to suggest that rivals may not initially understand how they disagree (because they continue to use the same terms in the same contexts), but that once they realize that they are using incommensurable theories that implicitly have *inconsistent rules of usage*, they have to use the idiom of new theory to understand how they disagree and why they are incompatible. To see Feyerabend's point about meaning variance and the facts as expressed in everyday language is to realize the possibility that there could be a pseudo-agreement posing as a logical consistency in the form of a single sentence that makes two different statements, depending on which of two deductively disjoint theories are used to interpret the sentence because of inconsistent rules governing its correct use so that progress looks cumulative with respect to empirical content through revolution when it is not. For example, to take one of Feyerabend's oft repeated colorful illustrations of a covert conceptual change that may take time to correct with re-

[464] Feyerabend, ERE (1962), p. 75.

[465] Feyerabend, ERE (1962), p. 75.

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spect to its use in everyday language, [466] if "white" is interpreted according to two deductively disjoint theories, then "Snow is white" does not always logically contradict "Snow is not white", as one sentence may take "white" (or whiteness) to be a property of an object, while the other takes it to be a property of a relation that varies with relative motion between an object and an observer (given the red shift). In this case, "Snow is white" and "Snow is not white" do not logically contradict because the term "white" does not have the same implications. One is seduced into thinking they formally contradict because they look like they do, just like it only looks like evidence is transitive through revolution, but it is not (compare Kuhn on the invisibility of revolutions in **Structure**).

Feyerabend's explicitly simplified summary of his position in his "How to Be a Good Empiricist" (1963) can make it look just like Feyerabend merely repeated Popper's position in ERE (1962): [467] "the methodological unit to which we must refer when discussing questions of test and empirical content is constituted by a whole set of partly overlapping, factually adequate, but mutually inconsistent theories". [468] According to some interpretations: "Here we may find, *in nuce*, Feyerabend's pluralistic test-model" in his early philosophy. [469] However, this paper is explicitly a *simplified* summary, and it does not even the term contain "incommensurable", just "irreducible", just as the paper glosses over the distinction between inconsistent predictions deduced from a theory and inconsistent rules of usage (or incompatible principles) that give the terms used to state the theories their meaning. Perhaps Feyerabend does not even use "incommensurable" due to all the confusion he had caused by using "incommensurable" in a different way than Popper's "inkomensurabel" and Kuhn's "incommensurable", but he surely did not abandon the idea or change it with respect to as to whether he was using it to criticize Popper's methodology, which continued to need to be disinfected of its empirical core. For this reason, Feyerabend can appear to have backed-off (at least for the moment) with respect to Popper's methodology, but this is clearly not the case, given that according to "Problems of Empiricism" (1965) Popper's methodology needs to be disinfected for the same reason, so that it can cope with mean-

[466] See for example Feyerabend, "An Attempt at a Realistic Interpretation...".

[467] See for example, Gattei and Agassi, "Introduction...", pp. xxviii-xxix.

[468] Feyerabend, "How to Be a Good Empiricist...", pp. 22–23.

[469] Gattei and Agassi, "Introduction...", p. xviii.

ing variance in the terms used to state the theories, as illustrated by the same example and the same test-model, regardless as to whether he called the rival theories "incommensurable" or in his 1963 publication not.

Feyerabend did not explicitly exempt Popper's methodology from his criticism in his 1963 simplified summary. In fact, quite to the contrary, this time Feyerabend makes clear that his criticism applies to Popper's methodology (as part of contemporary empiricism) right at the start of developing the two conditions he challenges (consistency condition and meaning invariance):

> One of the cornerstones of contemporary empiricism is its theory of explanation. This theory is an elaboration of some simple and very plausible ideas first proposed by Popper. [470]

In this way, as in ERE (1962), Hempel's account, which is explicitly criticized, is presented as elaboration of Popper's methodological views. This allows Feyerabend to criticize Hempel in lieu of Popper, as his criticism of Hempel's account should then also apply to Popper's views too. Feyerabend claims that these conditions are "restrictive conditions and therefore bound profoundly to influence the growth of knowledge". [471] This is how Feyerabend characterizes his point that they will impede progress. Feyerabend also points out that: "This is how the consistency condition is used in arguments against theories such as those of Bohm, de Broglie and Vigier" (p. 84), making it very clear how his argument is connected to the events unfolding in foundations of quantum theory at the time. [472] Popper's methodological views seems to lend credence to dismissing Bohm's proposal because it does not falsify the existing theory, so that it is merely *ad hoc*.

Moreover, the suggestion that Feyerabend may have backed-off with respect to his criticism of Popper's methodology between ERE (1962) and his awakening in December 1967, is clearly also incorrect. Feyerabend explicitly makes it very clear that Feyerabend's argument was supposed to disinfect Popper's methodol-

[470] Feyerabend, "How to Be a Good Empiricist…", p. 9.

[471] Feyerabend, "How to Be a Good Empiricist…", p. 10.

[472] On this connection, see especially Marij van Strien, "Pluralism and Anarchism in Quantum Physics: Paul Feyerabend's Writings on Quantum Physics in Relation to His General Philosophy of Science", *Studies in History and Philosophy of Science Part A* 2020, Vol. 80, pp. 72–81.

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ogy of its empirical core again in "Problems of Empiricism" (1965): [473]

> The idea that a theoretical pluralism should be the basis of knowledge can be found both in the dialectical philosophy of Bohm and in Popper's critical rationalism. However, it seems to me that it is only within the framework of the latter that it can be developed without undue restrictions. Such development is bound to eliminate whatever remainder of an empirical "core" may still be contained in Popper's point of view. [474]

Here is clear evidence that Feyerabend was trying to criticize Popper's methodological views within a critical rationalist framework. Feyerabend is claiming that critical rationalism should be developed without "undue restrictions", such as the consistency condition, which implies the condition of meaning invariance, and this is bound to undermine Popper's account of formal relation between predictions deduced from incommensurable rivals. This is why Popper's account needs to be disinfected of its empirical core. This is why Popper's test-model needs to be replaced by his proposal for comparing rival universal theories that are incommensurable. Of course, it becomes ever clearer in his subsequent papers after 1967 that Popper's methodology is still a target of his criticism in exactly this same way. In any case, while it is true that in ERE (1962), Popper's position is presented and praised as "a model in which we make use of at least two factually adequate but mutually *inconsistent* theories", [475] according to Feyerabend in ERE (1962), read carefully:

> It will turn out that many test procedures presuppose the existence of a class of mutually *incompatible*, but factually adequate, theories. [476]

This is Feyerabend's theoretical pluralism (based on incompatibility) in contrast to Popper's theoretical pluralism (based on inconsistency) in a nutshell. The rendition of Feyerabend's theoretical pluralism in "How to be a Good Empiricist"

[473] See Feyerabend, "Problems of Empiricism...", which Feyerabend claimed "is based on results that [Feyerabend] explained in the following earlier essays: »Explanation, Reduction, and Empiricism«; »Problems of Microphysics«; »A Note on Linguistic Philosophy and the Mind-Body Problem«; »An Attempt at a Realistic Interpretation of Experience«; »Complementarity«", Feyerabend, "Problems of Empiricism...", p. 153.

[474] Feyerabend, "Problems of Empiricism...", p. 153.

[475] Feyerabend, ERE (1962), p. 30.

[476] Feyerabend, ERE (1962), p. 50.

is not, in a nutshell, Feyerabend's theoretical pluralism as illustrated by Brownian motion and his test-model throughout the rest of his philosophy of science. It is a simplified version. For Feyerabend, universal theories are not formally consistent, they are *incompatible* because they are incommensurable ("deductively disjoint"). In Feyerabend's test-model, if there is meaning variance then neither the theories nor their predictions can be formally consistent (or inconsistent). They are incommensurable (deductively disjoint), because they are based on *incompatible* ontological principles that make incompatible claims about "everything" which appear to be "inconsistent" because they are based on inconsistent rules of usage, but which mean something different by "everything" and so are not formally inconsistent. Feyerabend sometimes seems to call the ontological principle "inconsistent", other times "incompatible". A close, reading shows what he is trying to say. The rules of usage of the main descriptive terms used to state the theories are not consistent. If the theories are deductively disjoint according to a realistic interpretation of their ontology, then so too, it would seem to follow, are the ontological principles used to give them meaning. For example, given a contextual theory of meaning, rival incommensurable universal principles only appear to be contradicting about "everything" (for example, whether it is determined or not), but because they give incompatible interpretations of it (of "everything"), so that they cannot formally contradict with respect to it. This fits naturally with Feyerabend's suggestion that during revolutions, ontological principles should be replaced, not because they are formally inconsistent with new ones, but because they are part of ontologically incompatible worldviews — you cannot coherently believe both at the same time. So when there is meaning variance, inconsistent rules governing the correct use of the same terms is precisely what precludes formal inconsistency between rivals theories or any statements deduced from them.

2.3 From the Limited Validity of Falsificationism to "Anything Goes!"

As Popperstein's baby grew (misunderstanding of Feyerabend's criticism of Popper's methodology as an extension of falsificationism), so to spread the myth of the incomparability interpretation of incommensurability — that "incommensurable" implies empirically "incomparable" in Feyerabend's work, culminating in

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Popper's 1994 account of the myth of the untestable framework. [477] Feyerabend (and Kuhn) spent decades trying to clarify that specific Wittgensteinian fog by debunking this decidedly Popperian myth. But no matter how hard they tried, there were always new purported sightings of the mythical monster, just like in Loch Ness. The purportedly "Popperian pedigree" of Feyerabend's use of the term "incommensurable" is one of the foggiest sightings to date, perpetuating the myth that Feyerabend was following Popper's use of the metaphor of "inkommensurabilität". The suggestion that Feyerabend's "incommensurable" is somehow following Popper's "inkommensurabel" is belied by Feyerabend's test-model of crucial experiments between incommensurable rivals based on Brownian motion in ERE (1962). Feyerabend, as we have seen, was trying to disinfect empiricism, not abandon it completely, as Popper's myth of the empirical incomparability of framework theories interpretation of Feyerabend would seem to imply. Incommensurable theories have no formally comparable consequences with respect to what can be deduced from them, but according to Feyerabend, they can still be tested with respect to the ontological principles that give the terms used to state them their meanings when the theory is realistically interpreted as a potential explanation of experience.

This section tries to shows how, in this foggy context, the *final formative step* in Feyerabend's philosophical development took place. The transition from the conclusion of ERE (1962) that falsificationism has only a limited validity to the more general conclusion that all methodological rules have a limited validity, *even as* normative *rules of thumb*. This is reflected in Feyerabend's point about the potential permissibility of *ad hoc* hypotheses (highlighted with reference to Ehrenhaft, see the postscript to part 3), which Feyerabend thinks should be able to be used to defend theories in some circumstances (for example to buy time while one tries to develop a non-*ad hoc* explanation), so that even Popper's admonish against protecting theories from falsification with *ad hoc* hypotheses has a limited validity as a methodological rule. Recall, Feyerabend uses this example to illustrate his realization of just how far he had come from Popper's methodology (according to which a hypothesis should not be protected by *ad hoc* hypotheses according to Lakatos's rendition of it), while watching Lakatos lecture Popper's methodological views to students (see Feyerabend's awakening section, 2.1 above).

[477] See Popper, **The Myth of the Framework...**

Feyerabend's final formative step with respect to the views set out in **Against Method** continues to be widely misunderstood because of the fog, but this is the moment (while watching Lakatos lecture) it appears to have happened. It is often taken to have transpired further down the road, and for different reasons, and is not connected directly to the views Feyerabend developed in ERE (1962), except for by my account since 2006, which takes ERE to be the moment Feyerabend revealed that his views on methodology do not fit within a falsificationist framework with the idea of incommensurability. In publications, Feyerabend did not explicitly argue that there cannot be any universally applicable methodological rules at all until "Against Method" (1970). [478] However, Feyerabend arrived at this conclusion in December 1967, as it becomes clearly visible in the letter Feyerabend sent to Watkins in early January of 1968. In his early work, Feyerabend had been trying to disinfect empiricism (Popper's included), explicitly stating, as we have seen, that the purpose of his theoretical pluralism was to develop and to defend the outlines of such a disinfected empiricism.

Feyerabend announced his rejection of Popper's methodological views in his letters to Lakatos and Watkins on 17 December 1967, even in some of the exact same terms and phrases:

> My "position" now is *thoroughly* Popperian: Karl always starts his lectures on scientific method (he did so in 1952, when I listened to him in London; he did so again in 1962 in Berkeley) with the remark that "there is no scientific method". It is this remark which I accept, and I reject everything which comes afterwards. [479]

With "thoroughly" Feyerabend was obviously being somewhat facetious, as he is saying that he rejects everything else Popper's says after his first famous remark in his lecture. Feyerabend makes it quite clear on 17 December 1967 that

> The only theoretical restriction (or "definition") of science which I am prepared to tolerate is what follows from a principle of general hedonism: *all* those elements of science which are inconsistent with hedonism *must go* [...] Here Karl has stopped midway between Plato and his own Open Society. [480]

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[478] Feyerabend, "Against Method. Outline of an Anarchistic Theory of Knowledge...".

[479] Feyerabend to Lakatos, 17 December 1967, in: Lakatos and Feyerabend, **For and Against Method...**, pp. 120–121 [emphasis added].

[480] Feyerabend to Lakatos, 17 December 1967, in: Lakatos and Feyerabend, **For and Against Method...**, pp. 120–121 [emphasis added]. This was insightfully presented by Eilenberger in his

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We are quickly approaching Feyerabend's infamous "anything goes". Feyerabend is suggesting that Popper's philosophy is too authoritarian and consequently antihumanitarian because it is not progressive, because it puts undue restrictions on the acceptability of new proposals.

Feyerabend is challenging falsificationism as *the* normative methodological rule of thumb, claiming to have shown one should not always try to falsify the existing theory by proposing a better one that logically contradicts some of its predictions while also correctly predicting those it gets right. Rather, sometimes one should try to refute the established theory by undermining its underlying ontological principles by establishing a new kind of fact with an incommensurable rival that the established theory cannot possibly explain because it has the wrong ontology, and not by establishing formal relations between deductively disjoint rivals. A crucial experiment between incommensurable theories shows why the ontological principles of the established view need to be replaced. The result is not a falsification of the existing theory. The result is that all of the *sentences* used to state the successful predictions that had been deduced from the existing theory have to be re-interpreted according to the new theory and the new ontological principles used to interpret it (realistically) into new statements that are deducible from the new theory. Feyerabend's argument for theoretical pluralism does not involve any universally valid methodological rules, even as mere rules of thumb. In ERE (1962), as we have seen, there were at least *two* ways to justify replacing an established theory. By contrast, according to Feyerabend in ERE (1962), for Popper a single "procedure of testing a theory by comparing it with experience, as seen in the light of alternatives [i.e. falsificationism], is identical with the scientific method". [481]

It is important to emphasize that Feyerabend is not denying that for Popper falsificationism is just a rule of thumb, which Popper emphasizes. His disagreement with Popper is just that Popper's methodology seems to suggest otherwise with respect to the justification of replacing established theories with new proposals, because falsification is the *only* normatively justifiable rule of thumb for how best to justify when to replace an established theory with a new proposal on the basis of empirical evidence in Popper's account of testing theories. In this

talk.

[481] Feyerabend, ERE (1962), p. 73.

sense, Popper's normative methodology is based on a single methodological rule of thumb: As a rule of thumb, you should always try to falsify the existing theory, if you want to have the best empirical justification for replacing it. Feyerabend's idea of the limited validity of methodological rules implies that Popper's single rule of thumb is not the only rule of thumb, because it *cannot* take incommensurable theories empirically into account, not just that it does not always work in every situation. It is just a normative ideal.

Moreover, Feyerabend's test-model can also only be a rule of thumb, both in Popper's sense (it is not always going to work in every circumstance, maybe the simpler will be more fruitful than the most empirically successful quantitatively at some point in some specific case, and so on) and in the sense that for Feyerabend there are two possible strategies for testing theories, and so two possible test-models, and so neither can be universally valid even as mere rules of thumb that are not always going to work given other historical contingencies. Now the question arises naturally how can we know when to recommend which strategy to apply? It seems clear only time will tell. According to the interpretation suggested here, it is exactly this step from the limited validity of falsificationism in ERE (1962) to the limited validity of all methodological rules (anything goes!) that Feyerabend made when he awoke in December 1967. This marks Feyerabend's break with Popper's school, after which he felt out on the streets alone until he found a new home in Mill's philosophy. [482] In this way, a main conclusion of (1962) was Feyerabend's first step toward the door, while his generalization of that main conclusion of ERE (1962) in 1967 was the last step out the door to his new "position" in **Against Method**, according to which "Anything goes!".

Popper's account of empiricism, according to which there is only *one* valid methodological rule of thumb with respect to testing theories empirically (falsification), does not leave room for Feyerabend's test-model, nor can it address the question that having two test-models naturally raises. In this way, Feyerabend's subjectivist account of scientific justification and his relativism with respect to facts are at odds with Popper's account of *objective* knowledge. Perhaps Feyerabend needed to move out partly because as a young father what he thought was beautiful unfortunately was seen as "Popperstein's baby". He was searching for a healthy environment for his idea of incommensurable worldviews to grow, not

[482] See Collodel, "Was Feyerabend a Popperian…", pp. 27–56, for more details concerning Feyerabend's subsequent move toward Mill.

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as Popperstein's baby but as a criticism of Popper's methodology as it was originally intended. Feyerabend's ideas make little to no sense from a Popperian perspective. Witness the confusion about Feyerabend's seemingly incoherent contrastive account of empirical content that takes deductively disjoint statements to be part of a theories empirical content, if they have been ruled out because they are incompatible with the ontological principle used to interpret the new more comprehensive theory. However, the methodological implications developed in ERE (1962) based on the spooky Wittgensteinian idea of the same sentences making different statements and the deductive disjointedness of the worldviews that rival universal theories support, quite clearly for Feyerabend, implied that falsificationism can have only a limited validity and this is what led straight to "Anything goes!". In his next letter to Watkins (January 1968), Feyerabend returned to his criticism of Popper's empiricism:

> Some people will say: "Because experience is the foundation [the testing ground] of our knowledge". This for me means being too sure of experimental statements, more sure than a Popperian should be. Question: is it true as I suspect that Popperians at LSE are more sure of experience than Popperians should be? [483]

Feyerabend is trying to get Watkins to admit that Popper's empiricism should be disinfected of its empirical core, as he has been arguing since ERE (1962)? And then in that same letter sent to Watkins, we find the first instance of "anything goes" in Feyerabend's writings:

> Popperianism, I say, is the most reasonable philosophy in existence. "So don't try to stamp it out" you [Watkins] say, "try to improve it". Well, that is just what I am not inclined to do (and now I am again not capable of giving arguments, just a mood — but there is nothing wrong with this if one admits it). Why not? I really don't know. Besides, if I am correct in what I said in my lectures and in some of my arguments, then the improved method just says: *"Anything goes* except those things which are inconsistent with hedonism" and so from there we then wander off to the discussion of hedonism. There is only one methodology: how to keep misery away. The rest we can and must live without. (More about this in my Anti-Kuhn paper which will be rewritten with the title "Against Method" and will contain Kuhn as a subsection, Popper as another. That should at last give you some of the arguments you want *plus lots of guesses*). [484]

[483] Feyerabend to Watkins, January 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

The one method Feyerabend is referring to is, of course, "anything goes" as initially set out in his published work in 1970:

> Some of my friends have chided me for elevating a statement such as "anything goes" into a fundamental principle of epistemology. They did not notice that I was joking. Theories of knowledge as I conceive them develop, like everything else. We find new principles, we *abandon* old ones. Now there are some people who will accept an epistemology only if it has some stability, or "rationality" as they are pleased to express themselves. Weil, they can have such an epistemology, and "anything goes" will be its only principle. [485]

In the letter Feyerabend sent to Lakatos on the same day as the Watkins letter, also announcing his break from the Popperian school for the same reasons, sometimes in almost the same terms (compare his criticism of Popper's methodology for stopping halfway between Plato and the Open Society), Feyerabend also explains his step from ERE (1962) to "Against Method" (1970). He begins to hint at it in the body of the letter.

> My "position" now is thoroughly Popperian: [486] Karl always starts his lectures on scientific method (he did so in 1952, when I listened to him in London; he did so again in 1962 in Berkeley) with the remark that "there is no scientific method". It is *this* remark which I accept — and I reject everything which comes afterwards. The only theoretical restriction (or "definition") of science which I am prepared to tolerate is what follows from a principle of *general hedonism:* all those elements of science which are inconsistent with hedonism *must go*. [487]

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[484] Feyerabend to Watkins, January 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...** [emphasis added]. Feyerabend appears to be referring to the fact that he had left Watkin's guessing who the target of Feyerabend, ERE (1962) had been, having failed to clarify how his criticism applies to Popper.

[485] Feyerabend, **Against Method...**, p. 105, fn. 38 [emphasis added]. This is excepted from a footnote attached to the first instance of "Anything goes!" in Feyerabend's published work. The footnote begins with a citation from Lenin's **Left Wing Communism**, which expresses more or less the same idea, i.e., that there are no universally valid methodological rules. The note proceeds to qualify and severely restrict Feyerabend's endorsements to some very limited aspects of the ideas of Marx, Lenin, and Mao (insults Popper in passing); and then states the passage quoted.

[486] Feyerabend made it clear why he put "position" in air quotes: "When I started my lectures I hope I made it clear that they would [...] not [be] a systematic presentation of a new PKF mythology". Feyerabend to Wakins, January 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

[487] Feyerabend to Lakatos 17 December 1967, in: Lakatos and Feyerabend, **For and Against Method...**, pp. 120–121.

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And finally, in the postscript to Watkins letter, Feyerabend explicitly emphasizes how his criticism of Popper's methodology in ERE 1962 led him to the title of his new "position »Against Method«":

> *P.S. My paper, of course, will *no longer be an anti-Kuhn paper,* but it will be *an independent piece* that will touch on Kuhn only *as a side issue* (though it will contain almost all the material from my original Kuhn paper) [...] and the title will be "Against Method" (this in analogy to Susan Sontag's *Against Interpretation).* If you could lure Karl [Popper] into commenting upon it I would be eternally grateful. (This is pure curiosity on my part). But I wonder how *you* will get out of my onslaught. I haven't got a clue. Have you? [488]

Evidently, Feyerabend, who had gradually changed how he taught falsificationism, had recently explained his criticism of Popper's methodology to his students in his lecture on Popper's empiricism, and Lakatos was there: "So, can you let me have some criticism of what you heard in my lecture? What did you make of it? Was there anything in it?". [489] This appears to have been the first time Feyerabend presented his criticism of Popper's views to Lakatos, and Feyerabend clearly seemed eager for feedback on it from Lakatos, just as he had tried to engage Watkins in discussions of his criticism of Popper's empiricism. Feyerabend admitted to Watkins that he was not quite sure why he is so completely rejecting Popper's methodological views rather than trying to correct them: "Why not [try to improve it]? I really don't know". [490] But he clearly knew that he was trying to break free from Popper on methodology, and why.

Postscript. Einstein's "Unscrupulous Opportunism" as a Lessing Effect 2.0

Popper had emphasized the limited validity of falsificationism to Feyerabend,

[488] Feyerabend to Lakatos, 17 December 1967, in: Lakatos and Feyerabend, **For and Against Method...**, p. 125.

[489] Feyerabend to Lakatos, 17 December 1967, in: Lakatos and Feyerabend, **For and Against Method...**, p. 121.

[490] Feyerabend to Watkins, early January 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

insisting it is are just a rule of thumb, that there is no "the" scientific method that is always going to work in every case. Sometimes the smartest thing to do leads to failure, just as the worst rational choice might lead to unexpected, even unprecedented, advance. In other words, for Popper, what you should do is not always going to be the most fruitful thing to do given the relevant evidence and the theoretical possibilities available, and other considerations (like simplicity) at any given time. Or in other words, according to falsificationism, sometimes an unwarranted decision about what to believe given what one should believe based on the evidence at any given time might result in more fruitful progress than would have been made that if the normatively speaking "correct" decision about which theory to believe to be true would have been made. In this sense, methodological rules were only ever to supposed to have a limited validity *for Popper*. For Feyerabend this is also the case. There can be no guarantee that the most rational course, will be the most fruitful, but incommensurable proposals will have a bigger pay-off they are successful.

On the interpretation of ERE (1962) proposed in this essay, Feyerabend's conception of the limited validity of methodological rules is different than Popper's. Its origins trace way back to 1948, around the time Feyerabend and Popper first met. In 1948, Feyerabend used the same basic two-pronged argumentative strategy as in ERE (1962), and throughout his philosophy of science, to challenge Schrödinger's suggestion that intelligibility should be a universal validity methodological ideal. [491] Feyerabend argued against Schrödinger that "Anschaulichkeit" [intelligibility/visualizability], as a normative methodological ideal (like Popper's falsificationism as a normative ideal), is not universally valid, but not for Popper's rule of thumb reason. Feyerabend, as was his penchant, launched a much more radical argument. He argued that even as a normative ideal, "Anschaulichkeit" *had to have been* suspended by some pioneers of quantum theory at least temporarily

[491] Although Schrödinger's paper is not explicitly identified, nor is Schrödinger's name even mentioned, Feyerabend's essay is clearly an unsigned response to Schrödinger's in "Die Besonderheit des Weltbilds der Naturwissenschaft" [The distinctiveness of the scientific world view], in *Acta Physica Austriaca*. For an English translation, see Erwin Schrödinger, "On the Peculiarity of the Scientific World-View", in: Erwin Schrödinger (ed.), **What is Life? And other Scientific Essays**, Doubleday Anchor Books, New York 1956, pp. 178–228. See Feyerabend, "The Concept of Intelligibility in Modern Physics..."; Daniel Kuby, "Feyerabend's »The Concept of Intelligibility in Modern Physics« (1948)", *Studies in History and Philosophy of Science. Part A* 2016, Vol. 57, pp. 57–63.

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for them to make the progress that they did. [492] It was not universally valid as a methodological rule historically speaking, and it is not even something as a rule of thumb that should be used to as a guide because it puts restraints on the development of new theories that may only be able to become intelligible later normatively speaking.

Of course, from Popper's perspective, it seemed like Feyerabend was stealing *his* idea that methodological rules only have a limited validity because they are only rules of thumb, which was something he had been emphasizing to Feyerabend and many others long before ERE (1962), and Feyerabend knew it. [493] Popper makes no distinction between his views and Feyerabend's in ERE (1962), because he reads them as an unclear repetition of his own views, perhaps in part because he conflates his "*inkommensurabel"* with Feyerabend's "incommensurable". In this way, from Popper's perspective, with **Against Method**, Feyerabend is lessening him for the second time. First in 1962 Feyerabend made a name for himself in philosophy of science by "lessening" him on "inkommensurabel" while misappropriating his criticism of Nagel, and then in the 1970s Feyerabend had the audacity to pull a lessening 2.0, making a name for himself as an international best-selling author in philosophy, by misappropriating Popper's again ideas (methodological rules only have a limited validity, they are just rules of thumb) while pretending to criticize Popper for holding the opposite view. This is how Feyerabend seemed to be trying to turn himself from a disciple of Popper's methodology to chief critic of Popper's methodology without changing is his views or even saying anything new. So, while Feyerabend becomes increasingly adamant, and at some point downright vicious in his published criticisms of Popper overtime, to Popper and Popperians in some ways it just looks more and more like Feyerabend is getting even more Popperian from ERE (1962) to **Against Method** (1970) and beyond, by misappropriating Popper's ideas on a much grander scale as **Against Method** became part of a "Lessing effect" 2.0.

Feyerabend infamously defended his epistemological anarchism, according to which there is no fixed scientific method (Recall, "Anything goes!") by citing Einstein:

[492] See Feyerabend, "The Concept of Intelligibility in Modern Physics…", p. 4.

[493] See Collodel, "Was Feyerabend a Popperian…", p. 25.

> the external conditions, which are set for [the scientist] by the facts of experience, do not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist. [494]

In this way, Feyerabend was following Einstein's suggestion both times Popper thought Feyerabend had "lessened" him, by misappropriating his ideas while presenting them as arguments against his own views, first in ERE (1962) with "incommensurable", and then again with **Against Method** in a "Lessing effect" 2.0.

In sum, as we have seen, several crucial distinctions are glossed over in standard Popperian reconstructions of Feyerabend's ideas that are needed to understand the evidence given various rival the accounts. Many figures made substantial contributions to shaping the development of Feyerabend's peculiar views. One basic distinction crucial for understanding Feyerabend's early philosophy is between falsificationism as a methodology that Feyerabend criticized and critical rationalism as a philosophy that acted as a philosophical framework within which he was working. That framework was committed to fallibilism (not falsificationism), the maxim to try to increase testability (not falsifiability) and a progressive attitude toward science. This distinction is necessary to understand the senses in which Feyerabend was and was not a Popperian from the early 1960s until he announced his break from Popper's school in December 1967. Even after that, all the way through to **Against Method** (1993), Feyerabend's theoretical pluralism is still critical rationalist in many important respects. For example, his contrastive account of empirical content, while incoherent within a falsificationist framework, is still critical rationalist in spirit as it tries to show how to increase testability, just as his test-model, in contrast to falsificationism, is still critical rationalist in spirit because it is aimed at showing how incommensurable rivals increase the indirect testability of established views by novel corroboration.

Another crucial insight in Feyerabend's account can be traced back to Carnap's distinction between the act of the interpretation of an observation sentence and the act of the re-interpretation of it as part of a causal (materialist), pragmatic account of observation sentences. Feyerabend put that together with Wittgenstein's contextual theory of meaning of observation statements so that theoretical

[494] Einstein, "Remarks Concerning the Essays Brought Together…, p. 684, see also Feyerabend, **Against Method…** (1975), pp. 10–11 and Feyerabend, ERE (1962).

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context determines the meaning of the reinterpretation of an observation sentence, so that using incommensurable theories results in a situation in which a single sentence makes two incompatible statements (like the duck/rabbit in the *Investigations*). [495] Einstein's distinction between the challenges posed to testing "universal theories" in contrast to lower-level laws and generalizations and his use of "incommensurable" to describe those challenges in a way he could explicitly not further explain also played a crucial role in the genesis of ERE (1962), especially its terminology. Wittgenstein's account of genuinely philosophical problems was used to criticize Wittgenstein's quietism, by showing legitimate role "that clarifying them can play in the process of progress according to his test-model, which involves a resolution of an ontological dispute on empirical grounds. The dissolution of the philosophical fog can follow from the genuine corroboration of a novel phenomenon (Brownian motion) that only refutes the existing theory by showing it has been wrong about everything, without requiring formal relations between corresponding predictions or rival theories. Moreover, according to Feyerabend proponents of the established theory could never have found out that Brownian motion refutes classical phenomenological thermodynamics without the development of a deductively disjoint rival that explains the phenomena with incompatible concepts. That is why incommensurable alternatives should always be welcome. They can make new evidence available that is otherwise unavailable, which can refute the established theory by challenging its ontological principles "by introducing new rules for the correct use of the terms used to state and test those theories.

There is just one more piece of the puzzle to put in place: David Bohm's oversized role in all of this.

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[495] Kuhn, **The Structure of Scientific Revolutions...**, p. 114 also explicitly mentions the duck-rabbit gestalt switch. The figure was created by an anonymous illustrator in late 19th Century Germany and later popularized by Joseph Jastrow (**Fact and Fable in Psychology,** Houghton, Mifflin and Co, Boston 1900). See John Donaldson, "The Duck-Rabbit Ambiguous Figure", in: Fiona Macpherson (ed.), **The Illusions Index** 2016, https://tiny.pl/jtdb-64w. The phenomena of being able to switch between seeing incompatible, or mutually exclusive, *Gestalten* while looking at something had been scientifically studied much earlier. See, for example, Louis Albert Necker, "Observations on Some Remarkable Optical Phaenomena Seen in Switzerland; and on an Optical Phaenomenon which Occurs on Viewing a Figure of a Crystal or Geometrical Solid", *The London and Edinburgh Philosophical Magazine and Journal of Science. Third Series* 1832, Vol. 1, No. 5, pp. 336 [329–337]. Note that the two complete forms are made up of sub-forms, some of which are shared. For example, while the sub-form of the duck's beak can be seen as a different sub-form (a rabbit's ears), both share the same sub-form for eye and neck.

Part 3. Bohm's Influence on Feyerabend's Break with Popper's Falsificationism

A complete account of Bohm's influence on Feyerabend's philosophy would far exceed the scope of this essay. Even a complete account of the relationship of Bohm's developing ideas and Feyerabend's ideas up until ERE (1962) would far exceed our scope. As we shall see, in this period (the late 1950s and early 1960s), with respect to methodology, Bohm was transitioning from his "infinite levels" phase, to his "qualitative infinity of nature" methodology, and then on to his call for a new conceptual order in physics. After his experience with Feyerabend in Bristol, from the early 1960s onwards, Bohm actively worked on developing new concepts (a new conceptual framework) for all of physics that would make quantum theory and relativity compatible, by trying to develop new topological concepts that could be used to capture what they share in common: the idea of inseparability manifest in relativity theory as the inseparability of motion and framework and the idea of inseparability manifest in quantum theory as the inseparability of observer from the system observed. [496]

With respect to the foundations of quantum theory, and quantum indeterminacy more particularly, Bohm was extremely influential on Feyerabend's developing views. However, Bohm's relation to Feyerabend had gone largely unexplored until quite recently. In **Feyerabend's Philosophy** (2006), I stressed Feyerabend's enormous debt to Bohm, as Feyerabend credited Bohm with the Brownian motion example that became the backbone of Feyerabend's argument for theoretical pluralism from ERE (1962) through **Against Method** (1993).

Collodel criticizes my account of Bohm's influence on Feyerabend's formative development. According to Collodel:

Oberheim's emphasis on Bohm's influence on Feyerabend's alleged critical reap-

[496] IF I understand Bohm's suggestion correctly (which is a big IF), his insight is based on the idea that topological concepts describe properties like location in relation to other things that causally interact with them (including the observer) as an inseparable part of a framework that *co-constitutes* it, instead of in relation to an independent and inert Cartesian coordinate system that exists independently of the objects and their interactions within it. He wanted to try to revolutionize the basic concepts used in basic statements that would be universally applicable (across all domains and levels) in a way similar to what Einstein had done with respect to motion and framework and Bohr had done with respect to the observer and the system observed.

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> praisal of Popper's critical rationalism already in the early 1960s [...] *can hardly be reconciled* with such a conspicuous passage from the mid-1960s [...], which is however ignored by Oberheim. [497]

The reconciliation is a simple and straightforward distinction between critical rationalism as a framework philosophy and falsificationism as a methodology, which is quite clearly what Feyerabend was trying to say in ERE (1962). Collodel is focusing on the wrong contrast (Popper's critical rationalism versus Bohm's dialectics) and so he misses the point. The point is not that Bohm deeply influenced Feyerabend's views on critical rationalism in the early 1960s. They did not. The point is that Bohm deeply influenced Feyerabend's views on falsificationism in the late 1950s and early 1960s. Feyerabend did not substantially change his views on critical rationalism even after he awoke from his Popperian slumber in December 1967. He sought and then found a new home for them in Mill's work. On the other hand, Bohm was trying to challenge (Feyerabend and Popper's) *falsificationism*, and that is where Bohm's deep influence on Feyerabend's ideas lies, and that is where Bohm's deep influence on Feyerabend's relation to Popper and his school lies. For consider, Collodel bases his challenge to my account on the following passage cited from Feyerabend in 1965:

> [t]he idea that a theoretical pluralism should be the basis of knowledge c[ould] be found both in the dialectical philosophy of Bohm and in Popper's "critical rationalism", yet 'it seem[ed] to [Feyerabend] that it [was] only within the framework of the latter that it c[ould] be developed without undue restrictions. [498]

This is in not evidence against the deep influence Bohm had on Feyerabend's theoretical pluralism which can clearly be seen in ERE (1962). Moreover, there is no mention of *Feyerabend's very next sentence* (perhaps see below) — due to the Popperian blinders he seems to be wearing due to conflating critical rationalism with falsificationism, which misleadingly makes it seem to him like every time Feyerabend praises critical rationalism, he is also praising Popper's methodological views. There is no emphasis by Collodel on the fact that Feyerabend had been trying to criticize Popper's methodology since ERE (1962) as anti-progressive because Collodel rejects that reading of it, nor does Collodel recognize that it was

[497] Collodel, "Was Feyerabend a Popperian...", p. 39, fn. 28 [emphasis added].

[498] Collodel cites Feyerabend, "Problems of Empiricism...", p. 153 and Feyerabend, "Professor Bohm's Philosophy of Nature...".

this criticism of Popper's methodology that developed into the reason Feyerabend gave to the Popperian school to explain why he was breaking from it, which Collodel also presents very differenty. There is no mention by Collodel that when he announced his break from Popper's school in December 1967, Feyerabend explicitly says it was because he rejected Popper's methodological views (which he had been criticizing since ERE), although Collodel cites from the letter exact that clearly says so to emphasize the tension between Popper and Feyerabend at the time was largely due to Feyerabend's criticism of Popper's propensity interpretation instead. Without a distinction between critical rationalism and falsificationism, due to taking falsificationism as part of the meaning of critical rationalism, the evidence is unclear and leads Collodel to the wrong conclusions, because the question is unclear (see Bohm on learning how to frame the right questions below). [499] Collodel's conflation of critical rationalism and falsificationism is extremely instructive in this respect.

Nevertheless, Feyerabend's criticism was explicitly prominently presented in the conclusion of ERE (1962) in a big reveal of Popper as his ultimate main target in ERE (1962) revealing whose empiricism needs to be disinfected of its empirical core. This interpretation is further corroborated in publications after ERE (1962), but not before it, and before his 1967 awakening, and is also very explicit. (And now for a mini-reveal ...). It is also the very next sentence that Collodel does not present, when challenging my account:

> However, it seems to me that it is only within the framework of the latter [critical rationalism and not Bohm's dialectics] that it can be developed without undue restrictions [i.e. in ERE (1962)]. *Such development is bound to eliminate whatever remainder of an empirical "core" may still be contained in Popper's point of view*. [500]

This clearly corroborates the interpretation of ERE (1962) as a criticism of Popper's methodology from within a critical rationalist framework intended to eliminate any remaining empirical core in Popper's methodology (more specifically, the meaning invariance implied by falsification based on formal relations) as developed in Part 1 of this essay. Feyerabend was trying to disinfect *Popper's em-*

[499] Collodel seems to be aware of the problem, and has emphasized the need for a more fine grained approach to critical rationalism, but does not seem to apply this to his assessment of the evidence, nor to the various accounts that he compares and criticizes.

[500] See Feyerabend, "Problems of Empiricism...", p. 153.

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piricism of its empirical core. He says so in ERE (1962) in a big reveal, and he repeats the point more straightforwardly in 1965. The evidence clearly inidcates Feyerabend was trying to criticize Popper's methodology for the same reason and in the same way in 1965 as in ERE (1962) about two years before awakening from his Popperian slumber in December 1967. Feyerabend used critical rationalism to criticize Popper's falsificationism and, as we shall see, Feyerabend developed Bohm's ideas to do it, and that is how Feyerabend ended up waking into a living in nightmare in December 1967.

Collodel claims that:

> Bohm's position started to play an explicit role in Feyerabend's criticism of Popper's philosophy of science only from the late 1960s.[501]

This is simply incorrect, and it is incorrect for the same reason as his view of Feyerabend's break with Popper's school is misguided. Once again, by conflating critical rationalism with Popper's methodology, Collodel is mischaracterizing the evidence, addressing the wrong issue (critical rationalism), and so coming to the wrong conclusions. Collodel is absolutely correct that by taking his approach of treating critical rationalism and falsificationism as synonymous his proposals are very limited with respect the philosophical issues. But it is equally impossible to make sense of the historical issues.

As we shall see, Bohm deeply influenced the development of Feyerabend's criticism of Popper's methodology by the early 1960s. Feyerabend developed Bohm's example and Bohm's argument for pluralism and more tolerance with respect to his 1954 proposal (with Vigier) into a criticism of Popper's methodology in ERE (1962) in a way that Bohm had suggested and this criticism is what eventually led to Feyerabend's break with the Popperian school in 1967.

The evidence Collodel provides for his assessments of the accounts he evaluates merely supports the view that Feyerabend still openly preferred critical rationalism (as a philosophy) over Bohm's dialectical philosophy in the mid-1960s. This was never in contention, and this is not the correct contrast with respect to Feyerabend's deteriorating relationship to Popper. Bohm influenced Feyerabend's criticism of Popper's *methodology* by the early 1960s, which is where Feyerabend's enormous debt to Bohm lies, which can be seen in ERE (1962) and after,

[501] See Collodel, "Was Feyerabend a Popperian…", p. 39, fn. 28.

and this dispute with Popper's methodology was the reason Feyerabend gave, when announcing his breaking from the Popperian school to Lakatos and Watkins in December 1967. As we shall see, the ideas and arguments Feyerabend developed *with Bohm* as a criticism of Popper's methodology became the backbone of Feyerabend's theoretical pluralism as it initially appeared in ERE (1962). Feyerabend did not retract or withdraw his criticism of Popper's methodology between 1960 and 1967. He repeated it. He did however retract some of his criticisms of Bohm's philosophy in the mid-1960s, [502] while continuing to work within a critical rationalist framework (clearly emphasizing fallibilism and the maxim to try to increase testability; with conjectures and novel corroborations test-model). But he did not give up critical rationalism. Fortunately, much of the story of Bohm's influence on Feyerabend has recently been exceptionally well told by Marij van Strien. [503] However, Bohm's influence on Feyerabend specifically with respect to Feyerabend's criticism of Popper's methodology is a tale that has not yet been told, and for that tale some context will be helpful. First, a basic overview, before going into some details in the subsequent sections.

Directly after obtaining his Ph.D. in Vienna in late 1951, Feyerabend applied for a British Council scholarship to study the foundations of quantum theory in London for the academic years 1952–53 under Popper's supervision. In London, in addition to attending Popper's seminar, Feyerabend "concentrated on two topics: quantum theory (von Neumann and Bohm), and Wittgenstein. [504] Von Neumann was not easy; I worked my way through his book, page by page, and eventually wrote a critical review". [505] Popper, who was developing his own realistic, in-

[502] See especially Paul K. Feyerabend, "Dialectical Materialism and the Quantum Theory", *Slavic Review* 1966, V. 25, No. 3, pp. 414–417.

[503] On Bohm and Feyerabend, see especially van Strien, "Pluralism and Anarchism in Quantum Physics…"; See also Marij van Strien, "The Challenge of Quantum Mechanics to the Rationality of Science: Philosophers of Science on Bohr", *International Studies in the Philosophy of Science* 2023, https://doi.org/10.1080/02698595.2023.2207404; and Marij van Strien, "Why Bohm Was Never a Determinist", in: Andrea Oldofredi (ed.), **Guiding Waves in Quantum Mechanics: 100 Years of de Broglie-Bohm Pilot-Wave Theory**, Oxford University Press 2024 (forthcoming).

[504] See Feyerabend, "Eine Bemerkung…", pp. 421–423 and Feyerabend, "Wittgenstein's **Philosophical Investigations…**

[505] Feyerabend, **Killing Time…,** pp. 92–93 (see also Feyerabend, "Eine Bemerkung…", pp. 421–423 and Feyerabend, "Wittgenstein's **Philosophical Investigations…**). Feyerabend is referring to his review of von Neumann's book **Mathematical Foundations of Quantum Mechanics**, trans. Robert T. Beyer, Princeton University Press, Princeton 1955), see Paul K. Feyerabend, "**Mathematical Founda-**

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deterministic interpretation, [506] had been praising Bohm in the early 1950s: "*most* of the younger physicists have been brought up to believe that [...] all that matters is the formalism and its application". [507] But fortunately, "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)". [508] 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". [509]

Bohm's 1952 publications became famous as having established the possibility of a deterministic interpretation of the quantum theory, but Bohm, and this is not what Bohm was trying to show with his 1952 hidden variables proposal, which was intended (with Einstein encouraging him to this end) to provoke a renewal of discussion in the foundations of quantum theory that had gone dormant do to the hegemony of the Copenhagen interpretation. Bohm regretted the deterministic implications of his 1952 publications. "Bohm was not a Bohmian" in this sense. [510] However, Feyerabend's "*Determinismus und Quantenmechanik*" (1954)

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tions of Quantum Mechanics. By John von Neumann [book review]", *The British Journal for the Philosophy of Science* 1958, Vol. 8, No. 32, pp. 343–347.

[506] Popper was developing and defending "Indeterminacy in Quantum-Mechanics and in Classical Physics", see Karl R. Popper, "Indeterminism in Quantum Physics and in Classical Physics. Part I", *The British Journal for the Philosophy of Science* 1950, Vol. 1, No. 2, pp. 117–133 and Karl R. Popper, "Indeterminism in Quantum Physics and in Classical Physics. Part II", *The British Journal for the Philosophy of Science* 1950, Vol. 1, No. 3, pp. 173–195.

[507] Karl Popper, "A Defence of Free Thinking in Quantum Theory (1953)", in: Matteo Collodel and Eric Oberheim (eds.), **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper,** *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Venice — Berlin 2024, p. 468 [429–474].

[508] See Popper, "A Defence of Free Thinking in Quantum Theory (1953)...", p. 472, fn. 47. Popper's is referring to Bohm (David Bohm,**Quantum Theory**, Prentice Hall, New York 1951). Bohm wrote this textbook on the standard interpretation of quantum mechanics to better understand what he thought was wrong with it.

[509] Popper, "A Defence of Free Thinking in Quantum Theory (1953)...", p. 459.

[510] See Flavio Del Santo and Gerd Christian Krizek, "Against the »Nightmare of a Mechanically Determined Universe«: Why Bohm Was Never a Bohmian", *Arxiv. History and Philosophy of Physics* 2023, https://tiny.pl/yc_vzyyr, pp. 1–28 [06.08.2025]; van Strien, "Why Bohm Was Never a Determinist...".

reads as a criticism of Bohm 1952 hidden variables proposal, [511] as it argues that determinism is an outdate ontological prejudice — like locality — which is the kind of antiquated ontological principle that our best theories should be understood to show need to be replaced according to Feyerabend. [512]

Bohm and Feyerabend became close colleagues just a few months after the infamous Colston conference, in August 1957, when Bohm moved to the University of Bristol, where Feyerabend had been a lecturer in philosophy of science since 1955. [513] Feyerabend and Bohm appear to have bonded over their shared dislike for Maurice Pryce, who was head of the department of physics. [514] Feyerabend championed Bohm, as an ostracized outsider, over the establishment. For his part, Bohm appears to have found a kindred spirit in Feyerabend, "Discussing philosophy with [Feyerabend] made his otherwise intolerable situation in Bristol more tolerable". [515] Bohm's unorthodox views became a major topic of discussion among Feyerabend and Popper, and Bohm's a position at the University of Bristol in 1957–58 provided a substantial new member to Feyerabend's rather limited discussion group in Bristol. [516] As we shall see, however, in this period Bohm was trying to criticize Popper's methodology, although he does not seem to have un-

[511] See Feyerabend, "Determinismus und Quantenmechanik…"; and David Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables I", *Physical Review* 1952, Vol. 85, pp. 166–179, https://doi.org/10.1103/PhysRev.85.166 and David Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables II", *Physical Review* 1952, Vol. 85, pp. 180–193 https://doi.org/10.1103/PhysRev.85.180.

[512] See especially Feyerabend, "Physik und Ontologie…".

[513] On Feyerabend and Bohm, see Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", pp. 59–422. Issues in the foundations of quantum theory are often discussed in Feyerabend and Popper's correspondence.

[514] Feyerabend wanted to perform experiments, perhaps to test ontological principles he was discussing with Popper, but he was too afraid to ask Pryce to let him into the lab: "Yesterday I had a private discussion with Pryce, the physicist here. He listens to my lectures. I did not yet dare to ask him for allowance to perform some experiment in his laboratory — what I would very much like to do" (Feyerabend to Popper, 6 February 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 219). Hence, Feyerabend's "plea for tolerance in matters epistemological".

[515] Maurice Wilkins, "Interview of David Bohm, January 30, 1987" *Niels Bohr Library & Archives*, American Institute of Physics, College Park, MD USA, https://tiny.pl/1r-cqvwd [15.10.2025].

[516] On Bohm's interpretation of quantum theory, see Jammer, **The Philosophy of Quantum Mechanics…**, Chapter 7.

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derstood some central aspects of it until much later, as for example when he wrote a chapter on falsificationism in his **The Special Theory of Relativity** (1966).

Feyerabend brought Bohm in contact with Popper's work and acted as a go between, bringing Popper to Bohm, while helping chauffeur Bohm into professional philosophy of science: [517]

> *Would it be possible for you to send proofs of your book + especially also of the chapter containing your criticism of determinism. Bohm is discussing his contribution with me and I really think that he should read what you have to say on the matter*. Can you suggest any way in which this can be done? I gave Bohm all things I have from you, **Open Society**, "Three Views...", the Determinism-paper, your **Scientific Autobiography**, he read it, but he did not get on with the **Logik der Forschung** and gave it up as it is in German.[518]

Note that at the time Bohm had not read Popper's **The Logik** which had not yet been published in English. And Feyerabend also brought Bohm to Popper: "I am enclosing a MS by Bohm which contains his more recent ideas on hidden variables and which he asked me to send to you and to Joske". [519]

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[517] Feyerabend retrospectively recounted how he had sat in a car stuck in fog over three hours discussing philosophy with Bohm (and Agassi, and Roy Edgley, who was Popper's assistant): "Dave's knowledge of Ryle and Wittgenstein was not very detailed but he had an amazing ability of getting the whole picture from a few hints. After that he criticized the whole picture, not just a few embroideries here and there as is the custom of many philosophers" (Feyerabend to Peat in 1993, in: Collodel and Oberheim**, Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**).

[518] Feyerabend to Popper, 15 December 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 271 [emphasis added]. This explains Bohm's somewhat limited understanding of falsificationism as based on direct refutation in David Bohm, "On Certain Epistemological and Cosmological Problems Raised in Dr. Feyerabend's Review of Causality and chance in Modern Physics (1959)", in: Matteo Collodel and Eric Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical empiricism, Bohm and Kuhn**, *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Berlin 2024, pp. 239–256; David Bohm, "On the Relationship between Methodology in Scientific Research and the Content of Scientific Knowledge", *British Journal for the Philosophy of Science* 1961, Vol. 12, No. 46, pp. 103–116 and David Bohm, "On the Problem of Truth and Understanding in Science", in: Mario Bunge (ed.), **The Critical Approach to Science and Philosophy in Honor of Karl R. Popper,** The Free Press of Glencoe, New York 1964, pp. 212–223 in the lead up to Bohm's chapter "Falsificationism" in Bohm's book David Bohm, **The Special Theory of Relativity**, W. A. Benjamin, New York 1966.

[519] Feyerabend to Popper, 1 May 1958, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 280.

Bohm's influence on Feyerabend's developing ideas up to 1962 is a saga of its own. The main goal of this part of the essay is merely to demonstrate the oversized role Bohm and his ideas played in the genesis of ERE (1962) and Feyerabend's criticism of Popper's methodology, which is what eventually led to his awakening in December 1967, when he announced he was leaving the Popperian school. On the interpretation suggested, Bohm and Feyerabend should be seen as working together, and ERE (1962) represents the fruits of this collaboration for Feyerabend — just as Bohm's ensuing quest for a new conceptual order in physics is best understood with respect to Bohm's maturing methodological views as developed through his interaction with Feyerabend and his ideas on meaning variance and methodology which began at the Colston conference in 1957. What happened after they parted ways is also a long story, parts of which still seem almost completely unexplored. For our purposes, it will be enough to examine Feyerabend's enormous debt to Bohm with respect to the genesis of ERE (1962) as an attempt to criticize Popper's methodology resulting in a new test-model for incommensurable theories that became the backbone of Feyerabend's argument for theoretical pluralism. This will provide a framework for understand their (Feyerabend, Bohm and Popper's) interactions on methodology and in light of the situation in the foundations of quantum theory. [520]

Some of the potential influences Feyerabend may have had on Bohm will be indicated along the way. For example, after meeting Feyerabend and becoming close discussion partners in Bristol, Bohm noticeably began to publish on methodology in philosophy of science journals, for the first time publicly reaching out directly to professional philosophers of science (such as Popper and Feyerabend). [521] Feyerabend's relation to Bohm and Popper eventually led to Bohm's

[520] Bohm's remarks on methodology in his early papers, and in Bohm, **Causality and Chance...** and especially at the 1957 Colston conference depict a sophisticated methodological view that deserves its own more detailed reconstruction. For more on Bohm, Bohr and Feyerabend in this periods, see especially van Strien, "Pluralism and Anarchism in Quantum Physics..."; van Strien, "The Challenge of Quantum Mechanics...; van Strien, "Why Bohm Was Never a Determinist...".

[521] On Bohm's maturing methodological views after Bohm's **Causality and Chance...**, see and compare for example the progression, Bohm, "On Certain Epistemological and Cosmological Problems..."; Bohm, "On the Relationship between Methodology...", pp. 103–116; Bohm, "On the Problem of Truth...", pp. 212–223; chapter 6 of Bohm, **The Special Theory of Relativity...**; David Bohm, "On the Relationship of Science and Art", in: **DATA Directions in Art, Theory and Aesthetics**, Faber and Faber, London 1968. There is quite some irony here, given that Feyerabend's experience with von Weizsäcker in 1965 led to his view of professional philosophy of science as a little-known form of

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chapter on falsificationism in his book **The Special Theory of Relativity** (1966). Given the early repercussions of Bohm's loose association with Marxism (he had to flee from McCarthy), it seems natural to assume that Bohm would have been reluctant to delve into a new philosophical school of any kind, let alone take Feyerabend (given his background) as an explicit comrade in arms. Bohm's views were dialectical, and Feyerabend rejected them for that reason (they make everything rational seem real, and they seem to posit some sort of historical necessity into a contingent process).

However, Feyerabend was trying to defend Bohm's physics from Rosenberg's censorship, and from the ostracism of Bohm by the Copenhagen school more generally, and to promote Bohm and Vigier's 1954 proposal as exactly the kind of new revolutionary theory his test-model was designed to be able to take into account in contrast to falsificationism. Bohm's methodology was designed to support Bohm's strategy in physics, more sepcifically what Bohm had called one of "two kinds of Brownian motion" in **Causality and Chance in Modern Physics** (1957), which Feyerabend read excitedly shortly before the conference and his meeting Bohm (as can be seen by letters Feyerabend had sent to Popper asking if he had seen it, while explaining his initial reactions to it). The second kind of Brownian motion, for Bohm in **Causality and Chance** is quantum indeterminacy. In other words, quanta behave like Brownian particles for the same reason that Brownian particles act like Brownian particles. Quanta are bombarded stochastically from all sides by the elements of the sub-quantum "fluid" in which they are suspended. [522] This was a different publication than the infamous first step Bohm made in 1952, which was a "pilot" "pilot-wave" theory, that was not completely deterministic within his framework of levels, but clearly had deterministic implications taken outside of that context. Bohm immediately his proposals deterministic implications, and was trying to correct this misunderstanding with this publication with Vigier in 1954, which announced their intention to replace quantum theory. [523] Bohm's contribution to the Colston conference reports on progress

insanity (because it is completely detached from its subject). See Feyerabend, "Die Wissenschaftstheorie...", pp. 88–124.

[522] See David Bohm and J.P. Viger, "Model of the Causal Interpretation of Quantum Theory in Terms of a Fluid with Irregular Fluctuations", *Physical Review Journals* 1954, Vol. 96, pp. 208–216.

[523] See Bohm and Vigier, "Model of the Causal Interpretation of Quantum Theory...".

made on the 1954 proposal, specifically that they had recovered the results of the predictions that can be deduced Einstein and Dirac's equations. [524]

As we shall see, in the discussions at the Colston conference, Feyerabend explicitly tried to clarify, if Bohm and Vigier had meant that they have shown they can deduce those equations, or that they can deduce the same quantitative predictions that can be deduced from those equations. Bohm responded that they can recover the predictions and proposes that their theory will eventually show that the quantum theory is a limiting case of their theory, similar to the case of the relation between classical and relativity theory.

The story of Bohm's influence on the genesis of ERE (1962) begins in 1952 in Copenhagen. But like most good stories, there is also a backstory, which in this case reaches all the way back to 1947, when Feyerabend was still trying to become a physicist, and was working as Ehrenhaft's assistant in Vienna. It is a veritable saga of mix-ups, misunderstandings, ambiguities, and dramas, with Feyerabend at the last minute (before his lectures on the foundations of quantum theory) switching to Popper's side and hastily publishing a "A Remark on von Neumann's Proof" in 1956, [525] only to be accused of plagiarism by Popper, before immediately reverting to his earlier view. Our story is centered around the events that took place at the Colston conference in 1957, [526] when Feyerabend found out he had inadvertently been "lessening" Bohr. As we are about to see, Popper had been promoting two myths, first that Bohm had disproved von Neumann's proof as a striking refutation of the complementarity view, and second, the so-called "myth of the Copenhagen interpretation". Shortly thereafter, Feyerabend and Bohm became close colleagues at Bristol in 1957. In this period, [527] Bohm pub-

[524] See David Bohm, "A Proposed Explanation of Quantum Theory in Terms of Hidden Variables at a Sub-Quantum-Mechanical Level", in: S. Körner (ed.), **Observation and Interpretation: A Symposium of Philosophers and Physicists**, Butterworths, London 1957.

[525] Feyerabend, "Eine Bemerkung…", pp. 421–423.

[526] On the 1957 Colton conference, see especially Boris Kožnjak, "The Missing History of Bohm's Hidden Variables Theory: The Ninth Symposium of the Colston Research Society, Bristol, 1957", *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 2018, Vol. 62, pp.85–97.

[527] All of Bohm's most cited papers in physics were from 1949 or the 1950s, Bohm's work on both plasma and his hidden variables continue in an upward trend, see Olival Freire Junior, **David Bohm: A Life Dedicated to Understanding the Quantum World**, Springer Nature Switzerland, Cham 2019, pp. 352, 354, and 256. Bohm's 1952 papers have seen increased interest since the ap-

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lished two of his most cited papers in physics: his 1957 paper with Aharonov: "Discussion of Experimental Proof for the Paradox of Einstein, Rosen, and Podolsky", [528] and his paper in 1959: "Significance of Electromagnetic Potentials in the Quantum Theory". [529]

Popper has already been criticized for propagating the "myth of the Copenhagen Interpretation", [530] however, he also propagated the specific myth that von Neumann's proof had demonstrated that *any hidden variables account is impossible until Bohm had shown otherwise*. [531] Popper, at the time, 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". [532] 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". [533]

However, as Feyerabend points out in his first publication on quantum theory, von Neumann's proof is addressed to a specific class of hidden variable theories of

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pearance of Sheldon Goldstein's Bohmian mechanics, (a slightly modified presentation of Bohm's early ideas) following the increased interest in quantum information in the mid-1990s, see Freire Junior, **David Bohm...**, p. 356.

[528] David Bohm and Y. Aharonov, "Discussion of Experimental Proof for the Paradox of Einstein, Rosen, and Podolsky", *Physical Review* 1957, Vol. 108, pp. 1070–1076, https://doi.org/10.1103/PhysRev.108.1070.

[529] See Bohm and Aharonov, "Discussion of Experimental Proof for the Paradox of Einstein..." and Y. Aharonov and David Bohm, "Significance of Electromagnetic Potentials in the Quantum Theory", *Physical Review* 1959, Vol. 115, pp. 485–491, https://doi.org/10.1103/PhysRev.115.485. With the latter, they showed that "an electron beam travelling in a region free from any electrical field but near a solenoid where a magnetic field is confined may still undergo influence due to the variation in the confined magnetic field [and] started an industry devoted to discussing its theoretical and experimental implications" (Freire Junior, **David Bohm...**, p. 357).

[530] See Howard, "Who Invented the »Copenhagen Interpretation«?...".

[531] 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 below.

[532] Popper, "A Defence of Free Thinking in Quantum Theory (1953)...", p. 459. See Feyerabend, **Killing Time...**

[533] Popper, "A Defence of Free Thinking in Quantum Theory (1953)...", p. 472 fn. 33.

which Bohm's was not one. [534] On this, both von Neumann and Bohm agreed. [535] 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. [536] De Broglie seems to have started the general misunderstanding, Popper's included. By 1958, Feyerabend clearly seems to have thought so: "In this way von Neumann's book has created a good deal of confusion among physicists (given de Broglie's statement in the Introduction to "*La physique quantique restera-t-elle indéterministe*) *and, naturally, among philosophers*". [537] It seems more than likely that this last remark was directed at Popper, as Feyerabend had argued that this is not the case in "*Determinismus und Quantenmechanik*" with respect to von Neumann's proof and Bohm's hidden variables. [538]

With respect to Bohr, however, Feyerabend (to his great embarrassment) seems to have fallen for the myth that Bohr belonged to the Copenhagen school, ("lessening" Bohr) in front of the physicists at the Colston conference in 1957. After the conference, Feyerabend wrote to Popper:

Do you know that Bohr was extremely critical of both Heisenberg's and von Neu-

[534] See Feyerabend, "Determinismus und Quantenmechanik…".

[535] On Dennis Dieks, "Von Neumann's Impossibility Proof: Mathematics in the Service of Rhetorics", *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 2017, Vol. 60, pp. 136–148, https://doi.org/10.1016/j.shpsb.2017.01.008 account, the standard story that proponents of the Copenhagen interpretation dismissed hidden variables out of hand given their belief that von Neumann had shown that hidden variables approaches are impossible (as Popper's account in Popper 1953 seems to suggest) has it the wrong way around. Generally speaking, it was the proponents of hidden variables proposals who were using the alleged failure of von Neumann's proof to try to discredit the Copenhagen interpretation's rejection of their views (when in fact von Neumann's proof never pretended to do that in the first place). As we shall see, according to Feyerabend, initially, the Copenhagen school reacted by hiding behind von Neumann's proof without being able to explain it (but *not* Bohr himself, who never invoked von Nuemann's proof in this way, and who had left the room). However, by 1957, as Feyerabend makes clear in a letter to Popper reporting on the Colston conference, Bohm and Vigier often brought up von Neumann's alleged failure at inappropriate times. This would seem to relieve the tension between the two accounts by making both partly true. One describes the situation in 1952, the other in 1957.

[536] See Dieks, "Von Neumann's Impossibility Proof…", p. 30, fn. 42.

[537] Paul K. Feyerabend, "Review of John von Neumann, Mathematical Foundations of Quantum Mechanics", in: Feyerabend, Philosophical Papers. Vol. 4…, p. 298 [emphasis added]. See Louis De Broglie, "La physique quantique restera-t-elle indéterministe?", *Revue d'histoire des sciences et de leurs applications* 1952, Vol. 5, No. 4, pp. 289–311, https://doi.org/10.3406/rhs.1952.2967.

[538] See Feyerabend, "Determinismus und Quantenmechanik…".

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> mann's book because of the account of the "reduction of the wave-packet" given in those two books? What I tried in my paper on measurement was to make use of this criticism as well as of Bohr's suggestion to regard measurement as an irreversible process which is represented, in a schematic way, by the "reduction of the wave packet". [539]

Feyerabend's strategy, following Bohr, was to advocate deterministic laws, realistically interpreted, identifying the source of the indeterminism as part of the measurement procedure, concluding that measurements of microscopic process are limited to superpositions of quantum properties. As he explained later:

> I want our laws to remain precise, reversible, deterministic but want to put the imprecision and sloppiness into the method rather than into the laws [in contrast to Popper]". [540]

As we shall see, Feyerabend was very critical of Popper's propensity interpretation strategy in quantum theory in the 1950s, both before and after it was presented to the public for the first time by Feyerabend on Popper's behalf at the Colston conference in 1957. [541] That will have to do for an overview. Now for some specifics of our story: Bohm's influence on the genesis of ERE (1962).

3.1 Feyerabend, Bohm and Bohr: From 1952 to 1957

Feyerabend's relationship to Bohm's ideas began in the early 1950s after Pop-

[539] Feyerabend to Popper, 21 July, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", pp. 267–268. Feyerabend is referring to Feyerabend, "On the Quantum-Theory of Measurement...".

[540] See Feyerabend to Hempel, 20 April 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, Part 1.

[541] According to Feyerabend: "The fact that the probability-statements of quantum-mechanics, whether they are now interpreted in terms of relative frequencies or in terms of propensities imply, in the case of the single electron, that it cannot have definite momentum and position at the same time. That means: Whether a physicist adopts now the frequency interpretation, or your interpretation, he would in both cases add some further arguments in order to show that the single system cannot be in a well defined state. More especially: Von Neumann's proof is valid in the frequency-interpretation as well as in the propensity-interpretation. But if this is true, then your paper is not conclusive" (Feyerabend to Popper, 30 March 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 256.

per had become his patron and mentor. Heavily under the influence of Popper's views on the foundations, [542] Feyerabend became keenly interested in Bohm's attempt to revive a "hidden variable'' interpretation of quantum theory [543] despite von Neumann's *alleged* proof that no deterministic completion by "hidden variables" can be consistent with the principles of quantum mechanics. [544] Von Neumann's *alleged* proof and the corresponding measurement problem became major topics of discussion between Feyerabend and Popper throughout the 1950s and early 1960s. As we shall see, Feyerband's engagement with Bohm's hidden variables proposal and von Neumann's alleged proof shaped the development of Feyerabend's own views on the measurement problem and the foundations of quantum theory, and his relation to Bohm, which in turn, shaped *Feyerabend's general methodology in* ERE (1962). [545] More specifically, Feyerabend's investigation of the relationship between Bohm's hidden variables proposal and von Neumann's alleged proof led Feyerabend to propose his own outline of a solution to the measurement problem and to develop and defend his own Bohrian (not Bohmian), realistic interpretation of quantum theory, and to develop his test-model as a criticism of falsificationism limiting its validity to commensurable cases, while trying also to promote Bohm's views, and what became Bohm's attempt to find a new conceptual order for physics.

In 1952 in Copenhagen, Feyerabend witnessed firsthand Bohr's initial reaction to learning of Bohm's 1952 "Hidden Variables" publications. [546] Bohr was

[542] Feyerabend met Popper in 1948 in Alpbach and they quickly became friends, with Popper increasingly taking on the role of mentor, and even father-figure. See especially Collodel and Oberheim, **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper...** and Collodel, "Was Feyerabend a Popperian…", pp. 27–56.

[543] See Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables I…" and Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables II…". De Broglie had made a similar proposal in the 1920s in his dissertation that Bohm later independently "rediscovered" in 1952. As is well known, de Broglie 1925 pilot-wave model led to Schrödinger's formulation of wave mechanics, after which the pilot-wave model was abandoned in favor of the quantum formalism. Bohm had been *unaware* of de Broglie's pilot-wave proposal when he developed his own.

[544] See von Neumann, **Mathematical Foundations of Quantum Mechanics...**

[545] This point has been made most clearly by van Strien, "Pluralism and Anarchism in Quantum Physics…".

[546] See Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables I…" and Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables

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amazed, according to Feyerabend. [547] This also seems to be the first time Feyerabend had even heard of Bohm's hidden variable proposal, during the *Nordisk Sommeruniversitet* (Scandinavian Summer University), in Denmark in the summer of 1952, where Feyerabend attended a seminar by Niels Bohr providing him the opportunity to intereact with his collaborators. [548] According to Feyerabend, directly after Bohr had given a talk, Feyerabend approached him asking for details.

> Bohr repeated his old explanations [...], but with renewed vigor, for he had just heard of David Bohm's apostasy. "Can you understand that?" he asked with a puzzled look on his face. Unfortunately, he was soon dragged away to another meeting". [549] As Feyerabend recounted, "It seemed that, for [Bohr], the sky was falling down". [550]

According to Feyerabend, Bohr was "neither dismissive, nor shaken [suggested by Peat in his enquiry]. He was amazed". [551] After Bohr left the room, the discussion of his talk 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

II...". Feyerabend met Niels Bohr at the first *Nordisk Sommeruniversitet* [Scandinavian Summer University], Askov (Danmark), 11–26 August 1951 (on the general theme "The Causality Problem". For background and details, see Matteo Collodel and Eric Oberheim, "Introduction", in: Matteo Collodel and Eric Oberheim (eds), **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper,** *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Venice — Berlin 2024, pp. 1–16.

[547] See Collodel and Oberheim**, Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, Part 2, and F. David Peat, **Infinite Potential: The Life and Times of David Bohm**, Addison-Wesley, Reading, Massachusetts 1997.

[548] See Peat, **Infinite Potential** ..., p. 129.

[549] See Feyerabend, **Killing Time...**, p. 78. This trip to Denmark resulted in Paul K. Feyerabend, "Bemerkungen zu Interpretation and Preciseness", in: Jens R. Gjerløw, Anders Bjerrum, and Hans Regnéll (eds.), **Semantiske problemer: inledningsforedrag og diskusjonsreferater fra Krets 5, Nordisk sommeruniversitet, 2. sesjon, Ustaoset 1952**, Akademisk forlag, Oslo 1953, pp. 77–84.

[550] See Feyerabend to Peat, 1993, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...** (note: "falling down" changed to "falling in" by Peat).

[551] See Peat, **Infinite Potential...**, p. 129.

> and how he had achieved it. It seems that Bohr himself never used von Neumann as a crutch. [552]

As we shall see, two years later, it is exactly these issues that Feyerabend investigated in his first major paper on the foundations of quantum theory. [553]

In the mid-1950s, Feyerabend was skeptical of ontological indeterminacy and interested in the relation of classical concepts and quantum concepts:

> Why do people believe in blurred electrons? I said in my lecture — and I think this to be true — that the philosophy of the orthodox is the philosophy of the older quantum-mechanics (of the Bohr-Sommerfeld conditions) and that Bohr never got rid of this philosophy. In spite of concluding from the fact that this theory was only meant to be a theory of energy-levels + (with the help of the correspondence principle, then handled very intuitively) transition probabilities and not a dynamical theory of the transitions themselves — in spite of concluding from this fact that it was incomplete all the future theories, which gave the dynamics of the transitions (e.g. Schrödinger) were interpreted as if the point of view of the older theory was still right, i.e. in terms of indivisible jumps. There is also an interesting ambivalent attitude towards classical physics. On the one hand (love) classical terms are the only meaningful terms of physical discourse. On the other hand (hate) we can never hope to understand electrons on classical lines. [554]

The relationship of classical concepts to the concepts of relativity theory and quantum theory was a much-debated topic at the time. [555] By 1955, contrary to some accounts Bohm's theory had not been completely ignored — or at least it was not being ignored by Feyerabend in Bristol, [556] when he began preparing his lectures on the foundations of quantum theory. Topics were to include some of the same topics he had discussed in his first paper: Bohm's response to von Neu-

[552] See Feyerabend to Peat, 1993, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, p. 236.

[553] See Feyerabend, "Physik und Ontologie...", pp. 464–476.

[554] Feyerabend to Popper, 11 February 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 223.

[555] Compare Einstein and Infeld, **The Evolution of Physics...**

[556] Pryce attended Feyerabend's lectures. See Feyerabend to Popper, 6 February 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 219 and Feyerabend to Popper, 11 February 1956, Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 223.

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mann's proof, [557] as well as the objections raised by Grete Hermann, de Broglie, as well as Einstein's criticism of Bohm's views and Bohm's response to Einstein. [558] Others had responded to Bohm as well. [559] Pauli had responded to Bohm in a 1952 publication rehearsing his earlier objections against de Brogie's prior pilot wave proposal. Einstein published a criticism of Bohm's theory in 1953, which he regarded to be essentially the same as Pauli's earlier criticism of de Broglie, [560] and Heisenberg published papers explaining his reasons for being unconvinced by Bohm's proposal in 1955, and then again later in 1958. [561] In 1955, foreshadowing his own proposed solution to the measurement problem to come, 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. [562]

Feyerabend's lectures on the foundations of quantum theory began in Bristol

[557] On Feyerabend and von Neumann, see Feyerabend to Popper, 7 July 1953, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 66; Feyerabend to Popper, 28 October 1955, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", pp. 199–200, and Feyerabend, "Autobiographfical Remarks 1976...", pp. 35, 37, 42.

[558] See Feyerabend to Popper, 28 October 1955, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 281 and Feyerabend to Popper, 6 February 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 219.

[559] On early criticisms of Bohm's theory, see Wayn C. Myrvold, "On Some Early Objections to Bohm's Theory", *International Studies in the Philosophy of Scienc*e 2003, Vol. 17, No.1, pp. 7–24.

[560] See Albert Einstein, "Elementäre Überlegungen zur Interpretation der Grundlagen der Quanten-Mechanik", in: Sir Edward Appleton, David Bohm, Louis de Broglie, Richard Courant, Albert Einstein, Pascual Jordan, Th. V. Kármán, S. S. Penner, Alfred Landé, Erwin Schrödinger, and Hermann Weyl (eds.), **Scientific Papers Presented to Max Born**, Hafner, New York 1953, pp. 33–40.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.

[561] Werner Heisenberg, "The Development of the Interpretation of the Quantum Theory", in: Wolfgang Pauli, Léon Rosenfeld, Victor Weisskopf (eds.), **Niels Bohr and the Development of Physics. Essays Dedicated to Niels Bohr on the Occasion of His Seventieth Birthday**, Pergamon, London and McGraw-Hill, New York 1955, pp. 12–29 and Werner Heisenberg, **Physics and Philosophy**, Harper and Row, New York 1958.

[562] Feyerabend to Popper, 1954, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 204.

at the start of 1956. Physicists regularly attended, including Maurice Pryce (who was head of the department) with whom Feyerabend often sparred during the lecture. This must have felt like quite a heavy lift for Feyerabend, who switched to Popper's point of view at the last minute (chickening out?): [563]

> 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. [564]

Feyerabend's ERE (1962), and his argument for theoretical pluralism more generally, carries on with this effort to fight for more tolerance (to leave proponents of the established theory less dogmatic) to promote progress. In this respect, Feyerabend and Popper shared the same goal. Notice how the Brownian motion example at the heart of Bohm and Vigier's 1954 sub-quantum fluid already takes center stage in these discussions in the same way. The Brownian particle is being compared to an electron. It is used to illustrate the idea that it exhibits indeterministic motion without having an indeterminate position. This appears to be one of the earliest times the example of Brownian motion appears in

[563] Feyerabend made it clear to Popper that he had switched to Popper's position in a letter on 11 February 1956: "You see, I have completely changed my outlook. I do not yet know whether everything can be explained in the (for me) new way, but at least there is much more room for fruitful investigation and I am really sorry that I was so obstinate for such a long time" (Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 222).

[564] Feyerabend to Popper, 11 February 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 223. Feyerabend also took Alfred Landé's reaction to Bohm's proposal to be very dogmatic (at least in a letter to him): "He [Landé] wrote me a letter about Bohm which is very dogmatic" (Feyerabend to Popper, 7 June 1958, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 286).

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Feyerabend's unpublished writings. Around the same time, Feyerabend made it even clearer that he was familiar with Bohm's use of the Brownian motion example in a letter to Agassi in 1956:

> Bohm has been here and gave a talk. There is much more behind his ideas than becomes evident from his articles in Physical Review. His intention is to explain the statistics of quantum mechanics out of the fluctuations of an underlying medium (similar to the theory of Brownian-movements) and atoms are conceived as vortices in this medium. This new theory of his which is not yet finished leads to Dirac's relativistic equations as a first approximation. [565]

Having switched to Popper's perspective, Feyerabend wrote to Popper at the end of January 1956: "for me the Neumann-proof loses more and more of its spell," and he announced to Popper that he planned to publish what became "A Remark on von Neumann's Proof" in 1956, [566] which he hastily submitted about six weeks later, [567] only to be accused by Popper of plagiarism two days later:

> I have read your new paper "A Remark on von Neumann's Proof". I do agree of course with its contents — since you got the complete contents of this paper here, in my room, from Joske and myself. As to your way of presentation, I do not like it much. But it is in any case a great improvement on your previous version; an improvement derived from our latest discussion of that version. I may remind you again of the fact that, when you still believed in the Neumann Proof, you were here and we had a great fight about it. During this fight Joske and myself asserted that this proof listed no more than the laws of the excluded gambling system which we stressed, was valid for dice or coins. In fact, we made the point much clearer and better than you do it now in your paper. [568]

Popper concluded his letter with a stern reprimand and demanded Feyerabend immediately add a note acknowledging himself and Agassi for Feyer-

[565] Feyerabend to Agassi, undated 1956, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

[566] See Feyerabend to Popper, 30 January 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 217.

[567] He sent Popper a draft on 14 April 1956, only after it had been submitted for publication, see Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 231.

[568] Popper to Feyerabend, 16 April 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 232.

abend's "Remark". [569]

> If you do not add a very full acknowledgement to Joske and myself, it will be a flagrant piece of plagiarism. I have warned you before against this practice. I think it would be quite wrong if I would close my eyes to this case. I do not intend to do this, and I am now warning you again. [570]

According to Popper, Feyerabend had added nothing to the discussion he had with Agassi about von Neumann's proof (with Feyerabend merely present). Feyerabend initially agreed to do as Popper bid (or at least he told Popper he would ask them to add such a note), but then seems to have decided better of it, willing to pretend to Popper that it had been too late to add the acknowledgement. [571] As in later cases of Popper accusing Feyerabend of misappropriating his ideas, the situation is quite complicated. On the one hand, Feyerabend clearly had a penchant for adopting ideas without always acknowledging his sources according to academic standards. On the other hand, Popper clearly had a penchant for thinking somehow owned every idea that ever crossed his mind, and was not being given due credit for them (for example, he thought Wittgenstein was getting credit for *his*, which ideas he had presented in a much clearer fashion). A close look at their correspondence in this case shows that Feyerabend excitedly sent Popper two consecutive letters outlining the four the main points of the short note, suggesting to Popper that he read an article by Fényes that he included with the letter. [572] The explicit goal of the short "Remark" is to replace Fényes complex considerations with more straightforward considerations. This was surely Feyerabend's contribution, as Feyerabend sent Feynes' paper to Popper. In this way, these letters raise some questions about the legitimacy of Popper's accusation of plagiarism in this case, as Feyerabend (at a minimum) bought Fényes derivation of the whole of quantum theory on the basis of the theory of Markoff-processes into the discussion, which is what Feyerabend used to try to clarify what Popper

[569] See Feyerabend, "Eine Bemerkung…", pp. 421–423.

[570] Popper to Feyerabend, 16 April 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", pp. 232–233.

[571] See Collodel, "Was Feyerabend a Popperian…", pp. 27–56 and Collodel and Oberheim, **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper…**

[572] See Feyerabend to Popper, 30 January 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 217 and Feyerabend to Popper, 6 February 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 220.

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claimed to be *his* point. [573] Moreover, Feyerabend had already very prominently explicitly cited Popper with respect to the claim that the dice example shows this problem with von Neumann's proof:

> I owe to Prof. K. R. Popper (London) the remark that NP did not reach its goal because it is valid not only for QM, but also for the game of dice. [574]

Popper seems to think he owned more than just the idea that there is nothing special about indeterminacy in quantum theory beyond the indeterminism on the classical level as exhibited by a game of dice. Of course, it is easy to see why Popper was understandably irritated. In a way, Feyerabend was downplaying the originality of the insight underlying Popper's long promised propensity interpretation of quantum theory, which Feyerabend only later first presented publicly on Popper's behalf at the 1957 Colston conference. It is worth noting that just two days later, Popper made it clear that he did not think the note contained anything worth publishing, while openly threatening Feyerabend:

> I must explain why I expect an immediate reply from you. I do not believe that the contents of your paper are really worth publishing. I should never have published it myself. It is thus not that I think you are taking valuable goods from Joske and myself — goods which we find valuable. It is a different thing — a matter of principle. The goods are goods which you find valuable enough to publish. And they are not your property. I therefore want to know at once what you propose to do. If I do not get a reply from you within four or five days, I intend to write to the Editor of the *Physikasche Zeitschrift*, sending him copies of my letters to you. I shall leave it to him whether or not to publish, of course; but I shall express my view that I consider a publication to be morally wrong. [575]

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[573] See Paul K. Feyerabend, "A Remark on von Neumann's Proof(1956)", in: Stefano Gattei and Joseph Agassi (eds.), **Paul K. Feyerabend. Physics and Philosophy. Philosophical Papers Volume 4**, Cambridge University Press, Cambridge 2016, pp. 46–48, https://doi.org/10.1017/CBO9781139034197.005.

[574] See Feyerabend, "Eine Bemerkung...", p. 422.

[575] Popper to Feyerabend, 18 April 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", pp. 59–422. Popper concluded this letter by suggesting he was trying to do Feyerabend a favor: "I need not tell you that I have most urgent work to do and that I do not like wasting my time on this issue– quite apart from the fact that I am grieved and worried to have to do such a thing. But I have to do it for your sake: otherwise you will never learn what one may do and not do. I think it better for you to have this conflict with me than with somebody else" (Popper to Feyerabend, 18 April 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 234).

Feyerabend very quickly decided that his remark on the von Neumann proof that Popper had tried to appropriate as his own, was wrong, returning to his earlier view of what he took von Neumann's discussion of the measurement problem to have shown, which motivated the strategy in the foundations of quantum theory for solving it that he subsequently tried to develop. [576] By January 1957, Feyerabend made his growing discomfort with respect to the content of this "Remark" very clear in a letter to Popper: "if I am right your and Joske [Agassi]'s criticism [of von Neumann] which I have adopted myself is wrong". [577] It now seemed to Feyerabend once again that "there is much more behind [von Neumann's] proof than you were inclined to admit". [578] This explains why Feyerabend never published this paper in English, given that most of his other papers in this period are published in both German and English.

3.2 Feyerabend, Bohm and Popper on The Measurement Problem (1957)

Later that year (1957), Feyerabend's paper "On the Quantum-Theory of Measurement", which discusses Heisenberg and von Neumann's assumption of "quantum jumps" and the difficulties to which it leads when incorporated into a theory of measurement, was presented at the Colston conference and published both in German and English. [579] Feyerabend was trying to show that these difficulties arise because of the incompleteness of their accounts, and he tried to provide a sketch of a complete account of the theory of measurement that does not contain "quantum jumps". [580]

[576] See also Flavio Del Santo, "Beyond Method: The Diatribe Between Feyerabend and Popper Over the Foundations of Quantum Mechanics", *International Studies in the Philosophy of Science* 2022, Vol. 35, pp. 5–22.

[577] Feyerabend to Popper, 21 January 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 249.

[578] Feyerabend to Popper, 21 January 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 249.

[579] See Feyerabend, "Zur Quantentheorie der Messung…" and Feyerabend, "On the Quantum-Theory of Measurement…", respectively.

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From then on, Feyerabend continued to defend von Neumann's proof, which he used to criticize Popper's propensity interpretation, claiming that Popper's propensity interpretation,

> does not contain any contribution towards the main problem of quantum-mechanics [...] Or if it does, this contribution clearly contradicts the Neumann proof which is not changed by introducing the propensity-interpretation instead of the usual interpretation in terms of relative frequencies (which von Neumann uses). [581]

Feyerabend was quite clear that he thought that von Neumann's proof was correct in this respect. According to Feyerabend,

> if within von Neumann's proof (which I think is correct and which I must discuss with you as soon as you return!) your probability is used instead of another interpretation, the proof still excludes hidden variables. [582]

Feyerabend once rejected Popper's propensity interpretation, reporting to Popper after the 1957 Colston conference that Bohm had used the same points that Feyerabend had used to criticize Popper's views:

> *Bohm using much the same arguments that I had used in my letter to you*, Pryce asserting that a photon was not only in two states, but in infinitely many states (which is irrelevant as, in the classical analog, you could replace the coin by a ball) and others using similar arguments [against Popper's propensity interpretation of quantum theory as presented by Feyerabend on Popper's behalf at the conference]. [583]

Popper tried to deal with the measurement problem by ascribing indeterminate objective propensities to both classical and quantum objects so that there is no measurement problem. The underlying idea is that there is no special problem of indeterminacy in the foundations of quantum theory.

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[580] Feyerabend, "Zur Quantentheorie der Messung...", p. 551. On the history of the composition of Feyerabend, "On the Quantum-Theory of Measurement...", see Feyerabend to Popper, 27 March 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 254.

[581] Feyerabend to Popper, 27 March 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 254.

[582] See Feyerabend to Popper, 27 March 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 254.

[583] Feyerabend to Popper, 15 December 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 271 [emphasis added].

Feyerabend initially proposed this strategy *for Popper* in two specific senses: First, as Popper was unable to attend, Feyerabend read Popper's paper for him at the 1957 Colston conference. This was the first public presentation of Popper's attempt to use his propensity interpretation to resolve the "great quantum muddle" about the collapse of the wave function and the mistaken purported role of the observer in it. As we shall see in section 3.4, at the conference, Feyerabend also first sketched his own proposed strategy for solving the measurement problem, which he tried to use to criticize Bohr, only to learn from Vigier after his talk that his proposed strategy was actually Bohr's own strategy (Feyerabend inadvertently "lessened" Bohr). [584]

Second, as we have seen, Feyerabend brief's "Remark on von Neumann's proof" (1956), [585] had already argued for the underlying idea, using the same oft used dice example as Popper. [586] In 1982, the third volume of Popper's **Postscript to The Logic of Scientific Discovery, Quantum Theory and the Schism in Physics** was belatedly published. In it, Popper presents Bohm as a determinist, who he criticizes. Popper's argument is apparently directed at what he took to be Bohm's views based on Bohm's initially 1952 deterministic hidden variables proposal, [587] which does seem to imply determinism. As we have seen, however, two years later in 1954, Bohm had tried to correct this misinterpretation of his views. [588] With Vigier, he promptly published an explicitly indeterministic account. This makes it look like Popper and many others overlooked Bohm's later work. [589]

[584] On the diatribe between Feyerabend and Popper over the foundations of quantum mechanics, see Del Santo, "Beyond Method…".

[585] See Feyerabend, "Eine Bemerkung…", pp. 421–423.

[586] Bohm, "A Proposed Explanation of Quantum Theory…" also used the same example, which del Santo (2019) uses to illustrate the similarity between Popper and Bohm's ideas.

[587] See Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables I…" and Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables II…".

[588] See Del Santo and Krizek, "Against the »Nightmare of a Mechanically Determined Universe«…" and van Strien, "Why Bohm Was Never a Determinist…".

[589] Del Santo argues that Popper misrepresents and harshly criticizes Bohm's alleged determinism. See Del Santo and Christian Krizek, "Against the »Nightmare of a Mechanically Determined Universe«…", p. 2. He seems to be unaware that Popper, **Quantum Theory and the Schism in Physics…** was written in the early 1950s, which explains why it was so outdated with respect to Bohm's views on determinism. Del Santo well-illustrates that Bohm's *sustained* support for indeterminism is par-

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Bohm complained to Popper in a letter about this mischaracterization of his views. He explained to Popper that he had never believed in determinism, [590] and that he had only published his 1952 proposal, on Einstein's advice, to re-open and otherwise Copenhagen-closed (shut up and calculate) discussion. It was just a pilot pilot-wave theory. Bohm also explained that his views are quite similar to Popper's propensity interpretation in that Bohm also ascribes objective probabilities to the behavior of quantum particles, and that he (Bohm) continues to support such an account.

The main difference between Bohm and Popper seems to be that for Popper the "objective propensities" are irreducible properties of the quantum objects themselves, while Bohm wants to explain these objective propensities by positing an even lower-level quantum "fluid" sub-stratum that causes them (irreversibly) through a stochastic process akin to Brownian motion, just on a level lower. In other words, just as quantum motions that obey deterministic laws cause classical stochastic motions (Brownian motion), a sub-quantum underlying "fluid" causes stochastic quantum motions reflected by the probability distributions in their behaviors. In this way, Bohm's is a causal or ontological theory *explaining* the results predicted by quantum theory. In this sense, it is not merely an attempt to reinterpret its formalism. It was supposed develop into a new theory that should replace the established theory with respect to a realistic interpretation. From Bohm's perspective, the established quantum formalism is just an approximation that does not give a satisfactory ontological account. As we shall see, Bohm suggests that the established theory should be treated like Newton's theory, which was superseded on the classical level by relativity theory with respect to the ontology that it implies, and Feyerabend was clearly trying to support Bohm's attempt to replace the established quantum framework with a new one. Feyerabend was clearly on Bohm's side. He criticized the established view as an "observationally complete theory", exactly because it implies that any deeper explanation such as Bohm's must remain "hidden". [591]

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tially acknowledged posthumously by Bohm in Mara Beller, **Quantum Dialogue: The Making of a Revolution**, University of Chicago Press, Chicago 1999, and then more fully in: Talbot Chris (ed.), **David Bohm: Causality and Chance, Letters to Three Women**, Springer, Cham 2017.

[590] See van Strien, "Why Bohm Was Never a Determinist..." and Del Santo and Krizek, "Against the »Nightmare of a Mechanically Determined Universe«...".

[591] See Feyerabend, "Observationally Complete Theories...", pp. 487–492.

Feyerabend's account tries to explain the indeterminism as an effect caused by the interaction between the microscopic and macroscopic domains, in some ways similar to Bohm's attempt to explain quantum indeterminacy based on the interaction between autonomous levels of reality, each of which obeys its own sets of laws. Since the Colston conference in 1957, Feyerabend saw himself as developing Bohr's strategy, and yet, according to Feyerabend, Bohm thought his view (Bohm's) was closest to Bohr's view. [592]. The deep similarity between Bohm and Feyerabend with respect to determinism and indeterminism is that, following Bohr, they both want to locate the source of the indeterminism in the causal interactions between objects that belong to different levels that are both deterministically described. Feyerabend tried to explain the indeterminism and irreversibility as resulting from the *causal* interactions between a microscopic realm of quantum superpositions that obey deterministic laws and the classical realm of definite properties and positions that also obey deterministic laws. This can also be seen in the stochastic behavior of the Brownian particle on the classical level, which otherwise obeys deterministic laws, which is caused by its interactions with quantum particles that also obey deterministic laws. The idea is that indeterminism results because of casual (irreversible) interactions between two otherwise deterministic (reversible) processes on different autonomous (obeying different laws) levels. Similarly, Bohm explains the indeterminism as resulting from the *causal* interactions between a quantum realm described by deterministic laws and a deeper sub-quantum "fluid" that causes it. This can clearly be seen through an analogy between Bohm's position and Brownian motion (which is the easiest way to understand Bohm and Vigier's 1954 indeterministic hidden variables proposal. [593]The idea is to show that there is an underlying "fluid" substratum that interacts *causally* (irreversibly) with quantum particles so that they behave according to the deterministic processes described by the Schrodinger equation, analogously to the kinetic theory's quantum explanation of the cause of the stochastic behavior of the Brownian particle on the classical level.

Bohm was bothered by the perceived deterministic implications of his 1952 hidden variables proposal. Bohm and Vigier's 1954 proposal is specifically de-

[592] For Bohm on Bohr, see especially David Bohm, "On Bohr's Views Concerning Quantum Theory", in: Ted Bastin (ed.), **Quantum Theory and Beyond**, Cambridge University Press, Cambridge 1971, pp. 33–40.

[593] See Bohm and Vigier, "Model of the Causal Interpretation of Quantum Theory…".

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signed to make their indeterminism explicit, by showing how indeterminism could be the result of causal relations between deterministic processes on different levels (classical, quantum, sub-quantum "fluid"). On this interpretation, Feyerabend and Bohm were close allies in discussions of the foundations of quantum theory *generally* (both advocating for a new framework to replace the quantum theory) *and more specifically* Feyerabend's strategy for solving the measurement problem, from Bohm's perspective, looks like just one important piece of a bigger picture. Feyerabend locates the source of the indeterminism in the causal interactions between classical objects that otherwise obey deterministic laws, and quantum objects that otherwise obey deterministic laws. Bohm adds an analogous explanation of the deterministic laws governing the quantum objects based on a causal interaction with an underlying "fluid" substratum (which itself may be causally interacting with underlying objects that obey different laws, potentially *ad infinitum*).

For Feyerabend the problem with Bohm's approach in the foundations of quantum theory is that it is *ad hoc* in the sense that it introduces postulates to explain known experimental results, that are not independently testable:

> if one does not prefer *ad hoc* to introduce some ensembles (like Bohm) which have then no empirical meaning (in the sense of independent testability) whatever and are by no means better than the Lorentz-connection-hypothesis in the case of relativity: If you want to save the ether you can do it. But you get things which are not independently testable. If you want to save the electron with definite position + momentum, you can do it, but you again get things which are not independently observable. Of course, you can always retrodict the path of an electron. But this does not show anything. For you can always describe the past of a patient in terms of psychoanalysis – which does not show that the assumptions of psychoanalysis are scientific or true. In the same way those past paths are not scientific, metaphysical, they are ad hoc. [594]

Clearly, the issue as Feyerabend sees it concerns the use of *ad hoc*, independently untestable assumptions. From a Popperian perspective, as we have seen, such *ad hoc* hypotheses should remain unconvincing so long as there is no independent evidence supporting them. In this respect, Popper's methodology makes Bohm's proposal seem reactionary, which is also how he was seen from the perspective of the establishment, according to which metaphysical speculation was fi-

[594] Feyerabend to Agassi, 25 February 1957, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

nally being removed from the quantum theory, when along comes Bohm trying to reintroduce metaphysical speculation into physics.[595] However, from Feyerabend's perspective, Bohm's views are progressive as "an attempt at a realistic interpretation of experience", because, as Bohm often emphasized, what at first may be independently untestable, may not always remain so, as evidenced by atomism, the kinetic theory and Einstein's predictions about Brownian motion. That is why for Feyerabend, it is of the Copenhagen school that was acting reactionary, for dogmatically dismissing Bohm's proposal.

Feyerabend, Popper and Bohm were fellow defenders of realism. They all fought in various ways against the hegemony of Copenhagen positivism in postwar discussions about the foundations of quantum theory, adamant in their support of the view that theories shape experience and should be understood as attempts to explain reality. They all also explicitly proponents of some form of indeterminism, although Popper can appear, not to have been aware that "Bohm was never a determinist",[596] or as del Santo and puts it: "Bohm was never a Bohmian".[597] In any case, Bohm's as we are about to see complex views defy this universal distinction (determinism vs. indeterminism) and other traditional distinctions in important respects.[598]

3.3 Bohm's *Causality and Chance in Modern Physics* (1957)

After Feyerabend's lectures on the foundations of quantum theory in 1956, the next major event that shaped Feyerabend's developing views in methodology took place a couple of months before the Colston conference, when Bohm's book

[595] See van Strien, "Pluralism and Anarchism in Quantum Physics…".

[596] Given that material Popper wrote in the first half of the 1950s was only published much later (in 1994). See van Strien, "Why Bohm Was Never a Determinist…".

[597] See Del Santo and Krizek, "Against the »Nightmare of a Mechanically Determined Universe«…".

[598] Del Santo and Krizek remark that "It is a peculiar case that a scholar [Bohm] to whom so many historical and philosophical studies have been devoted has been so misrepresented" (Del Santo and Krizek, "Against the »Nightmare of a Mechanically Determined Universe«…", p. 2). It might not be that peculiar, as the same could also be said of many others including Feyerabend.

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Causality and Chance in Modern Physics (1957) was published: [599] Feyerabend read it immediately.

> Did you see Bohm's book? [...] Very valuable observations (a violent attack upon the Bohr-Heisenberg interpretation) a popular exposition of his, Vigier's, de Broglie's attempts at a causal model — but many obvious blunders. [600]

And a few days later, as he had not yet heard back from Popper, Feyerabend again showed how eager he was to discuss Bohm's ideas: "P.S. Have you seen Bohm's book **Causality and Chance in Modern Physics**". [601] This book was foundational for ERE (1962), because it puts the example of Brownian motion center stage in discussions of issues in the foundations of quantum theory and in discussions of general methodology, in both of which Feyerabend went on to develop and defend his "plea for tolerance in matter epistemological". [602] So, a lot was happening in the build up to the 1957 Colston conference, and as we shall see, a lot of things relevant to our story happened at the Colston conference and shortly afterwards.

A complete reconstruction of Bohm's developing methodological views in **Causality Chance on Modern Physics** would far exceed the bounds of this section. Many of the most relevant topics to methodology that are discussed in the book were brought up at the 1957 Colston conference, so for the moment a very

[599] The book was mostly written when Bohm arrived in Israel in 1955, see Peat, **Infinite Potential...**. Although Bohm never mentions Engles, Marx, Lenin or Hegel, it was very clear to Feyerabend that Bohm's view was based on dialectical materialism, see Feyerabend's review, "Professor Bohm's Philosophy of Nature...", where he rejects Bohm's dialectical materialism in favor of Popper's falsificationism. On Bohm on Hegel and binary oppositions, see for example David Bohm, and F. David Peat, **Science, Order, and Creativity**, Bantam Books, Toronto — New York — London — Sidney — Auckland 1987. Compare Peat's description of Bohr's belief that, "while the opposite of truth is an error, the opposite of a great truth is another great truth" (Peat, **Infinite Potential...**, p. 184. According to Peat, "this maxim even influenced the way Bohr wrote his scientific papers. Bohr would agonize about the best way to express a particular idea in a given sentence; then, when he had written it down, he would attempt to express its exact opposite in the sentence that followed" (Peat, **Infinite Potential...**, pp. 184–185). Feyerabend rejected dialectics.

[600] Feyerabend to Popper, 27 March 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 253.

[601] Feyerabend to Popper, 1 April 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 259.

[602] See Feyerabend, "Problems of Microphysics...", and title of Feyerabend, "How to Be a Good Empiricist...".

schematic introduction will suffice, as we will return to many of the same topics in section 3.4. In his book, Bohm gave a methodological argument for his infinite levels. Bohm's view was that,

> both the assumption of absolute determinism and the assumption of indeterminism obstruct the progress of science: a determinist assumes that one already knows all causes at play, while an indeterminist assumes that there are no causes to be found, and therefore both views discourage a search for "deeper" causes than those already known. [603]

Bohm's view of the qualitive infinity of nature is defended on methodological grounds and is directly related to his views on determinism and its dialectical relation to chance, as Bohm's methodological argument for the dialectical relationship between causality and chance supports his methodological understanding of the infinite quality of nature.

According to Bohm's methodology of the infinite quality of nature, determinism and chance are related dialectically. Deterministic laws of nature, subject to further investigation, may be shown to arise through averaging over chance fluctuations, just as statistical laws of nature that ascribe probabilities to future events may be shown to arise from underlying determinism. As van Strein succinctly puts it, according to Bohm, "wherever one finds determinism, there can be underlying chance and vice versa". [604] In this way, Bohm argues both for and against determinism. He argues for determinism objecting to the standard interpretation of quantum mechanics because it takes indeterminism to be fundamental, eliminating the possibility of providing exact descriptions of the trajectories of quanta. At the same time, Bohm argues against determinism objecting that it contains two mistaken assumptions: The current theory is the final theory and laws hold absolutely. By contrast, Bohm holds that laws always have a limited domain of validity, even within which they do not hold absolutely. They do not hold absolutely within their limited domain of validity because the different domains overlap and so are never completely isolated from each other. [605] In other words, because laws of nature never hold absolutely, nor provide a complete description of nature, laws do not fully determine the future. As Bohm himself puts it, laws:

[603] In Talbot (ed), **David Bohm: Causality and Chance...**, p. 254.

[604] van Strien, "Why Bohm Was Never a Determinist...", p. 10.

[605] van Strien, "Why Bohm Was Never a Determinist...", p. 10.

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> make possible only a one-to-many correspondence between cause and effect, in the sense that a specification of certain causes will in general limit the effect to a certain range of possibilities. [606]

Complete determinism is an idealization because no system is ever perfectly isolated. There are always external disturbances. Moreover, even in a perfectly isolated mechanical system:

> there would still exist disturbances coming from motions at the molecular level. Of course, one could in principle try to take these into account by applying the laws of motion to the molecules themselves, but then one would discover still further disturbances coming from the quantum-mechanical and other deeper-lying properties of matter. [607]

Methodologically speaking, the assumption that the indeterminism of quantum theory is final, that chance is an irreducible property of matter, is as unjustified as the assumption that determinism of classical mechanics holds absolutely. Both views exclude the possibility that new theories will lead to radical changes in the theoretical framework. [608] It is not that Bohm believed that this structure of describing things at increasingly smaller scales will never break down in the long run, but it may "disclose a still more general pattern of organization of things". [609] Bohm was convinced of the infinite quality of nature, which is inexhaustibly rich so that science will never be finished. [610] As van Strein points out, Bohm argues: "Every time we have a regularity, we must have some limit to this regularity. [...] Wherever there is law, there must be lawlessness". [611] It follows for Bohm that, "the future is not fully determined by the past". [612]

By the early 1960s, Bohm had begun his search for a more radically new theory of quantum physics suggesting that also our conceptions of space and time

[606] Bohm, "A Proposed Explanation of Quantum Theory...", p. 16.

[607] Bohm, "A Proposed Explanation of Quantum Theory...", p. 16.

[608] See Bohm, **Causality and Chance...**, p. 102.

[609] Bohm, **Causality and Chance**, p. 140.

[610] See van Strien, "Why Bohm Was Never a Determinist...", p. 11.

[611] Paavo Pylkkänen (ed.), **Bohm-Biederman Correspondence. Vol. 1. Creativity and Science**, Routledge, London — New York 1999, p. 34.

[612] Bohm in Pylkkänen, **Bohm-Biederman Correspondence...**, p. 14.

may have to be revised. As Bohm later explained in **Wholeness and the Implicate Order** (1980), the idea of undivided wholeness attempts to ground a new framework on what quantum theory and general relativity share in common, each in their own way. As Bohm put it:

> 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 [relativity theory requires continuity, strict causality (or determinism) and locality. On the other hand, quantum theory requires noncontinuity, noncausality and non-locality]. [613]

So that according to Bohm:

> 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. [614]

However, there seems to be little evidence that this change affected his views on causality and chance, as Bohm continued to believe that whenever there is determinism, there can be underlying chance and vice versa, and "ultimately

[613] Bohm, **Wholeness and the Implicate Order...**, p. 223. Bohm was dissatisfied with the tension between classical and non-classical concepts and throughout the 1960s he proposed developing a new language structure or conceptual framework to subsume both them, see especially David Bohm, **Problems in the Basic Concept of Physics: An Inaugural Lecture Delivered at Birkbeck College 13th February 1963**, London University, Birkbeck College, London 1963 and his inaugural lecture, David Bohm, "Hidden Variable Theories as a Step towards a New language Structure for Physics", *Contemporary Physics: Trieste Symposium 1968 (Trieste, 7-28 June 1968), Proceedings Series. Vol. II*, International Atomic Energy Agency, Vienna 1969, pp. 439–448.

[614] Bohm, **Wholeness and the Implicate Order...**, p. 223. Bohm tries to provide a realistic explanation (a model) of non-locality and quantum entanglement as characteristic of this interconnectedness based on a hologram. Briefly, if you cut the film in half that produces a hologram, you still get two complete holograms, and if you cut those in half you have four *complete* holograms (just a quarter the size). This magical feat is possible because in contrast to a normal photography where each point on the film only records the light landing at that point, in a hologram the film records the entire image at every point on the film (somewhat similar to how every cell contains a copy of the DNA of the whole organism). According to Bohm's Kant-on-wheels-and-levels realism, we are living in a kind of holographic film in which the entire underlying reality is stored at every point.

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> our overall world view is neither absolutely deterministic nor absolutely indeterministic". [615]

3.4 Meaning, Methodology and The Foundations of Quantum Theory: The Colston Conference (1957)

The section outlines the impact and significance of the discussions at the 1957 Colston conference on the genesis and development of Feyerabend's criticism of Popper's falsificationism in ERE (1962). As we shall see, the discussions at the conference served as a testing ground for Feyerabend's views on the methodological implications of meaning change, and Feyerabend found a compatriot in Bohm, who he saw as a fellow outsider, and whose cause he took up in the struggle to advance science against the dogmatic philosophies they took to be holding it back. That is what led to Feyerabend's methodological argument for theoretical pluralism in ERE (1962). In this way, this conference, and especially the discussions that took place, provide the context for understanding the impact that Bohm and his ideas had on Feyerabend's philosophical development in relation to Popper's methodological views.

Before the conference, the scientific community had taken the attitude that Bohm had been slayed both in philosophy and in physics. [616] The general view was that Bohm had been doubly ex-communicated: from the Church of the Copenhagen school and from the USA by McCarthyism. Feyerabend and Popper, by contrast, were both initially very excited by Bohm's work, and Feyerabend clearly was eager to side with Bohm. According to Freire Junior, Rosenfeld, who saw Bohm's causal interpretation as metaphysical regression:

> mobilized colleagues wherever he could to take up the fight against the causal interpretation. He appealed to his professional connections as well as companions sharing

[615] David Bohm and Basil James Hiley, **The Undivided Universe: An Ontological Interpretation of Quantum Theory**, Routledge, London 1993, p. 324.

[616] Einstein to Born, 26 November 1953, in: Albert Einstein, Max Born, and Hedwig Born, **The Born-Einstein Letters: Correspondence Between Albert Einstein and Max and Hedwig Born from 1916-1955, with Commentaries by Max Born**, Macmillan, London 1971, p. 207.

> ideological ties with Marxism. He pushed Frédéric Joliot-Curie—a Nobel prize winner and member of the French Communist Party—to oppose French Marxist critics of complementarity; advised Pauline Yates— Secretary of the "Society for cultural relations between the peoples of the British Commonwealth and the USSR"—to withdraw her translation of a paper by Yakov Ilich Frenkel critical of complementarity from *Nature*; asked *Nature* not to publish a paper by Bohm entitled "A causal and continuous interpretation of the quantum theory;" and advised publishers not to translate one of de Broglie's books dedicated to the causal interpretation into English. [617]

Feyerabend alludes directly to the situation in ERE (1962), without mentioning Bohm by name:

> Today, of course, the elimination takes the more refined form of a refusal to publish (or to read) what is not in agreement with the accepted doctrine. However, this liberalism applies to physical theories only. It does not seem to apply to political theories. [618]

The conference is often taken as the turning point in redemption of Bohm's reputation that broke the hegemony of the Copenhagen interpretations by re-establishing the legitimacy of questions about the foundations of quantum theory.

At the start of the conference, Bohm made it clear that he was eager to set the record straight with respect to his views on determinism in the first discussion:

> as I will discuss tomorrow, one may say that there are many levels of reality or being and that, for example, at one level the facts which can be found out are not enough to determine exactly what will happen at that level. Moreover there is an element of randomness in the sequences of events in that level which is due to events that take place or to things that exist at another level. As a result randomness is just as much a necessity as whatever causal determination may exist. [619]

Bohm then elaborated on his views on determinism and indeterminism it in the second discussion:

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[617] Olival Freire Junior, **The Quantum Dissidents: Rebuilding the Foundations of Quantum Mechanics (1950–1990)**, Springer, Cham 2015, pp. 37–38, see also van Strien, "Pluralism and Anarchism in Quantum Physics…".

[618] Feyerabend, ERE (1962), p. 69, fn. 85.

[619] Bohm in the discussion after Alfred J. Ayer, "The Conception of Probability as A Logical Relation", in: Stephan Körner and Maurice Henry Lecorney Pryce (eds.), **Observation and Interpretation: A Symposium of Philosophers and Physicists, Proceedings of the Ninth Symposium of the Colston Research Society**, Butterworths Scientific Publications, London and New York 1957, p. 22 [3–32].

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> In terms of the infinity of nature we may rather say that every law is at one stage determinate, at another stage statistical; every law leaves out an infinity of factors in lower levels. It is this fact which makes the law an approximation, which makes it statistical. But as you go deeper you see the causes of the fluctuations and thereby the law becomes determinate; but then the deeper level is in its turn subject to factors which have been left out and it will be statistical too. Hence from this point of view, every law must have both a determinate and a statistical side. To say that it must be purely determinate would be metaphysical, just as to say that it must be purely statistical would also be metaphysical. [620]

Bohm's basic idea is in some way similar to Kuhn's emphasis on how theories act as blinders directing what is, and what is not, seen when assessing a situation. To bring some aspects into sharp focus through normal science, other aspects of some level of reality must go unnoticed. For Bohm, similarly, for an approach to succeed it must leave out another equally valid side of the story (a new, or deeper, or more comprehensive level, that he would later call an "implicate order"). [621] As we shall see, while Feyerabend explicitly rejected Bohm's "dialectical" methodology in 1960, [622] because he rejected the idea that historical laws determine patterns in the growth of knowledge, the theoretical pluralism that he initially delineates in ERE (1962) shares many significant affinities with Bohm's views.

According to Bohm and Vigier's worldview, as a whole, the behavior of the universe is causal and indeterministic because while on certain levels it obeys deterministic laws, they hold only approximately within a domain with a limited validity. Chance fluctuations can result from interactions between laws in new domains, or from processes on a lower level, or from interaction of laws on different levels that apply to objects at the border between their domains, and there are an infinite number of qualities to investigate, with endless qualitatively new properties at different levels and in different domains as they interact. As Vigier put it in the discussions at the conference:

> For nature should be considered as infinitely complex, which implies that no given set

[620] Bohm n the discussion after Léon Rosenfeld, "Misunderstandings About the Foundations of Quantum", in: Stephan Körner and Maurice Henry Lecorney Pryce (eds.), **Observation and Interpretation: A Symposium of Philosophers and Physicists, Proceedings of the Ninth Symposium of the Colston Research Society**, Butterworths Scientific Publications, London and New York 1957, p. 60–61 [41–64].

[621] See Bohm, **Wholeness and the Implicate Order...**

[622] See Feyerabend, "Professor Bohm's Philosophy of Nature...".

> of laws can be sufficient for justifying the validity of the laws of probability. Outside each context there exist more complex things which act on the phenomena in question, and it is this very complexity which allows us to apply the calculus of probability, rather than specific features of information or peculiarities of the observers. [623]

Bohm emphasized that the observer is part of the system studied, and the system is casually influenced by the observer:

> Now the difference between Professor Rosenfeld and myself is this: we both agree that present quantum mechanics implies that the apparatus plays a very fundamental role in helping to produce what is there, because there is such a close sensitive coupling between the thing and the apparatus. But I propose a model which aims at the explanation of why this happens. It is a model of the observer and of what he does as well as of the system. This model shows how both the observer and the system exist at a deeper level and how they must both influence each other. [624]

Bohm's model tries to explain how and why this quantum indeterminacy of location and motion happens. To do this, Bohm developed a model according to which the quantum potential is an effect of a statistical motion analogous to Brownian motion in thermodynamics:

> as soon as you have fluctuations there appears an additional term, the temperature times the entropy. This term represents an effect of the statistical fluctuations of the motion of the atoms. Indeed it is a kind of statistical potential which must be added to the energy in order to get the effective free energy in any actual transformation of the microscopic system. The present argument is therefore that the quantum potential comes out as an effective statistical term which should be added to the remaining energies to help determine the average motion of the electron. The quantum potential therefore ceases to play an essential role because everything is explained in a deeper way. [625]

It is a model of the observer and an interaction between things that obey laws on different levels. Bohm tried to illuminate his views with a metaphor to icebergs:

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[623] Vigier in the discussion after Bohm, "A Proposed Explanation of Quantum Theory…", p. 34.

[624] Bohm in the discussion after Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 61.

[625] Bohm with in the discussion after Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 60.

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> In this model the electron may be conceived as an iceberg most of which is not visible above the surface of the sea. The same is true of the observing apparatus. If we make an observation many things are going on underneath; and this is why things are so complicated. [626]

As Bohm and Vigier tried to explain these views, Feyerabend witnessed them go head-to-head against Rosenfeld and Pryce. [627] Given that Pryce was Bohm's boss, this might look like a Bohm and Goliath situation, but to Feyerabend, Bohm was plainly the intellectual giant in the room, which he thought had not been properly appreciated by Pryce, Rosenfeld and others (Feyerabend on the other hand, would have been familiar with Heisenberg and Einstein's appreciation of Bohm and his earlier work). To Feyerabend it seemed that Bohm was being derided by Rosenfeld and Pryce and some of their allies from around the world, who were dogmatically dismissing Bohm's proposal as metaphysical speculation based on of their unjustifiable philosophical views — and not on sound scientific reasoning. At the time, Bohm was clearly the outsider, as few physicists were interested in pursuing Bohm's approach, except de Broglie and a group of his colleagues in Paris. Feyerabend, by contrast, visibly supported and encouraged Bohm and Vigier and their views.

Rosenfeld gave a talk on "Physics and Metaphysics" criticizing Bohm's approach, but not by engaging directly with his theory. [628] Instead, he argued against interpretations of quantum theory as metaphysical in general. This did not seem to sit well with the audience. Even some members of the Copenhagen school seemed uncomfortable. Feyerabend thought the orthodox physics community was treating Bohm's theory, much the same way as Ehrenhaft's effects had been dis-

[626] Bohm with in the discussion after Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 61.

[627] This is not meant to imply that there were not valuable contributions to the discussions by other participants, but just that Pryce was Bohm's boss, and Rosenfeld is sometimes taken to epitomize the positivistic shut up and calculate attitude of the Copenhagen school. Rosenfeld, of course, objected to these ascriptions of positivism to him. Feyerabend participated directly in the discussions of Bohm's: "A Proposed Explanation of Quantum Theory in Terms of Hidden Variables at a Sub-quantum-mechanical Level", together with E. W. Bastin, D. Bohm, F. Bopp, M. Fierz, H. J. Groenewold, E. H. Hutten, S. Körner, D. M. Mackay, M. Polanyi, M. H. L. Pryce, L. Rosenfeld, J.-P. Vigier, and J. O. Wisdom. See Bohm, "A Proposed Explanation of Quantum Theory…".

[628] Léon Rosenfeld, "Physics and Metaphysics", *Nature* 1958, Vol. 181, Iss. 4626, p. 1814, https://doi.org/10.1038/1811814b0.

missed by the physics community in the late 1940s (see the postscript). Feyerabend later compared the dogmatic attitude confronting Ehrenhaft to the dogmatism confronting Galileo from the Church:

> an iron curtain surrounding Maxwell's equations [that], protect[ed] physics from Ehrenhaft-an iron curtain of precisely the kind that had protected Galileo's opponents. [629]

In this case, Feyerabend thought the orthodox interpretation of quantum theory was being dogmatically shielded from Bohm's proposal. Feyerabend reported to Popper:

> this is only a short note on the course of the conference. It was a success in the sense that the discussions were very informative, well-conducted and the papers very interesting. Körner's paper was a great success. The most obvious thing about the congress was the split between Bohrians (Pryce, Rosenfeld) and the Bohmians (Vigier, Bohm) which led sometimes to riotous discussions in which the Bohmians, fighting *like crusaders* and bringing up the controversy whenever possible, sometimes at quite irrelevant places, were violent sometimes nearly to the extent of being abusive. [630]

However, the scandal, for Feyerabend, was not the rowdiness of the discussions. It was that Bohm's proposal was dismissed *even though* it could recover the predictions deduced from the standard theory (specifically, Einstein and Dirac's equations) so that it was empirically indistinguishable from the established theory. Bohm and Vigier had announced their plans to try to accomplish this in the 1954 paper setting out the this strategy, and Bohm's paper at the 1957 Colston conference delivers on that promise, showing how they recovered the successful predictions. As Feyerabend put it a year later:

> It is the great merit of Bohm's first model to have shown that a formulation of quantum theory is possible which is empirically equivalent with extended quantum mechanics as well as consistent (and which therefore satisfies all those criteria which according to Niels Bohr are sufficient for accepting it as an adequate theory), *and which is yet not observationally complete*. He has thereby shown that the "epistemological turn" of modern physics is not "imposed upon us" by experience, as some physicists seem to believe, but is the result of a choice (which in the case of the adherents of the

[629] Feyerabend, **Killing Time...**, p. 66 [emphasis added].

[630] Feyerabend to Popper, 7 April 1957, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 260.

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> Copenhagen interpretation consists in the acceptance of the Heisenberg program). [631]

The problem, as we shall see, is that Bohm's theory was perceived to be *ad hoc*, because it did not explain anything new, and so was not independently testable. It was designed to recover known successful predictions. Here we can begin to see the close connection between Feyerabend's views in ERE (1962) and Bohm's situation in the foundations of quantum theory, [632] and the impact these events had on shaping Feyerabend's views on theoretical pluralism begin to become very clear. As we shall see, Feyerabend's "plea for tolerance in matters epistemological" is designed directly to promote Bohm and Vigier's attempt to find a realistic explanation of the empirical results of quantum theory, given the transient underdetermination of theory choice by evidence that they had achieved with their proposal, [633] given that the same measurement results can be used to corroborate both theories by being deduced from them. This transient underdetermination was not just like how classical mechanics was related to relativity theory before relativity theory became independently testable. They could not even show how to deduce any predictions that could contradict predictions deducible from the established theory, which is why Bohm did not think they had managed to show that their proposal had achieved the status of a new rival theory.

To Bohm and Vigier, it was clear that interpretations are not uniquely determined by the facts. Vigier explicitly rebuts Rosenfeld on this point:

> Professor Rosenfeld has said that interpretations are uniquely determined by the facts. I think this is also too dogmatic a statement. The gas laws could be interpreted both ways. On the one hand, you have the interpretation according to which a fluid or a gas is a continuous stuff; and on the other hand you have the atomic hypothesis. Here the same mathematical laws cover very different physical assumptions. [634]

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[631] Feyerabend, "Observationally Complete Theories…", p. 490 [emphasis added].

[632] "Even of the great David Bohm himself who says that he is closer to Bohr that anyone else". Feyerabend to Hempel, 20 April 1968 in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, p. 631. Feyerabend and Bohm were working together in this way.

[633] On transient underdeterimation, see Hoyningen-Huene, "Reconsidering the Miracle…".

[634] Vigier in the discussion after Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 50.

This is Vigier, at the conference, illustrating a case in which underdetermination was with the example of classical phenomenological thermodynamics and the kinetic theory based on the atomic hypothesis. As we have seen, Feyerabend used exactly this example to illustrate incommensurable universal theories and how to test them by indirect refutation in ERE (1962). According to Feyerabend, Brownian motion broke the empirical deadlock between these incommensurable (deductively disjoint) universal theories. The idea of incommensurable frameworks arises because prior to the corroboration of Einstein's prediction of Brownian motion, ontologically incompatible rival physical assumptions were used to interpret (by being deduced from them) the "same" lower-level laws and theories in incompatible ways, and yet based on their empirical predictions, they were indistinguishable before the confirmation of Einstein's predictions about Brownian motion that corroborated the atomic hypothesis. Bohm's use of these ideas specifically to try to criticize Popper's falsificationism becomes ever clear as he begins to publish in philosophy of science journals. [635] In 1963, in his simplified summary of ERE (1962), Feyerabend described the situation in general terms:

> The exclusion of alternatives is then simply a measure of expediency: their invention not only does not help, it even hinders progress by absorbing time and manpower that could be devoted to better things. The [logical empiricists'] consistency condition [new theories must be consistent with established views] eliminates such fruitless discussion, and it forces the scientist to concentrate on the facts which, after all, are the only acceptable judges of a theory. *This is how the practicing scientist will defend his concentration on a single theory to the exclusion of empirically possible alternatives*. [636]

This was how the dismissal of Bohm's proposal was justified by adherents to the Copenhagen interpretation according to Feyerabend. At the conference, Bohm himself compared the situation with his proposal in the foundations of quantum theory to the unjustifiable resistance to the Copernican theory In that revolution, the established theory was protected from counter evidence (retrograde motion) by the ad hoc introduction of epicycles. Bohm characterized "the usual quantum

[635] See Bohm, "On Certain Epistemological and Cosmological Problems…"; Bohm, "On the Relationship between Methodology…"; David Bohm, "Classical and Non-Classical Concepts in the Quantum Theory. An Answer to Heisenberg's **Physics and Philosophy**, *The British Journal for the Philosophy of Science* 1962, Vol. 12, No. 48, pp. 265–280, and Bohm, "On the Problem of Truth…".

[636] Feyerabend, "How to Be a Good Empiricist…", p. 91 [emphasis in the original].

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mechanics", as "closer to the epicycle point of view". [637] And, Vigier warned:

> I think it is very dangerous to be dogmatic on those questions. The history of science has shown that sometimes assumptions which have no direct experimental basis can have important experimental results. Professor Fierz has quoted the example of de Broglie, but the atomic hypothesis at the beginning of the nineteenth century were in exactly the same position. [638]

This is Vigier again alluding to how the kinetic theory became independently testable when Einstein's predictions about Brownian motion were confirmed by Perrin, corroborating it, which as we have seen, became the backbone of Feyerabend's argument for theoretical pluralism in contrast to falsificationism in ERE (1962). Vigier is arguing that dogmatically dismissing their proposal because it is not yet independently empirically testable is a dangerous presumption that may be impeding progress, as their proposal may one day become independently testable. *This exemplifies exactly what Feyerabend argues more generally in* ERE *(1962) with respect to theoretical pluralism in methodology*. Furthermore, as we shall see, there are several other points made in these discussions that anticipate central aspects of Feyerabend's views as set out in ERE (1962) and after, especially with respect to his methodological argument for theoretical pluralism, and his test-model for comparing incommensurable theories. More specifically, Feyerabend's ERE (1962) develops several points that Feyerabend made in these discussions, while also pulling together points made by Bohm and Vigier and others. The result, as we have seen, was an argument for theoretical pluralism that was neither based on Popper's falsificationism, nor Bohm's dialectical methodology.

The first time Feyerabend alluded to the methodological implications of Brownian motion as a test of phenomenological thermodynamics in any of his published work is also in this second discussion at the 1957 Colston conference, where Feyerabend initially brought up the example, introducing it into the discussions. As we have just seen, it was then mentioned or alluded to repeatedly by Bohm and Vigier in defense of their proposal. According to Feyerabend:

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[637] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 56.

[638] Vigier in the discussion after Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 49. Feyerabend's contempt for conceptual conservativism and the basic ideas behind his methodological argument for pluralism based on and his pluralistic test model as illustrated by Brownian motion can be found in the discussions at the Colston conference in 1957.

> The first thing that comes to one's mind when one hears the objections of Bohm's opponents [especially Rosenfeld], are objections which were raised, in the nineteenth century, against the atomic theory. Those objections were much more formidable, as they seemed to reveal a contradiction in the idea of a reduction of thermodynamics to the mechanics of a collection of atoms. [...] nevertheless the atomic theory was finally successful and could even be used for showing that the phenomenological account could not be universally correct. [639]

This is the first published passage in which Feyerabend argued that the atomic theory was used to show the limited validity of classical phenomenological thermodynamics by Einstein, who predicted the stochastic character of Brownian motion, and Perrin (among others), who confirmed the prediction of this novel effect corroborating the kinetic theory based on the atomic hypothesis. As we have seen, this became Feyerabend's main illustrative example in methodology for supporting his argument for theoretical pluralism, which shows rival incommensurable theories can be compared empirically despite meaning variance in ERE (1962) through to chapter three of **Against Method** (1993).

In Bohm's **Causality and Chance in Modern Physics** (1957), which Feyerabend had excitedly read shortly before the conference, Bohm had clearly already been using atomism as an example of a theory that had hidden variables when it was first proposed that was eventually successfully corroborated by Brownian motion. Atoms and molecules had been the "hidden variables" analogous to his sub-quantum fluid. Predictions were deduced from the kinetic theory based on the atomic hypothesis that were corroborated by an effect that the existing theory could not have explained. Bohm had even already suggested the main methodological point of the example. The existing theory could neither explain the effect correctly, nor be directly or indirectly refuted by it, because the new theory that is needed to characterize the effect correctly is based on incompatible basic postulates. In fact, as Bohm tried to explain at the conference, his own hidden variables strategy aims to allow for discoveries about the behavior of quanta that are otherwise invisible from within the quantum framework, just as the cause of the stochastic nature of the trajectory of the Brownian particle (shaking molecules) was invisible from within the phenomenological energetist framework. Bohm tried to make these points from his book in the discussions at the Colston conference. As

[639] Feyerabend in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 48–49.

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Feyerabend was already familiar with Bohm's book, it is no wonder that Feyerabend introduced the example into the discussions in defense of Bohm and Vigier's proposal in this way.

By introducing the example into the discussion, Feyerabend was clearly trying to help Bohm and Vigier defend their hidden variables proposal from Rosenfeld's criticism that *it was unhelpful metaphysical speculation* by using the Brownian motion example to show that there was once a hidden variables theory (in this case the atoms and molecules were the hidden variables) that later became independently empirically testable by inspiring new methods of testing. In the discussions, Rosenfeld comes across just as dismissive of this argument defending their strategy as he was of Bohm's achievement that Bohm reported on in his talk. Instead of trying to tease out the methodological components of Bohm and Vigier's psychological sounding rendition of such an argument (which is what Feyerabend later tried to do), Rosenfeld responded by emphasizing that supporters of atomism just got lucky:

> As regards the atoms and the atomic theory which has been invoked by Bohm and also by other people [he clearly means both Vigier who also developed the example in this way in the discussions, and Feyerabend who initially alluded to it to criticize what he took to be Rosenfeld's dogmatic rejection of Bohm's proposal] as an example of a theory with hidden parameters which has been successful, I have to say this: of course there are atoms and therefore people who spoke of atoms before it was possible for us to observe them were very lucky. [640]

Rosenfeld's attitude that *they just got lucky* implies that there were no good methodological grounds for proceeding in this way. For Rosenfeld, this in no way warrants purported criticism of quantum theory that is based on mere metaphysical speculation. This is how, from Feyerabend's perspective, Rosenfeld dogmatically dismissed Bohm and Vigier's attempt to justify positing hidden variables that would explain quantum indeterminism as the result of laws at a lower level, which hopefully sometime in the future would become independently testable, potentially by inspiring new methods of testing the established theory that are otherwise unavailable. By contrast, in Rosenfeld's view, quantum theory should be further developed to exploit its potential, not criticized with metaphysical

[640] Rosenfeld in the discussion after Rosenfeld, "Misunderstandings About the Foundations of Quantum...", p. 54.

speculation.[641] Of course, Bohm had no quarrel with that; with developing the quantum theory to further exploit its potential. However, he did so in the hope that he could find the limits of its validity, and so to discover a problem that only his their proposal could explain.

Bohm and Feyerabend clearly did not think that Rosenfeld appreciated the methodological point behind the example. At the time, Bohm replied to Rosenfeld's response as follows:

> I want to bring up this question of the discovery of atoms. Again I believe this was not a question of luck [as Rosenfeld had suggested]. For the foundation of the atomic theory consisted in the fact that *it was necessary to explain certain difficulties of the large-scale theory*.[642]

Later, Feyerabend wrote ERE (1962). Here, Bohm makes the point that the atomic theory was necessary to explain the difficulties of classical phenomenological thermodynamics. This is exactly the basis of the main methodological point of the Brownian motion example as Feyerabend develops it in ERE (1962). The established theory could never have been refuted directly by the phenomena because *the new theory was necessary to explain the problem for the established theory*. Feyerabend argues that this is how new incommensurable theories can increase the testability of established theories by making new kinds of evidence for testing them available. In this way, Feyerabend's criticism of falsificationism sharpens Bohm's point illustrated by Bohm's example to develop Bohm's argument methodologically by showing why falsificationism cannot take meaning change into account, exposing a tension between formal relations and a contextual theory of meaning, that led Feyerabend to reject the idea that everyday language provides an empirical core that supports our best theories, in favor of the view that everyday language is something that needs to be revised to reflect advancements established by our best theories.

In the discussions at the conference, Bohm also tried to make the situation and his disagreement with Rosenfeld and Pryce very clear:

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[641] This is a harbinger for the dispute between Kuhn and Feyerabend with respect to normal science to come. See Collodel and Oberheim "Introduction", in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**

[642] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum...", p. 57.

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> 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 many important respects — for instance it says there are stationary states — and nevertheless it approaches classical mechanics as an approximation. Now Professor Rosenfeld believes that future progress will be made not by contradicting quantum mechanics but by building upon it as a foundation and perhaps by enriching it and adding new things to it. Perhaps some progress will be made in this way—I would not wish to say it is not a way to do it; but 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. [643]

In the discussions at the conference, Bohm tried to explain several stages in the development of his hidden-variables strategy, as well as his future plans about how to try to further develop their proposal. In contrast to Feyerabend, Bohm did not just talk the talk, he walked the walk. He applied his self-prescribed methodology in the development and execution of his research strategy. Moreover, Bohm's views offer a more fine-grained, dynamic and flexible analysis of the relationship between new theoretical proposals and new kinds of methods that produce new kinds of empirical evidence that can be used to test them. As Vigier emphasized:

> the theory proposed by Bohm and myself will succeed or fall depending on whether or not it fits with experiment. But one should not see the relation between theory and experiment in a too dogmatic way. One should wait until the theory develops itself. If I remember correctly, the verification of the ideas of de Broglie [electron diffraction] came out quite by accident and people were not looking for that effect. [644]

On Bohm's account, the relationship between theory and evidence is neither reducible to logical contradiction and consistency, nor should theories always be introduced based soley on experimental results. The process of developing and testing theories against evidence can progress through several stages before independent testing of them can begin (before those theories are no longer *ad hoc* empirically). The first stage was a proof of concept or pilot project, Bohm's pilot-waves proposal, which tried to show that the location and motion of quanta could be interpreted realistically as having had a determinate location and path, to

[643] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 46.

[644] Vigier in in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 46.

prove that a such an interpretation is in principle possible. [645] The next step was developing the new proposal so that they could recover the successful predictions of the established theory:

> In the original form of this theory, which was quite different, the effort was to re-interpret the wave-function as an objective wave-function, and the particle as an actual particle and to show that you could obtain the same results as in the usual interpretation. [646]

This can be helpful because,

> the new interpretation constituted a broadening of the conceptual framework, permitting the introduction of a wide range of possible modifications, that could not even be expressed in the Copenhagen interpretation. [647]

This is important because:

> very often a different interpretation lends itself to changes and generalizations which *are not possible* in the first interpretation. [648]

In this case, they tried to use these new generalizations to re-explain the experimental results of quantum theory as resulting from a stochastic process caused by a lower-level processes, which is not a possibility according to the Copenhagen interpretation, which takes them to be irreducible. In other words, the new theory is proposed as a potential explanation of the existing theories confirmed predictions. The idea is to try to explain all the successful predictions of the established theory, but without thereby necessarily becoming empirically *equivalent* to it — just temporarily empirically indistinguishable from it, in the hope that eventually these new generalizations lead to the development of a crucial experiment based on the prediction of a novel effect deduced from the new theory that the new theory can explain, but that its rival cannot. Bohm clearly emphasized that his proposal was supposed to contradict quantum theory on a fun-

[645] By new interpretation, Bohm means a new interpretation of the results, that is, a new theory.

[646] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 60.

[647] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 270.

[648] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 61 [emphasis added].

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damental level: "I always supposed that at a deeper level there will be a *fundamental contradiction* between this theory and quantum theory". [649] Note this formulation implies that one theory is not supposed to be a deductive consequence of the other, nor merely that some of the corresponding predictions of the theories should formally contradict each other (per falsificationism), but that the contradiction is somehow more "fundamental", not as if there were somehow degrees of contradiction, but rather as if the contradiction is between the assumptions underlying the basic concepts used to state the theories. Bohm's awkward formulation seems to reflect Feyerabend's distinction between contradicting a theory's predictions and being inconsistent with its ontological principles (compare Einstein's notion of the basic postulates of the rival theories). Put another way, Feyerabend can be seen to be trying to capture Bohm's point about the fundamental contradiction between the theories with his account of the incompatibility of rival incommensurable theories with respect to the incompatibility of their ontological principles. The next step for Bohm and Vigier was going to be to try to produce testable predictions of a qualitatively novel effect, either of a known curiosity, like quantum indeterminacy itself, or of a new effect that the established theory cannot explain. The confirmation of the prediction of a novel effect could undercut the ontology of the existing theory by corroborating a rival that has an incompatible one, showing that the existing theory, by itself, could never have found out that the anomalous phenomenon is in fact a novel effect that refutes it.

The trick is finding something *only* the new theory can explain. To this end, Bohm went on actively trying to find problems with the existing theory by deducing counter-intuitive experimental results from it. For example, the historic paper published by Bohm and Aharonov in 1959 the tension at the heart of the measurement problem with respect to the indefinite path of the quanta until the interaction during the measurement. [650] If the path is indefinite, then it must be the same indefinite path for paired quanta, as a magnetic field that can only locally affect the path of one, also affects the path of the other. This counter-intuitive effect was deduced from the quantum theory and experimentally confirmed, setting up the discovery of Bell's inequalities. This work is usually considered to be Bohm finally just shutting up and calculating, but it is often overlooked that Bohm formulated

[649] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 55.

[650] See Aharonov and Bohm, "Significance of Electromagnetic Potentials…".

this step as part of his larger methodological strategy of finding the limits of the existing theory. Aharonov and Bohm (1959) could be understood as an attempt to probe the edges of quantum theory, a very fruitful failed attempt to find its boundaries, that is, where it might break down experimentally, perhaps even as a guide to finding a new effect that can only be explained by his rival proposal (similar to how the EPR thought experiment backfired).

Feyerabend's view that everyday language should be corrected according to our current best theories in ERE (1962) also comes across very clearly in these discussions. [651] Feyerabend tried to challenge Rosenfeld directly on this point. Rosenfeld responded:

> Feyerabend said that there is an arbitrary assumption in quantum theory, namely the assumption that the concepts must be defined in a classical way. Well, it is an assumption, if you like; but isn't that an obvious thing? How could you do it otherwise? [652]

Rosenfeld seems simply to assume that classical concepts are fixed independently of theory and are necessary to interpret the empirical results deduced from the quantum theory. This harkens back to Heisenberg's contention that:

> The concepts of classical physics are just a refinement of the concepts of daily life and are an essential part of the language which forms the basis of *all natural science*. Our actual situation is that we *do* use the classical concepts for the description of experiments [...] There is no use discussing what could be done if we were other beings than we are. [653]

Rosenfeld also defends Bohr on this point:

> He [Bohr] just uses classical language for this kind of experience because classical language is our only tool for describing this kind of experience although its application is limited by laws of nature which are independent of us. [654]

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[651] See also Feyerabend, "An Attempt at a Realistic Interpretation of Experience…".

[652] Rosenfeld in the discussion after Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 52.

[653] Heisenberg as cited by Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 273. See also Werner Heisenberg, **Physics and Philosophy...** Compare Popper, "A Defence of Free Thinking in Quantum Theory (1953)…", pp. 429–474.

[654] Bohm in the discussion after Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 61. As we shall see, Bohm ("Classical and Non-Classical Concepts in the Quantum Theory…") also attacks Bohr's contention directly. In 1953, Popper had criticized Bohr's contention that

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Bohm and Feyerabend were trying to disagree. Bohm thinks that different kinds of concepts can be used to describe experience, and as we shall see, he recommends taking topological concepts as fundamental both in science and everyday experience. As we have seen, in ERE (1962), Feyerabend also rejected Bohr's claims about the irreplaceability of classical concepts, comparing the idea to Popper's idea of basic statements. Feyerabend's Wittgensteinian view is that everyday language is foggy because it was designed to express antiquated scientific principles that belonged to earlier worldviews. Concepts are vague and can be ambiguous with respect to interpretation. The same sentences can mean very different things in the same material context, given incompatible interpretations, even by the same individual at different moments (if you have not been trained to keep the sentences and the rival theories used to interpret them together as separate packages, it can get philosophically foggy very fast). That is why a formal account of explanation is impossible for Feyerabend. In everyday language, we continue to use the same terms to mean very different things, just as in science the same terms take on very different meanings in some of the same context after revolutions. For Feyerabend, the meanings of the terms used in everyday language should be revised because they hold us back. After all, they were introduced to express antiquated principles of former universal theories, which support incompatible worldviews. [655] Everyday sentences couched as basic statements do not provide a neutral language that supports formal relations. Everyday sentences should be interpreted according to the theory they are deduced from and used to test so that they make incompatible statements after revolutions.

Feyerabend's idea is that as "soon as we have a new theory we have to use it throughout, for the description of experience as well as of anything else". [656] We

classical concepts will forever remain irreplaceable as part of what Heinsberg calls "closed theories", which Popper also criticized because that would make them untestable. See especially Popper, "A Defence of Free Thinking in Quantum Theory (1953)...", p. 472 n. 33 and Feyerabend, "Observationally Complete Theories...", pp. 487–492.

[655] Compare Popper, who had emphasized that: "What we call experience is always the result of interpretation, in the light of theories (including myths)" (Popper, "A Defence of Free Thinking in Quantum Theory (1953)...", p. 443), and that "even the most ordinary experiences assume their peculiar significance from theories — often theories of a magical character" (Popper, "A Defence of Free Thinking in Quantum Theory...", p. 443).

[656] Feyerabend in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum...", p. 52.

should admit that the earlier theory was mistaken, and replace it and its concepts with better concepts. According to Feyerabend:

> The natural procedure is to say that so far our experiences have misled us into believing that length is a property rather than a relation involving a co-ordinate system; and to say and improve the situation by describing even the results of experiments in terms of the new theory of relativity rather than in terms of a previous theory (for example, Newtonian theory.; which did not know anything about the relative character of length). [657]

In contrast, Feyerabend casts the Copenhagen interpretation as committed to:

> describing experience not in terms of the best theory available [relativity theory] but in terms of some previous theory [classical theory] which has become so intimately connected with our ways of experiencing things that we can hardly imagine things being different. [658]

Feyerabend tried to explain his peculiar views about meaning and everyday language to the physicists at the conference, testing out his ideas about the methodological implications of meaning change. He must have been encouraged to find that Bohm supported his suggestions, especially on meaning variance and basic concepts, which can be seen when Feyerabend used the concept represented by the term "length" and the special theory of relativity to try to explain his views:

> Take the special theory of relativity. According to this theory the concept of length is a relative concept in the sense that you cannot attribute length to a given object without at the same time referring to some co-ordinate system. Now before the special theory was introduced, length was regarded as a property of an object. [659]

Feyerabend is claiming that the definition of length has changed from a prop-

[657] Feyerabend in the discussuin after Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 53. Note that Hutten disagrees with Feyerabend, suggesting we use classical physics as a metalanguage. Hutten explicitly suggests "if you have a new language and introduce new ideas you must first express them in a language you know. Feyerabend often rejected such suggestions, pointing out that new theories and languages can be learned in their own terms from scratch, like how a child learns its first language". Compare, for example, Feyerabend, "Linguistic Arguments…".

[658] Feyerabend in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 53.

[659] Feyerabend in: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 52.

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erty of an object that is independent of a coordinate system to a relation that depends on an object's relative motion with respect to the observer within a specified coordinate system. In a clear show of solidarity, Bohm explicitly supported Feyerabend on this point:

> I agree with what Dr. Feyerabend has said about using a new theory to modify the definition of older concepts. [660]

According to Bohm, using new methods of measuring can lead to changes in the concepts measured:

> This is one of the lessons of the theory of relativity. In the theory of relativity we use measuring rods and clocks which behave in a certain way, *and we must change our conception of position in accordance*. [661]

As we have seen, this is a major point Feyerabend emphasizes throughout ERE (1962): Older basic concepts are and should be replaced. Antiquated statements are not explained by contemporary theories. The same sentences are given new meanings by new theories and the new methods used to test them. Bohm also briefly tried to clarify what Feyerabend had asked about earlier with respect to their successful "derivation" of the Dirac equation. Bohm tried to clarify that he had not meant to suggest a formal deduction of the equations from his theory, but was meant in the sense of deducing and thereby recovering the successful predictions as an approximation in the sense of a limiting case: "With regard to the *derivation* of the Dirac equation, this was meant in the sense of an approximation or a limiting case". [662]

Bohm's response to Feyerabend's point that new theories re-define even basic concepts like "length", which was once thought to be a property of an object, but as it turned out, is now taken to be a relation between an object and its relative motion, also includes what can be read as a striking anticipation of Feyerabend's "ever-expanding ocean of alternatives" understanding of progress based on his

[660] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 55.

[661] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 57 [emphasis added].

[662] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 55.

contrastive account of empirical content. When Bohm bolstered Feyerabend's point about "length" and tried to make his views clear with the example of "position", he said:

> I want to raise the question of how we are to define a thing or the conception of a thing, such as position and so on. This was raised by Dr. Feyerabend. I believe that we must define a property such as position in terms of all the concepts that are available. During the Middle Ages or before that time people were not very successful in defining positions accurately; then, when geometry developed, they were better able to define positions. Then with the development of mechanics and wave optics, it was possible to do this still better, for one could use mechanical and optical parameters and so on. And now if we should come to a sub-quantum level then we could define position in terms of the parameters of the sub-quantum level. In other words, the conception of position like all conceptions, may have infinite possibilities as we go deeper and deeper into nature and find out more and more of what is there. [663]

Bohm thinks that "this is really a fundamental way by which progress in science is made: difficulties of conceptions at a certain level are resolved by suggesting a new conception". [664] This sounds very much like Feyerabend's understanding of revolutionary scientific advance. But Bohm puts a dialectical twist on it that Feyerabend is not willing to accept. For Bohm, the new conception and concepts are dialectically opposed to the views that preceded them: "This again is characteristic of our concept. Each particular concept finds its reflection in its opposite". [665] Bohm explicitly emphasized the dialectical nature of his methodological views at the conference:

> I think that it is part of the cause of the difficulty inherent in the conception of probability that it does not apply with complete correctness any more than the conception of perfect determinism applies in this way. Rather, both of these conceptions must be regarded as opposite approximations to the same problem. [666]

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[663] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 57.

[664] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 57.

[665] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 57.

[666] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 57.

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Bohm tied his characterization of the positive results of his new approach as presented in his paper very closely to his dialectical methodology:

> from one conception you can deduce the opposite conception as an approximation, so that in a certain sense one conception implies the other. This is what happens here: from the continuity of the phases of the particles that are coming out of the background of the sub-quantum-mechanical level I deduce precisely the Einstein-relation and the de Broglie-relation for energy and momentum... I do not wish to say by this that only continuity is a correct concept and that discontinuity is wrong; I want to say that both conceptions are correct but that each one reflects only a certain side of a phenomenon. Each one is reflected into the other, so that you can deduct the one from the other as an approximation. [667]

While Feyerabend rejected Bohm's dialectical approach, these discussions at the Colston conference in 1957 clearly set the stage for the views Feyerabend developed in his publications on empiricism in the early and mid-1960s. Taken together, the ideas that were discussed anticipate Feyerabend's view of the relationship between theories and the statements used to test them, his view of the relationship between everyday language and scientific theories more generally, and even more specifically, his view that new theories *changes the definitions* of the concepts used to state *and test them.* Feyerabend and Bohm's examples of meaning variance in the terms used to state the measurement results are precisely the kind of terms Popper envisions as providing a shared observation language couched in everyday terms according to his falsificationist account of basic statements. The tension for Popper's account can clearly be seen in Popper's criticism of Bohr on the irreplaceability of classical concepts. [668] Popper is not sure what to make of Bohr's complementarity view because he is not sure whether it is testable. On the one hand, Popper agrees wholeheartedly that the new theory can give different meanings to the basic statements, which should themselves be testable, on the other hand, he posits formal logical relations between these agreed upon basic statements (despite the meaning variance). This seems to imply that they get their meaning either bottom up from the observations or from

[667] Bohm in the discussion after: Rosenfeld, "Misunderstandings About the Foundations of Quantum…", p. 55.

[668] See Karl R. Popper, "Complementarity and the Two-Slit Experiment (1953)", in: Matteo Collodel and Eric Oberheim (eds.), **Feyerabend's Formative Years, Volume 1, Feyerabend and Popper,** *Vienna Circle Institute Library*, Correspondence and Unpublished Papers, Springer, Venice — Berlin 2024, pp. 423–428.

everyday use, and not from the theories from which they are deduced to be used to test them, nor (presumably)from the theories used to try to test them. That way agreed upon basic statements deduced from the rivals can logically be consistent and inconsistent. Feyerabend rejects both possibilities, insisting the observation statements must get their meaning from the theories they are deduced from for the inferences to be valid. Moreover, for Feyerabend, this gives the existing view an unfair advantage over new proposals and would lead to the entrenchment of everyday views to the point that they become untestable, turning the empirical core of Popper's empiricism into untestable dogma. To repeat: Popper was aware and even emphasized that basic statements are theoretical, [669] but according to Feyerabend, Popper's view still (inadvertently) gives them a privileged status with respect to the theories they are used to test that they do not deserve. Popper emphasizes that formally speaking, statements about particulars can always be more secure than general statements. Feyerabend's complaint is that ironically, Popper's account makes them harder to test and therefore less secure.

3.5 Feyerabend's Criticism of Observationally Complete Theories (1958)

In 1958, Feyerabend criticized the quantum theory for being what he called an "observationally complete theory". [670] He was trying to show that quantum theory

[669] Popper emphasizes meaning variance in everyday terms in his, "A Defence of Free Thinking in Quantum Theory (1953)…", p. 442f.

[670] See Feyerabend, "Observationally Complete Theories…", pp. 487–492. Popper had convinced Feyerabend not to publish this paper: "I am sorry to say that it shocked me. You are strangely illogical: your summary contradicts the contents of your own paper" (Popper to Feyerabend, 5 May 1958, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 281). Feyerabend eventually reported to Popper: "I have withdrawn my paper to the Venice Congress as your criticism has shown me that it possesses serious shortcomings. Bohm has been very ill and he has still not recovered" (Feyerabend to Popper, 7 June 1958, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 286). Popper obviously does not seem to have understood Feyerabend's point which did not seem to make sense to him at all (as with his "Explanation, Reduction and Empiricism…" [1962]), when it seems rather that Feyerabend's criticism is very similar to Popper's own criticism of Bohr's complementarity view for being seemingly untestable, as well as Bohm's concerns with Bohr's complementarity view, see Bohm, "Classical and Non-Classical

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by itself lacks the resources to explain quantum indeterminacy (which is what Bohm was trying to explain by positing a sub-quantum fluid). In fact, as we have seen, Brownian motion, is in effect, Bohm and Vigier's "Model of the Causal Interpretation of Quantum Theory in Terms of a Fluid with Irregular Fluctuations" in quantum theory *and in methodology*, as further developed in Bohm's "Proposed Explanation of Quantum Theory in Terms of Hidden Variables at a Sub-Quantum-Mechanical Level" (1957) and during the subsequent discussions at the Colston conference. [671] This was the concrete situation that Feyerabend's account in general methodology was supposed to model. Feyerabend was trying to show that quantum indeterminacy is doomed to remain an anomalous phenomenon, until a rival incommensurable alternative is developed that can explain it, just like was the case of Brownian motion on the classical level before 1906. In other words, Feyerabend was trying to show that quantum theory could never explain quantum indeterminacy by itself, because it is "an observationally complete theory" that leaves no room for such an explanation. To understand this intuitively, consider von Neumann's measurement problem in its most simple qualitative form: Where is the quanta directly before the interaction used to make a measurement? The paradox that arises from quantum theory with respect to this measurement is that it is impossible to say, because there is no definite answer, because it is in a probability space until the interaction, so one cannot know its definite state directly before the interaction, because there is no definite state until the interaction. This is the paradoxical implication of quantum theory that Schrodinger's cat was designed to illustrate. Einstein's letter to Heisenberg about Schrodinger's cat in 1953 makes the point succinctly. [672] Feyerabend's 1958 paper on "Observationally Complete Theories" should be read as a philosopher's way of developing this methodological problem that arises from this aspect of quantum theory. This is another case in which Popper simply seems not to have understood what Feyerabend was trying to say.

The impact Bohm had on Feyerabend with respect to the genesis of Feyerabend's argument for theoretical pluralism in contrast to Popper's as set out in ERE (1962) becomes very clear in a letter Feyerabend sent to Agassi between the

Concepts in the Quantum Theory…".

[671] Körner and Pryce (eds.), **Observation and Interpretation...**

[672] Albert Einstein, "Letter to Erwin Schrödinger, December 22, 1950", Albert Einstein Archives, Hebrew University of Jerusalem.

2nd of April and the 9th of May, 1958 in which Feyerabend made his new Bohmian views on this issue very clear:

> I just sent Karl a long MS by Bohm which he wants you to read too. Please return it as soon as you have finished. Bohm + I are both looking forward to the next issue of B.J.P.S., containing your article on a "Hegelian view of Complementarity" and Bohm has ordered the journal from the publishers. We also discussed your [Popperian] criticism of Bohm, contained in your last letter to me, that his theory has an inbuilt mechanism to escape refutation. *Bohm says, and I am here completely in agreement, that any theory which is complicated enough will contain such an inbuilt mechanism*. [673]

That in-built mechanism is the universal conceptual framework can that conditions all experience according to it so that the anomalous character of a recalcitrant observation can go completely unnoticed coupled with the possibility that theories can also always be protected from recalcitrant measurements with *ad hoc* hypotheses (Einstein also emphasized this point shortly before using the term "incommensurable" in conjunction with his remarks on comparing universal framework theories)". Confronting this problem was an important step in genesis of the views Feyerabend sets out in ERE (1962). It led Feyerabend to his leading question: How can universal theories be tested by experience if they can condition all possible experience that can be used to test them and be protected from refutation by *ad hoc* hypotheses? Furthermore, it led Feyerabend straight to Bohm's answer, which as we have seen, is one of the main implications of Feyerabend's criticism of Popper's methodology in criticism of Popper's views in ERE (1962).

By May 1958, Bohm and Feyerabend had clearly become close discussion partners, and Feyerabend was very explicit with respect to who had emphasized to him that classical mechanics was not falsified, but superseded by a better corroborated rival, as set out in his conjectures and novel corroborations test-model from ERE (1962) to **Against Method** (1993):

> Another point which Bohm made is that falsification does not play such an important role — classical mechanics was not given up because it was falsified, but because another theory was more successful. [674]

[673] Feyerabend to Agassi, April/May 1958, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

[674] Feyerabend to Agassi, April/May 1958, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

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Here is incontrovertible evidence of Bohm's deep influence on Feyerabend's argument for, and account of, theoretical pluralism, as set out in part 1, which clarifies Feyerabend's conjectures and novel corroborations in contrast to and as part of a criticism of Popper's methodology of conjecture and refutation by falsification. [675] Bohm's deep influence on Feyerabend's methodological views led to Feyerabend's ERE (1962), which is what eventually led to his break from Popper's school in 1967.

3.6 Feyerabend's Criticism of Bohm's Dialectical Methodology (1959)

In 1959, Feyerabend finally finished his critical review of Bohm's book **Causality and Chance in Modern Physics** (1957), which was then published in 1960. [676] It is explicitly a criticism of Bohm's methodological views from a *falsificationist* perspective. According to Feyerabend, the problem with Bohm's dialectical methodology is that it implies that: "Nature itself forces man to speak the truth, and it also forces him to speak in such a way that his theories have objective reference". [677] Generally speaking, Feyerabend emphasizes necessity in Bohm's dialectical account, which he adamantly rejects. According to Feyerabend, Bohm's view is that:

> Knowledge is a natural process which leads to a mirroring, in the head of man, of the properties of the universe. The mirror-image may be distorted at the edges [but] this distortion does not reach into the center of the mirror which perfectly represents the situation at a certain level. [678]

[675] Collodel, "Was Feyerabend a Popperian…", pp. 27–56 does not appreciate Bohm's influence on Feyerabend's deteriorating relationship with Popper, because he does read, "Explanation, Reduction and Empiricism…" (1962) as an attempt to criticize Popper's methodology, as it was intended.

[676] See Bohm, **Causality and Chance…** and Feyerabend, "Professor Bohm's Philosophy of Nature…". *Finally*, as he first read it in 1957 shortly before meeting Bohm at the Colston conference, and as can be seen in his correspondence, he had not really gotten started with the review before December 1958.

[677] Feyerabend, "Professor Bohm's Philosophy of Nature…", p. 335.

[678] Feyerabend, "Professor Bohm's Philosophy of Nature…", p. 335 [emphasis added].

To Feyerabend, Bohm's account seems to imply that each theory gets something essentially right at a certain level within certain limits. Feyerabend explicitly rejected what he took to be,

> the epistemology behind Bohm's belief that every theory, however absurd it may seem at first sight, has some kind of truth in it and correctly mirrors what exists in the universe. [679]

He was also very clear about why he rejected Bohm's dialectical methodology: "I do not believe that this account of knowledge is a correct one [...] clearly Hegel's notorious »Alles Vernünftige ist wirklich«". [680] This was the eve of Feyerabend's draft criticizing Popper's methodology in 1960, and the *last* time Feyerabend explicitly endorsed *falsificationism* (not critical rationalism), quite prominently placed in the concluding sentences of his review of Bohm's book: "I cannot accept Bohm's methodology [...] I prefer [...] the methodology of falsification as it has been developed by Popper". [681] As we have seen, in his subsequent publications, Feyerabend continues to endorse critical rationalism as a philosophy, but in 1960, he launched his criticism of Popper's methodology published in ERE (1962), which led to his break with Popper's school in December 1967.

3.7 Bohm's Criticism of Falsificationism (1959–1961)

In 1959, Bohm wrote "On Certain Epistemological and Cosmological Problems

[679] Feyerabend, "Professor Bohm's Philosophy of Nature...", p. 337.

[680] Feyerabend, "Professor Bohm's Philosophy of Nature...", p. 337. Feyerabend is referring to Hegel's dictum. See Georg Wilhelm Friedrich Hegel, **Elements of the Philosophy of Right**, Duncker und Humbolt, Berlin 1820. He complained about Kuhn's philosophy alluding to the same dictum, see Hoyningen-Huene, "Two Letters of Paul Feyerabend ..." and Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, Part 3. The affinities between Kuhn's "*essential* tension" between tradition and innovation and Bohm's dialectical methodology with respect to necessity is clearly something that repelled Feyerabend from both of their accounts.

[681] In any case, after Feyerabend and Bohm parted ways, while their direct engagements seem to have ended, there was still a rich and lasting overlap in the subsequent development of some of their ideas (see Feyerabend's **The Conquest of Abundance**). The many affinities in their ideas brought them together, and as we have seen, Bohm brought the Brownian motion example to Feyerabend's attention, and it was Bohm who suggested to Feyerabend that falsificationism cannot take account of it.

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Raised in Dr. Feyerabend's Review of **Causality and Chance in Modern Physics** (1959)". [682] This was a hefty 38-page (in its original typescript) response to Feyerabend's Popperian critical review of Bohm's **Causality and Chance in Modern Physics** that remained unpublished, until in 2024. [683] The entire essay is structured as critical comparison of his own methodological views that he calls "the qualitative infinity of nature" with Popper's falsificationism (as Bohm understood it). The goal of this section is to show how Feyerabend's criticism of Popper's methodology in ERE (1962) was directly developed following Bohm's 1959 attempt to criticize Popper's methodology and Bohm's first publication in a philosophy of science "On the Relationship between Methodology in Scientific Research and the Content of Scientific Knowledge" (1961). Feyerabend sharpens Bohm's argument in the dialectical way that Bohm suggests and slightly redirects it so that it applies to its intended target. In this way, Bohm helped impregnate Feyerabend with Popperstein's baby. Bohm's criticisms of what he took to be Popper's methodology motivated and framed Feyerabend's criticism of Popper's methodology in ERE (1962), which is what eventually led to his break with Popper's school in December 1967.

In response to Feyerabend's critical review, Bohm introduced his idea of the "qualitative infinity of nature", [684] especially in its treatment of the relationship of human thinking to the cosmos and of questions about the meaning of truth in the context of theories have only a limited domain of validity, explicitly in contrast to Popper's falsificationism. [685] Bohm succinctly presented his dialectical view of progress, illustrated by the relation between universal theories such as relativity theory and Newtonian mechanics:

> every law has a limited domain of validity, these limits being determined objectively by the effects of domains that this law necessarily leaves out. When a theory is first proposed, these limits are not known, but eventually their existence is disclosed, when contradictions between theory and further experiments are discovered. Later, these limits are defined more precisely in terms of newer theories, which explain the

[682] See Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, Part II, Chapter 2.

[683] See Bohm, **Causality and Chance...** and Feyerabend, "Professor Bohm's Philosophy of Nature...".

[684] For Bohm's methodological views, see also Bohm, **Causality and Chance**, Ch. 5.

[685] See Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, Part II, Chapter 2.

> new experimental facts (e.g. as relativity defines the limits of validity of Newtonian mechanics as the domain of velocities small compared with that of light). [686]

According to Bohm, classical mechanics was not falsified. It was shown to have a limited validity by a more general theory, which explains new experimental facts, which itself in turn will one day be shown to have only a limited validity. Bohm explicitly rejects Feyerabend's characterization of him as a dialectical materialist in the sense that progress is determined by historical forces, or as a determinist according to which nature forces us to speak the truth about it. [687] Bohm never meant to suggest there is a necessity in how science develops historically, nor that the world forces us to speak the truth about it.

As we shall see, Bohm also set out his views on objective and subjective error and the meaning of truth in contrast to Popper's falsificationism. Less than a full year before Feyerabend began ERE, which as we have seen, calls for the dissolution of traditional philosophical problems like the mind-body problem by allowing meanings to change, Bohm (directly responding to Feyerabend's critical essay review) had suggested that ideas held by individuals cause events rejecting the unfruitful separation of mind from matter:

> It must now be emphasized once again that in the point of view under discussion here, ideas, emotions, desires, etc., are to be regarded as parts of reality, which belong to the psychological level of human beings. Thus, an idea held by a certain individual is just as much a real and essential part of the cosmos, as is, for example, a molecule. Both can be causes of future developments, both are to a certain extent the result of past causes, both have a certain limited degree of autonomy, etc. In other words, both are real, and in certain ways, irreducible links in the chains of cosmic development. [688]

When Bohm tries to criticize falsificationism by contrasting it to his own methodological proposal of the *qualitative infinity of nature*, [689] he clearly implies

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[686] See Collodel and Oberheim, **Feyerabend's Formative Years, Volume 2, Feyerabend, Logical...**, Part II, Chapter 2.

[687] See for example, Peat, **Infinite Potential...**

[688] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 246.

[689] For Bohm and Feyerabend, it was "evident that the part played in scientific method by already known laws will also eventually be played by laws which are not yet known, but which will come to be known in the future [. . .] Laws will be replaced by different laws that state the facts in a different form [forever]" (Bohm, "On the Relationship between Methodology…", pp. 107–108).

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that falsificationism cannot account for relating and testing framework theories with each other. It can only work within framework theories. [690] This should sound familiar. It also exposes part of the Bohmian nature of Feyerabend's criticism of falsificationism by showing how Bohm's criticism of falsificationism anticipates how Feyerabend develops his argument against falsificationism in ERE (1962). According to Bohm, "all the terms [...] depend for their clear meaning on what may be called a general framework of concepts, ideas, procedure, etc., which function together as a coherent whole". [691]

Bohm seems to be assuming some kind of contextual theory of meaning, as he continues directly:

> Against a background in which this general framework applies, one can consider specific problems *which do not call the framework into question*. [692]

Bohm is implying something near to what Kuhn claims about normal science. For Bohm, the method of conjecture and refutation only works within frameworks, whereas things get more complicated when framework theories are compared with each other than testing rival lower level theories frameworks. In this way, just as for Feyerabend, for Bohm there are two types of testing procedures of which falsificationism can only be one. Bohm could not have made it more clear that he was trying to criticize Popper's falsificationism with this distinction in this way:

> In order to bring out this point in more detail, let us consider briefly Professor Popper's well-known thesis, that whereas a hypothesis claiming to have universal validity cannot be verified by any number of confirmations, it can be refuted by one falsification. [693]

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[690] As we shall see, Bohm also argued that falsificationism can even fail within frameworks with respect to questions that arise at their borders, and with respect to questions about the relations between, and potential laws governing their relations: "it is interesting that this intermediate domain is just about that of the order of size of virus particles, which are on the borderline of life. [...] It is well known that these are essentially large molecules, having some quantum mechanical properties, and yet coming dose to the classical limit" (Bohm, "On the Relationship between Methodology...", p. 115).

[691] Bohm, "On the Relationship between Methodology...", p. 103 [emphasis in the original].

[692] Bohm, "On the Relationship between Methodology...", pp. 103–104 [emphasis in the original].

[693] Bohm, "On the Relationship between Methodology...", p. 104.

Bohm then agrees with this, even suggesting that scientists generally apply the scientific method intuitively, *within a framework*. Feyerabend also accepts falsification for commensurable theories (that is, within the same framework). However, Bohm, like Feyerabend, also criticizes falsificationism:

> Nevertheless, *the question of proper method becomes ambiguous under certain circumstances. This happens especially in the development of new sciences* [crises preceding revolutions], and in fields that are on the border line between science and something else (e.g., psychology, sociology, etc.). Even in traditionally scientific fields, however, the question of method sometimes becomes unclear. [694]

The italicized clause is Bohm suggesting what Feyerabend tries to argue in ERE (1962). Falsificationism *does not seem to be capable of taking* scientific revolutions into account. Bohm continues by arguing that independently untestable hypotheses should be allowed because by providing new forms of understanding, they can make new facts available that otherwise are not available.

> it follows that even a [independently untestable hypothesis like hidden variables] can help organize existing facts into a coherent whole and determine new forms in which relevant facts can further be developed. [695]

By now, this should sound very familiar. As we shall see, Bohm is discussing the point Feyerabend used to challenge what he later called the "autonomy principle", which Feyerabend characterized as his deep insight with respect to theoretical pluralism in his simplified summary of the views set out in ERE (1962) one year later. [696] In other words, Bohm had made this point in support of his own proposal in the foundations of quantum theory, which Feyerabend then developed and illustrated in general methodology with Bohm's Brownian motion example. Both Bohm and Feyerabend were arguing that the Brownian motion example shows that alternative proposals should always be welcome because sometimes the crucial observations that undermine an established theory only become evidence after a new theoretical proposal is used to interpret them correctly. They were both arguing that more tolerance toward new proposals could promote progress by making more facts available. They were both also argued that

[694] Bohm, "On the Relationship between Methodology…", p. 103.

[695] Bohm, "On the Relationship between Methodology…", p. 108.

[696] See Feyerabend, "How to Be a Good Empiricist…".

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progress is being stifled by overly restrictive methodologies that are based on illegitimate philosophical prejudices. In Feyerabend's terms, currently independently untestable conjectures ("hidden variables") can increase the possibility of discovering new kinds of facts, which then may increase the testability of established views, by corroborating ontologically incompatible alternatives. For both Bohm and Feyerabend, facts are *manufactured* or constructed within conceptual frameworks. Different frameworks make it possible to refer to the different sets of facts that can be stated within them. In this respect, both Bohm and Feyerabend are adopting a Kant-on-wheels account of facts, while arguing that falsificationism only has a limited validity. Moreover, they both limit its validity in the same way: Falsificationism works for testing *within* frameworks, but not for testing frameworks against each other. According to Bohm:

> Within this limited domain [of a framework theory], it is clear how one is to go about finding the facts, setting up hypotheses, testing them, etc. But as we approach the edge of such a domain, these questions become less clear. [697]

In this sense, Bohm's framework theories act like Kuhn's paradigms in The Structure of Scientific Revolutions (1962) and Feyerabend's "universal" theories. And again, seen in this light, in ERE (1962), Feyerabend was clearly trying to develop and focus Bohm's criticism of falsificationism. In Bohm's example,

> an object in a vacuum will fall at a constant velocity can be tested by an experiment, aimed at either falsifying or confirming it. This is because a very large number of things can already be accepted... *Within this framework*, it is possible, so to speak, to ask of Nature a question which has a definite "yes" or "no" answer. [698]

Bohm emphasized that,

> it is possible not only that the concepts arising in the physicist's field of specialisation

[697] Bohm, "On the Relationship between Methodology…", p. 104.

[698] Bohm, "On the Relationship between Methodology…", p. 113. [emphasis in the original]. On Bohm and Hegel and binary oppositions, see for example Bohm and Peat, **Science, Order, and Creativity…**, pp. viii and 299f. Compare Peat's description of Bohr's belief that, "while the opposite of truth is an error, the opposite of a great truth is another great truth" Peat, **Infinite Potential…**, p. 184. According to Peat, "this maxim even influenced the way Bohr wrote his scientific papers. Bohr would agonize about the best way to express a particular idea in a given sentence; then, when he had written it down, he would attempt to express its exact opposite in the sentence that followed" (Peat, **Infinite Potential…**, pp. 184–185).

> are not sufficient for a complete treatment of all aspects of Nature, but also, that these concepts may not even be completely adequate for relating all the facts that might arise in the physicist's own field. [699]

That is why sometimes hidden variables must be introduced. They can make new relevant facts that are otherwise unavailable available. Bohm is claiming that questions of truth and explanation *are relative to forms of the facts furnished by the framework*, which is exactly what Feyerabend's Wittgensteinian constructivism implies in ERE (1962). According to Bohm: "Thus, guided by different conceptions, one is led to seek different kinds of facts, some of which may be possible in a given field, *and some not*". [700] In 1963, Feyerabend then developed these views as what he took to be one of his most significant insights in his early philosophy of science, where he introduced the phrase "autonomy principle", according to which, "facts exist, and are available independently of whether or not one considers alternatives to the theory to be tested" to represent what he was rejecting. [701] And as we have seen, Bohm had also already suggested that,

> there may be a large range of facts that we simply do not look for, because there is no room in our general framework of thinking to express these facts. [702]

Bohm and Feyerabend were both arguing that there was no room in the classical phenomenological framework to express the fact that Brownian motion was caused by atoms. Moreover, Bohm developed this point in methodology as part of a criticism of how falsificationism itself shapes our thinking with respect to prediction (in contrast to his use of retrodiction). [703] In other words, Bohm illustrated this this methodological point with his scientific research: "Facts of this kind

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[699] Bohm, "On the Relationship between Methodology…", p. 113. Bohm uses the example of the question: "have you stopped beating your wife?" Bohm, "On the Relationship between Methodology…", p. 105).

[700] Bohm, "BOHM, "On the Relationship between Methodology…", p. 106. [emphasis in the original].

[701] Feyerabend, "How to Be a Good Empiricist…", p. 91; see also Feyerabend, "Das Problem der Existenz theoretischer Entitäten…", pp. 16–49.

[702] Bohm, "On the Relationship between Methodology…", p. 110.

[703] Bohm's 1952 proposal involves retrodiction, which Popper discussed at length in his 1953 lecture "Complementarity and the Two-Slit…".

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[retrodictive] must escape our conceptual net as long as we think only in terms of predictions of future events on the basis of initial conditions". [704]

Feyerabend was clearly also following Bohm's suggestion that Popper's falsificationist methodology has only a "limited validity". Bohm had pointed out that:

> The problem then immediately arises as to how we can prove the *universal applicability* of the *methodology of falsification*. As in the case of the infinity of nature, the mere agreement of this method with a broad domain of historical experience in scientific research could not provide such proof. Nor does the method itself indicate any way to prove its correctness, or indeed, to indicate that it is false (*not universally valid*) if, in fact, this latter should really be the case. [705]

With this Bohm had pointed to show that he appreciates the normative character of Popper's methodology, while intimating that this makes it untestable by its own standards, which is why we need to develop a better test-model that exposes the limits of its. Bohm is explicitly clear that he thinks that:

> the methodology of falsification (like all of the theories) has its *limits of applicability*, and that it is perhaps a special case of a proper method. If this were so, then under suitable conditions, one might even be led to adopt a methodology that in some ways or in certain instances contradict that of falsification (as relativity, containing Newtonian mechanics as a limiting case, leads to conclusions contradicting some aspects of the latter). [706]

Recall that in Feyerabend in ERE (1962) the big reveal was that while falsificationism can take testing commensurable theories into account, it cannot take testing incommensurable theories into account due to meaning variance rendering them deductively disjoint. According to Feyerabend, falsificationism implies that new universal theories could explain the facts potentially explained by earlier universal theories, whereas in fact new theories cannot explain antiquated facts. They must be re-explained as different kinds of facts. Bohm is suggesting that his

[704] Bohm, "On the Relationship between Methodology…", p. 111 [emphasis in the original].

[705] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 249 [emphasis in the original].

[706] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 249 [emphasis in the original]. Compare a little later in the paper: "As important a step forward as the development of the methodology of falsification has been, there is evidently something fundamental that is still lacking in it".

view of the dialectical relation between universal theories (in which the new theory limits the validity of the established theory) also applies to theories of scientific method. Moreover, he is suggesting that falsificationism is dialectically related to a broader methodology that "contradicts" it in some "fundamental" sense. As we gave seen, Popper's methodological views are based on formal relations such as a contradiction between predictions deduced from rival theories. Feyerabend's methodological views are based on incommensurability (which precludes formal contradiction), and Bohm's example of the corroboration of a novel phenomenon by a new theory with an incompatible ontology Bohm's methodological views are based on his dialectics and apply equally to scientific theories and scientific methods. But he seems to be using terms like "deduct" and "fundamentally contradict" informally.

Bohm also suggested, more specifically, that what is missing from the methodology of falsificationism (which he seems to think explains refutation, or the meaning of "false") is a methodology of confirmation that explains the meaning of "truth":

> What is missing is [in] an adequate account of the scientific method [is a] suitable definition of what is the significance of the term "objective truth",

or put another way, methodology:

> must consider the problem of what can be meant by the term "truth" as applied to a law that is not completely universally valid. [707]

Truth must be part of the story, even for falsificationism, according to Bohm, because we need the right kind of reproducible (or "true") observations to falsify theories (in Popper's terms confirmation of the falsifying hypothesis is required for a falsification). From Bohm's perspective, the problem with Popper's methodology is falsificationism's emphasis on false, when truth plays an equally valid dialectically related role in testing theories with empirical evidence. By contrast, Bohm's methodology of the qualitative infinity of nature is supposed to deal with this problem in a "natural way. We begin by supposing that a general law can be true," but "[o]f course, we do not immediately know the definition of its domain of validity". [708] This is supposed to be diametrically opposite to Popper's approach,

[707] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 251.

[708] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 251.

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which seems to suggest that we should not suppose that our theories are true because they can only be proven to be false and never confirmed as true or even more probably proven to be true. On the same page, Bohm suggested:

> In this way, we are led to develop the notion of a series of in principle knowable objective truths, each having objective limits of validity, which are determined by other terms in the same series (and which are therefore also in principle eventually knowable).

This represents Bohm's view of the dialectical relationship between scientific method and the growth of scientific knowledge. Truths are knowable to degrees and relative to domains that inform each other dialectically. For Bohm:

> an idea is true to the extent that it constitutes a reflection having some kind of homeomorphism with the immediate form and with the general law of its object. Thus, an idea which is true in a certain domain or within certain limits is one that reflects with such a homeomorphism only within the domain and limits in question. [709]

True statements correspond to facts only within the limitations of the theories that give them meaning and facts are facts only within limited domains that inform each other. Bohm took truth to be relative to framework, but objective within it (potentially making it seem unfalsifiable, which may be why Feyerabend initially thought that Bohm's view implies that nature forces us to speak the truth), and he praised what he took to be how falsification highlights the dialectical relation between with what Kuhn called "normal science" and Feyerabend's entire-worldview-changing revolutions:

> In this way the notion of *statements having a relative and limited kind of validity could be formalized and given a quite definite meaning*. Having defined the notion of relative and yet objective truth in the manner described above, we can now see immediately why the falsification of "well-confirmed" theories is such an exceptionally good way to be led to correct new hypotheses that are likely to be fruitful. For as we approach the limits of a given theory we will not only discover that it breaks down, but much more important, the precise way in which it breaks down will reflect many features of the new domains that are mainly responsible for this breakdown. (For example, classical mechanics broke down in such a way as to indicate discrete orbits, quantized energy and momentum, and a wave-like as well as the particle-like character for all matter and field systems). [710]

[709] Bohm, "On Certain Epistemological and Cosmological Problems...", p. 251.

Bohm tried to turn this point into a criticism of Popper for putting too much emphasis on falsification and not enough on confirmation:

> But the role of confirmation is much more than that of a mere preparation for future falsification. For after all, it cannot be denied that what scientists are actually seeking in their researches is most fundamentally, not to know in what ways their ideas are false, but rather, to try to arrive closer, to what is, *in some sense*, a set of truths concerning the objects of their investigations.[711]

The problem, of course, is specifying the "in some sense". Basically, Bohm thinks that falsificationism:

> is unbalanced in the sense that the role of falsification is overemphasized, while the at least equally fundamental role of confirmation is almost entirely excluded from the basic features of the theory. [712]

Given Bohm's dialectics, it seems safe to assume that Bohm would have understood Feyerabend's test-model to be dialectically related to Popper's falsificationism, each showing the limit of the other, that is, testing commensurable and incommensurable theories. Bohm even tried to highlight one of the most important conditions of Feyerabend's test-model (the new theory should show the existing theory could not find out it is wrong, without an alternative that then limits its domain), by applying it to methodology and falsificationism itself. Bohm suggested:

> But suppose that the methodology of falsification were to be wrong or inadequate, in some domains, or in certain cases. It is clear that nothing in the method could suggest a procedure by which one would be able to "collect refuting instances" that apply to the principle of falsification itself. For any attempt to give falsification a further meaning will lead us into *the problem of defining the sense in which general statements or laws can be true, a problem that Professor Popper does not regard as capable of being solved*. [713]

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[710] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 252 [emphasis in the original].

[711] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 253 [emphasis in the original]. This passage may have inspired Popper's development of "verisimilitude", which is defined by the relative sizes of their sets of predictions.

[712] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 253 [emphasis in the original].

[713] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 249 [emphasis in the original].

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As we have seen, according to the contextual theory of meaning applied to methodology, results of measurements expressed in one sentence can make two incompatible statements because of meaning variance, like Jastrow's duck/rabbit figure. This Wittgensteinian idea is the very foundation on which the idea of incommensurability is built. Without the requisite training in the use of the theory, the witnesses will not take the same sentence to mean the same statement, so there can be no falsification. If the witness to an experiment:

> had never been trained in the subject under investigation, and had simply jumped into the experiment without any preparation whatsoever. *We would then say that this was not really or truly a falsification*. But *similar difficulties have frequently arisen even in experiments carried out by competent and well trained scientists*. [714]

The difficulties to which Bohm is referring occur when rival incommensurable theories interpret the same experiences into evidence for ontologically incompatible theories. According to Bohm:

> it is necessary to have a theory of scientific research which pays adequate attention to confirmation as well as to falsification, because both are equally significant sides of the process by which scientific knowledge progresses toward greater truth. [715]

Here a major difference between Bohm and Feyerabend becomes clear This time Bohm sounds much more like Popper with his introduction of verisimilitude than like Feyerabend with his Bohmian criticism of Popper. Feyerabend wanted to give up on the idea of a progress toward greater truth altogether and limit his characterization of progress to greater generality.

In this way, Bohm complained that he does not see how falsificationism is falsifiable, and he and suggested that what it needs is a dialectically related complementary methodology that shows the limits of its validity, which explains the role of truth and confirmation and not just falsification. Feyerabend developed a dialectically related complementary counterpart to falsification, but not based on confirmation, but rather, based on *novel* corroboration. Bohm thought falsificationism has only a limited validity and an account of truth and confirmation is what is needed to complement it, showing where it works and does not work,

[714] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 250 [emphasis in the original].

[715] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 250.

thereby making falsificationism itself falsifiable and giving it empirical content, and Bohm implies that falsification has only a limited validity, which by itself, it could never discover. Bohm does not seem to realize that the opposite complementary method of testing to falsification is not an account of confirmation but of novel corroboration. Feyerabend should be read as taking up Bohm's proposal to develop a test-model in contrast to falsificationism that limits its validity. In Bohm's rebuttal of Feyerabend's review, Bohm criticized Popper's falsificationism concluding that it is in general more important to emphasize *the search for a domain of confirmation than it is to emphasize the effort to falsify hypotheses*. Bohm's lengthy response to Feyerabend, comparing his own view with falsificationism, is summarized into *four reasons* that Bohm prefers the former to the latter.

The first one is that it provides a definition of truth as applied to a law that does not have a completely universal domain of validity (based on subjective and objective error) that accords with actual scientific practice. The other three sound very reminiscent of the dispute about a year or two later between Kuhn and Feyerabend, where Kuhn emphasized normal science is needed to provide the backdrop to revolution, while Feyerabend objected to that normal science as a fomr of that impedes revolutionary advance. The second and third reasons, according to Bohm, that his views are preferable to falsificationism are that his view:

> brings out in a clear way the importance of having a body of theory, verified (or confirmed) within some limits, before the question of falsification can in general usefully be raised, [716]

which as a result:

> leads immediately to *an explanation of how and why a study of the precise manner of falsification of a theory previously demonstrated to be valid within certain limits is such good method for obtaining definite clues as to the nature of the next as yet unknown domain*. In this way, it provides a logical justification of the methodology of falsification, *but only for those cases in which this methodology is actually likely to be appropriate*. [717]

This is exactly what the test-model Feyerabend developed in ERE (1962) does. It makes falsificationism testable by showing when it is inappropriate — the lim-

[716] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 255.

[717] Bohm, "On Certain Epistemological and Cosmological Problems…", p. 255 [emphasis in the original].

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its of its validity. The fourth reasons Bohm prefers his view to falsificationism is that:

> It brings out the importance of a certain aspect of the scientific method which is not adequately treated in the methodology of falsification, viz., the restriction of particularization of the domain of validity of a theory, and of the positive search for such a domain. In this way, it leads explicitly to a recognition of the importance of verification (or confirmation), as opposed to a one-sided emphasis on falsification alone. Moreover, it shows that scientific research is not confined only to the search for theories having a greater and greater degree of universality, because the movement toward particularization (the intentional restriction of the domain of laws and the resulting tendency to study only the details of very narrow spheres) also plays, in the long run, a very essential role. [718]

Bohm is arguing, that limiting the validity of a theory or method, is part of how scientific progress is made. Moreover, framework theories are not falsified and replaced. When better theories are better corroborated, existing theories are retained as valid within their newly limited domain. The limits continue to inform the new theory, even if it is and ontologically incompatible with it.

Feyerabend's immediate reaction to Bohm's lengthy 1959 reply to Feyerabend's essay review (published in 1960) of Bohm's 1957 book was that Bohm's criticism of Popper's position was unclear and unsatisfactory. Popper seemed to have liked Feyerabend's review, as can be seen in Feyerabend's reply to Popper's assessment of it in October of 1959:

> Thank you very much for your kind words about my Bohm-review. This was the first longer piece I wrote after a long time and I was so afraid that it would not be good. Bohm wrote 38 pages reply to it, containing a long criticism of your position but I am afraid this reply is very unclear and unsatisfactory. [719]

Bohm, Feyerabend, and Popper all went on to try to address these problems raised by Bohm in his reply to Feyerabend. Bohm tried to criticize Popper's methodology in his first paper published in a philosophy of science journal in 1961, and in 1960, Feyerabend drafted what became ERE (1962), while Popper promised to provide a formal account of "verisimilitude" in Stanford. For these

[718] Bohm, "On Certain Epistemological and Cosmological Problems…", pp. 255–256.

[719] Feyerabend to Popper, 26 October 1959, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", pp. 328–329.

combined reasons, Bohm's 1959 response to Feyerabend's critical review of his book on **Causality and Chance** (1957) should be seen as the *impetus* for Feyerabend's criticism of Popper's falsificationism in ERE (1962) and Popper's turn toward truth with "verisimilitude" in the opposite direction.

Finally in 1961, Bohm's first paper in a philosophy of science journal was published with the title "On the Relationship between Methodology in Scientific Research and the Content of Scientific Knowledge". In this paper, drawing from his unpublished criticism of Popper's methodology of falsification in his 1959 rejoinder to Feyerabend's review of Bohm's **Causality and Chance** (1957), Bohm set out to try to criticize falsificationism, and to develop his own methodological views in contrast to falsificationism. In a clear anticipation of the criticism Feyerabend develops in ERE (1962), Bohm's attempted to criticize falsificationism based on the idea that refutations take place within frameworks that are not themselves directly refutable. His emphasis was on testing within frameworks in contrast to testing frameworks themselves. The paper begins by discussing how questions frame possible answers:

> It is clear then that underlying the problem of falsification and confirmation, there is something very much more fundamental; namely, that of *framing questions* that have a definite "yes" or "no" answer in a given situation. What must be emphasized here is that the form in which a question is put constitutes an implicit hypothesis about the object of the question; namely, that the object is such that this question has a clear "yes" or "no" answer. [720]

Bohm was trying to explain how questions arise within frameworks that delimit their possible answers. This is part of how established theories can condition experience of nature by delimiting the scope of sensible questions that can be asked about it. Note with this formulation, Bohm appears to take falsification as synonymous with direct refutation (of a theory by a prediction deduced from it) and as the opposite of confirmation (or what he sometimes calls "verification"). For Feyerabend, as we have seen, following how he presented Popper, a falsification is an indirect refutation (because in a falsification the evidence corroborating an alternative does the refuting), not a direct refutation, while the opposite of a refutation is a corroboration, not a confirmation (predictions are confirmed, theories are corroborated by confirmed predictions). In this way, with ERE (1962),

[720] Bohm, "On the Relationship between Methodology…", p. 105 [emphasis added].

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Feyerabend should be seen as developing Bohm's attempt to criticize falsificationism, but more precisely than Bohm. Feyerabend tried to develop Bohm's ideas and argument more squarely within Popper's framework so that it could hit its target (falsificationism).

Bohm emphasized both that progress involves learning to ask the right questions, and learning to ask them right:

> since we recognize that a science may pass through phases (especially while it is developing rapidly) in which the proper questions having this character are not even known, we are led into a whole new series of problems relating to the role of concepts, theories, and hypotheses in scientific research. [721]

On Bohm's view, we have to learn how to ask the right questions, and to do that we have to develop the right concepts. Refutation and confirmation take place within frameworks, which delimit the possibility of how questions can be asked. Different frameworks lead to different kinds of questions and different kinds of facts. For Bohm, there are classical facts, relativistic facts, and quantum facts, because facts are always only facts according to some theory, and framework theories and the worldviews they support determine what facts are experienced. Framework theories are not refuted. New theories based on "opposite" principles establish the limits of their validity.

Bohm emphasized that a lot has to happen before testing a framework theory can even begin. As we have seen, from Bohm's perspective that is what he had been working towards since 1952. To try to reach the testing phase of quantum theory stage, first he delivered a proof of concept (1952), then he laid out the strategy and the new conceptual framework (1954), then he tried to show that he could explain the same successful predictions as the established theory with his proposal (1957). His next step was to try probe the established theory to try to find the limits of its validity (1959). Only after all of that, would he be in position to try to test his theory by explaining a novel phenomenon that shows that the existing theory is wrong because it *cannot* explain that novel phenomenon, not by refuting the established view, but rather, by showing it to have a limited validity due to the fact that it is based on the wrong ontology. The novel phenomenon that Bohm's proposal aims to explain, which the established theory by itself cannot,

[721] Bohm, "On the Relationship between Methodology...", p. 105.

was quantum indeterminism. But the establishment was not looking for an explanation of quantum indeterminism. They wanted to use it to explain the peculiar results of their experiments.

As we have seen, in ERE (1962), Feyerabend explicitly set out his theoretical pluralism by developing the idea that universal theories (in contrast to laws and lower-level theories) act like Kantian frameworks that condition all possible experience according to them, but which are replaced in scientific revolutions by new universal theories and the new worldviews they support. In revolutions, new theories re-explain what were formerly taken to be facts as new kinds of facts. For both Bohm and Feyerabend, facts do not exist independently of the theories used to interpret them. In his 1961 paper, Bohm emphasized that:

> facts are not like things that exist in nature independently of man, so that they can, so to speak, be gathered as if they were stones or flowers. Rather, as the derivation of the word indicates, they are made (or manufactured). In this regard, nature may be compared to the raw material, while the fact is a finished product, having a form determined in part by man. [722]

Facts have a subjective component. (They are co-constituted by subjective and objective sided moments in Kuhnian terminology. [723]) In this respect, Bohm and Feyerabend clearly both subscribe to some form of Kant on wheels realism, but Bohm posited levels dialectically related, where Feyerabend saw an ocean of alternatives, of which only our current best universal theories should determine our worldview by being interpreted realistically across all levels and in all domains and used to reform everyday language. From Bohm's view, progress also looks like some kind of swelling ocean of alternatives, but Bohm's realism seems more like some sort of Kant on wheels *and levels within limited domains* realism. According to Bohm:

> the content of each field of scientific research calls for facts having appropriate forms. As we enter new fields, we must generally give new forms to the facts, forms that will be appropriate to the character of nature in the new domain [...] It is in the form of the facts that the whole body of the existing framework of concepts and laws plays a crucial role. [724]

[722] Bohm, "On the Relationship between Methodology...", p. 106.

[723] See Hoyningen-Huene, **Reconstructing Scientific Revolutions...**

[724] Bohm, "On the Relationship between Methodology...", p. 106.

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The existing framework plays a crucial role in establishing the form of the facts, while different levels and different domains determine different forms of facts on those levels and in those domains. Bohm, like Feyerabend, explicitly stressed that the claim that framework theories determine the facts is supposed to hold for everyday facts and everyday language, Bohm illustrated, with everyday examples such as the "sun shines", the "bell rings", the "dust here". Of course, in contrast to Feyerabend, Bohm suggested that universal frameworks are *dialectically related:*

> Even if the law should be refuted in some other domain, or in experiments carried out to a higher accuracy, the fact of its validity in the original domain and degree of approximation is not thereby altered (e.g. as the physicist accepts as a fact the behaviour of large-scale apparatus approximately according to the laws of classical mechanics, although he uses this apparatus in studying the laws of quantum mechanics, with the aid of which he will refute the notion that the classical theory is exactly and universally valid). [725]

For Bohm, the facts of classical mechanics are still facts within a limited domain, even after classical mechanics was "refuted" by its own predictions outside the validity of its domain. In this respect, Bohm seems to be closer to the target of Popper's (unspecific) criticism in his **Myth of the Framework** (1994) than Feyerabend. According to Bohm:

> Since the scientific method implies that we should proceed as if the hypothesis were true unless and until it is actually refuted, it follows that even a hypothesis can help organise existing facts into a coherent whole and determine new forms in which relevant facts can further be developed. For, if the hypothesis is correct, it *leads us* to ask appropriate questions of Nature [and] suggest[s] problems that stimulate fruitful research and eventually lead to better theories. On the other hand, there is, of course, the possibility also illustrated by many examples in the history of science, that wrong hypotheses may impede research, by drawing attention away from questions that are relevant. Thus, the organization of facts and determination of their forms with the aid of hypotheses has both great advantages and great dangers. [726]

Feyerabend recommended that universal theories should determine our entire worldview, while emphasizing the great danger of their all-pervasive charac-

[725] Bohm, "On the Relationship between Methodology…", p. 107. "Both Bohm and Bohr used this example to support the maxim that every fundamental truth is paired with an opposite truth.

[726] Bohm, "On the Relationship between Methodology…", p. 108.

ter. For Bohm, successful universal theories that once supported our worldview are never shown to be refuted, they are shown to have a limited validity when they are superseded by a better corroborated (more general) rival that establishes its limits that is dialectically related to it.

Bohm also briefly rejected reductionism for being overly restrictive:

> The notion that everything is, in principle, reducible to physics [...] [is] an unproved assumption, which is capable of limiting our thinking in such a way that we are blinded to the possibility of whole new classes of fact and law. [727]

For this reason, Bohm rejected the view that physics is a fundamental science to which biology and chemistry can be reduced. For Bohm, different domains on the same (classical) level require different forms of facts. Life itself may be irreducible to physics, and in some respects for Bohm the growth of science is analogous to life itself. Living systems:

> have a tremendous range of potentialities and [...] [are] continually moving toward various actualisations of these potentialities, without thereby falling into a state in which they have no further potentialities. [728]

For Bohm the same holds for science. Moreover, if laws governing the development of life only emerged with life, then:

> there could be facts that do not fit into the physicist's frame of concepts. They might well be expressible *fully* only with the aid of new concepts which are, in effect, a definition of some of the essential characteristics of living systems. [729]

The similarity between Bohm and Feyerabend's theoretical pluralisms can be seen very clearly. For Bohm:

> At any given moment then, we are faced, not with one coherent system of facts, but rather with a large number of partial systems, each having some intermediate degree of coherence. (There are different fields of study, different domains and levels in each field, etc). Within each partial system, certain kinds of facts are possible. [730]

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[727] Bohm, "On the Relationship between Methodology...", p. 113.

[728] Bohm, "On the Relationship between Methodology...", pp. 115–116.

[729] Bohm, "On the Relationship between Methodology...", p. 116.

[730] Bohm, "On the Relationship between Methodology...", p. 107.

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Recall that for Feyerabend:

> The methodological unit to which we must refer when discussing questions of test and empirical content is constituted by a whole set of partly overlapping, factually adequate, but mutually [incompatible] theories. [731]

Feyerabend's views are not the only ones that Bohm's clearly anticipate in significant respects. For example, according to Bohm:

> It is generally realised that while specialisation has important advantages, it requires us to pay a price to get them. There is, however, a certain aspect of this price, that does not seem to have been considered adequately. This is that specialisation narrows our conceptual framework sufficiently so that we cannot even express facts of a certain kind; namely, those whose framework essentially and intrinsically cuts across the boundaries between various fields [...] If such facts exist and are important, then it may well be that the price of specialisation will eventually involve the erection of a barrier to the very knowledge that we expect to get through it. [732]

Bohm concluded that "there is a distinct possibility that the current tendency toward specialisation may defeat its own purpose". [733] In his later work, Kuhn too erecting such barriers to be more than just as distinct possibility. He suggested they are part of the fundamental mechanism of progress through stages of normal scientific progress that lead to crisis resulting in revolutions. For Bohm:

> Method is determined in part by the effort to ask relevant questions in our researches; and it is essential to understand that the relevance of a question depends on the character of the material under investigation. Such questions help determine the forms of the facts that can be elicited in further researches. [734]

And just as Feyerabend emphasized in ERE (1962), according to Bohm:

> These questions are, in general, limited firstly by our concepts, laws, and hypotheses, and secondly, in a less obvious but equally important way, by our general habits of

[731] Feyerabend, "How to Be a Good Empiricist...", p. 92. The term "incompatible" has replaced "inconsistent" to avoid the conflation of formally inconsistent inconsistent rules of usage that make the theories incompatible. As we have seen, Feyerabend was sometimes ambiguous or even seemingly inconsistent in his use of "inconsistency".

[732] Bohm, "On the Relationship between Methodology...", p. 112.

[733] Bohm, "On the Relationship between Methodology...", p. 116.

[734] Bohm, "On the Relationship between Methodology...", p. 116.

> thought. Such habits can easily blind us to *the need for altering our ways of thinking in accordance with the nature of the material under investigation as we penetrate into new domains*. [735]

The success of the established theory can blind us to the need for introducing the right kind of new framework. This is precisely what Bohm thought was happening with respect to quantum theory. The establishment was ignoring the anomalous character of quantum indeterminism. Bohm stressed that we cannot see how some known observation could potentially refute an established theory, without first learning to ask the right questions by introducing the right concepts. Anomalies that clearly refute the theory from the new perspective were not always seen as such prior to the crisis that led to a revolution. They have been ignored or explained away with *ad hoc* hypotheses. Bohm used retrograde motion to illustrate the point that the crucial problem can be hiding in plain sight.

The point of this section was show that, ERE (1962) should be read as Feyerabend building on Bohm's [1959] and 1961 attempts to criticize Popper's falsificationism so that the criticism hits its intended target by limiting the validity of falsificationism to comparing commensurable theories, with Bohm's example of Einstein's prediction of Brownian motion, in support of Bohm plea for more pluralism and tolerance, in support of Bohm's proposal in the foundation of quantum theory, and with respect to new proposals more generally. They both agreed that a new proposal like Bohm's should not be dismissed because it is *ad hoc*. It should be given the chance to be developed so that it can try to prove its merits by leading to the right questions, eventually becoming independently testable by novel corroboration, thereby undermining the established view neither by direct refutation, nor by falsification. Feyerabend and Bohm argued that Popper's methodology fails to be valid for "universal" or "framework" theories. The validity of falsificationism is limited to testing within frameworks in contrast to testing frameworks themselves. In Bohm's 1959 reply to Feyerabend's review of Bohm's 1957 book and in his first published paper in a philosophy of science journal, published just one year prior to the ERE in 1962, there are clearly substantial similarities in Feyerabend and Bohm's arguments for theoretical pluralism and with respect to the views they set out on the non-autonomy of facts and dynamic Kantian frameworks, according to which facts are always facts according to some framework

[735] Bohm, "On the Relationship between Methodology…", p. 116 [emphasis added].

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theory. As we shall soon see, there is also substantial overlap in the ideas set out by Feyerabend in ERE (1962), and Bohm's second paper in a philosophy of science journal published that same year.

3.8 Bohm on Meaning Change and Methodology (1962)

In his second paper in a philosophy of science journal, again published in the *British Journal for Philosophy of Science*, Bohm replied to Heisenberg's **Physics and Philosophy**, about the relationship between "Classical and Non-Classical Concepts in the Quantum Theory". Bohm took issue with Heisenberg's contention that:

> The concepts of classical physics are just a refinement of the concepts of daily life and are an essential part of the language which forms the basis of *all natural science*. Our actual situation is that we *do* use the classical concepts for the description of experiments. [...] There is no use discussing what could be done if we were other beings than we are. [736]

Bohm also argued against Bohr's contention that:

> the quantum theory introduces *no new concepts into physics*. What is new in this connection is, however, that the *old concepts, such as position and momentum, can be defined only within maximum limits of precision set by the uncertainty principle*. [737]

Bohm is proposing that there can be conceptual change, even at the level of our most basic statements. He argues that topological concepts are more fundamental than classical Cartesian co-ordinates, and more importantly, that topological concepts could and should serve as the basis for a new conceptual order (that could be used to explain the facts of experience according to both relativity theory and quantum mechanics). As Bohm put it:

> We may ask, for example, whether in everyday experience we would describe the location of a pencil by giving its precise latitude and longitude (which is what is implied

[736] Heisenberg as cited in: Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 273. See also Heisenberg, **Physics and Philosophy…**

[737] Bohm cites a private communication with Bohr, see Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 272, fn. 1.

> by the above described "classical" concept of space). It is evident that we do not do this, but that instead, we describe the pencil as being on a certain desk, which is in a certain room, which is in a certain house, on a certain street, etc. […] It is clear then that behind the concept of space and time as described in terms of co-ordinates, there is a more fundamental topological concept, which lies at its basis, but which is usually just taken for granted without further discussion. [738]

This is the beginning of Bohm's quest to find a new conceptual order for *all* of physics grounded in topology. While much could be said about this development in Bohm's views (especially in the context of his theory of levels), for our present purposes, it is important to take note that Bohm is illustrating one of the main points that Feyerabend made in ERE (1962). Even the meanings of our most basic concepts can change when universal theories change. According to Bohm:

> It must be admitted that ideas of the type described above are commonly held by physicists. It takes only a little reflection, however, to see that there are strong reasons suggesting that such ideas are false. To see why this is so, let us concentrate on an essential feature of the concepts of space and time as they are used in classical physics, namely, the description of the location of an object or an event in terms of a set of continuous Cartesian co-ordinates. Is it really true that we have no other way to think of space and time except in terms of such a concept? [739]

Bohm is suggesting that new concepts for thinking about space and time could be used to describe location. This clearly harkens back to the discussions at the 1957 Colston conference and Bohm's views on the meaning of "position" responding to and bolstering Feyerabend's point about changes to the meaning of "length". Bohm is challenging Heisenberg and Bohr's views because they imply that classical concepts are fixed and will always remain necessary. As we have seen, Feyerabend challenged this assumption throughout his early philosophy, arguing that there is meaning variance, even in everyday language, which should be corrected to reflect our current best theories and the worldviews they are taken to imply. For example, as we have seen, Feyerabend criticized Rosenberg at the 1957 Colston conference exactly for this reason, just as he criticized Bohr's complementarity in ERE (1962) for this reason. [740] Feyerabend and Bohm are even us-

[738] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 273.

[739] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 273.

[740] See especially Feyerabend, "Complementarity…" and Feyerabend, "Problems of Microphysics…" on which, "Explanation, Reduction and Empiricism…" (1962) draws. In ERE (1962), Feyerabend

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ing the same example: Changes to the meaning of the terms "space" and "time". This illustrates how Bohm (1962) and ERE (1962) both argue that basic concepts of universal theories change the meanings of everyday concepts when the universal theories used to interpret experience realistically are replaced. Bohm argued:

> We see then that Heisenberg's contention that classical concepts are the only possible refinements of everyday concepts must be false. For as shown above, one can at the very least obtain refinements leading to topological concepts, which are at once closer to everyday experience and more fundamental in a logical mathematical sense. [741]

Bohm even noted, as Feyerabend often insisted: "Of course, refinements of ev-

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draws on all four areas of his prior publications in philosophy: formal logic, analytic philosophy, methodology, and the foundation of quantum theory. On formal logic, see Feyerabend's slew of early reviews, for example, Paul K. Feyerabend, "Review of Heskell B. Curry, **Outline of a Formalist Philosophy of Mathematics, Studies in Logic and the Foundations of Mathematics, Vol. 3**, North-Holland: Amsterdam 1951", *Nachrichten der österreichischen mathematischen Gesellschaft* 1952, Vol.17/18, p. 68; Paul K. Feyerabend, "Review of Georg H. von Wright, **An Essay in Modal Logic, Studies in Logic and the Foundations of Mathematics, Vol. 5**, North-Holland: Amsterdam 1951", *Nachrichten der österreichischen mathematischen Gesellschaft* 1952, Vol.17/18, pp. 68–69; Paul K. Feyerabend, "Review of Andrzej Mostowski, **Sentences Undecidable in Formalized Arithmetic: An Exposition of the Theory of Kurt Gödel, Studies in Logic and the Foundations of Mathematics, Vol. 10**, North-Holland: Amsterdam 1951", *Nachrichten der österreichischen mathematischen Gesellschaft* 1952, Vol. 9/20,pp. 62–63; Paul K. Feyerabend, "Review of Paul C. Rosenbloom, **The Elements of Mathematical Logic**, Dover: New York 1950", *Nachrichten der österreichischen mathematischen Gesellschaft* 1952, Vol. 19/20, pp. 67–68; Paul K. Feyerabend, "Neuere Probleme der philosophischen Logik", *Wissenschaft und Weltbild: Monatschrift für alle Gebiete der Forschung* 1952, H. 9, pp. 315–319;Paul K. Feyerabend, "Review of Reuben Louis Goodstein, **Constructive Formalism: Essays on the Foundations of Mathematics**, University College Leicester, Leicester 1951", *Internationale mathematische Nachrichten* 1954, Vol. 33/34, p. 59; Paul K. Feyerabend, "Review of Rudolf Carnap, **Einführung in die symbolische Logik mit besonderer Berücksichtigung ihrer Anwendungen**, Springer: Vienna 1954", *Internationale Mathematische Nachrichten* 1954, Vol. 35/36, p. 67; Paul K. Feyerabend, "Review of John Barkley Rosser, **Logic for Mathematicians**, McGraw-Hill: New York 1953", *Internationale Mathematische Nachrichten* 1954, Vol. 35/36, p. 78. On analytic philosophy, see Feyerabend, "Wittgenstein und die Philosophie, I"; eyerabend, "Wittgenstein und die Philosophie, II" ; Feyerabend, "Wittgenstein's **Philosophical Investigations..."**; Feyerabend, "A Note on the Paradox of Analysis...". On general methodology, see: Feyerabend, " Zur Theorie der Basissätze..."; Feyerabend, "Physik und Ontologie..."; Feyerabend, "An Attempt at a Realistic Interpretation..."; Feyerabend, "Das Problem der Existenz theoretischer Entitäten...". On the foundation of quantum theory, see Feyerabend, "Bemerkungen zu Interpretation and Preciseness..."; Feyerabend, "Determinismus und Quantenmechanik...", pp. 89–111; Feyerabend, "Eine Bemerkung...", pp. 421–423; Feyerabend, "Zur Quantentheorie der Messung..."; Feyerabend, "Complementarity..."; Feyerabend, "Mathematical Foundations of Quantum Mechanics. By John von Neumann..."; Feyerabend, "Niels Bohr's Interpretation of the Quantum Theory...", Feyerabend, "Problems of Microphysics...".

[741] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory...", p. 274.

eryday concepts in still other directions should be possible too", but for Bohm in "physics, topology is particularly important, since it deals with the description of space and time, which play a central role in all physical theory". [742] Bohm's conclusion depicts precisely the deadlock Feyerabend's test-model for comparing universal theories tries to show how to break:

> We conclude then that, contrary to what is maintained in the Copenhagen interpretation, the quantum theory can be developed further to introduce the notion of a "physical actuality", with a new experimental and mathematical content *not expressible in the usual formulation*, and nevertheless approaching the current theory in those domains in which the latter has really been well confirmed. [743]

In other words, Bohm's ontologically incompatible proposal introduces a new concept of "physical actuality" that gives new content *that cannot be expressed in terms of the established theory*, while nevertheless recovering its successful predictions. How has this result been achieved?

> In order to achieve this result, however, we have been led to introduce topological concepts that are quite different from those appearing in classical physics and in the Copenhagen interpretation of the quantum theory. [744]

By introducing new concepts with new meanings to redescribe the results predicted by both theories:

> The applicability of such topological concepts is not restricted to the micro-domain, but rather, extends through every level, including the large scale level of common experience. [745]

Bohm who accepts Feyerabend's point that universal theories should affect even the most common idiom is now trying to find the empirical basis of a new conceptual order for all of physics with topological concepts, which brings Bohm to his main conclusion in the penultimate sentence:

> It follows then that the use of topological concepts implies the need for a re-evalua-

[742] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 274.

[743] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 273 [emphasis in the original].

[744] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 280.

[745] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 280.

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> tion of the significance of every aspect of physics including not only the current quantum theory but also classical mechanics. [746]

Bohm's final sentence can seem a bit unclear, as if he were not suggesting changing the meaning of everyday language:

> When this re-evaluation is accomplished, however, it is discovered that we are simply giving up the excessively abstract classical notion, and returning to a topological notion of space and time that *is very close* to one that we have always been using in common experience. [747]

However, Bohm is implying, as he has been suggesting all along in the paper, that the basic concepts of everyday language can undergo meaning change due to new universal theories. The new concept of space is neither the classical concept, nor the concept used in everyday language. It is only *very close* to the notion we have always been using because it has a different meaning. However, in everyday language, while we continue to use the same terms in almost exactly all the same ways, we are saying something different with them.

By 1962, Bohm and Feyerabend were definitely in sync. As we have seen, many of the arguments and ideas that they had developed in this period clearly complement each other. With respect to Feyerabend's criticism of Popper, retrospectively, in 1978, Feyerabend says explicitly that he concluded that Popper was wrong about falsificationism in discussions with Bohm. According to Feyerabend:

> *While in Bristol* I continued my studies of quantum theory. I found that important physical principles rested on methodological assumptions that are violated whenever physics advances: physics gets authority from ideas it propagates but never obeys in actual research [...] *That falsification is not a solution became very clear in discussion with David Bohm who gave a Hegelian account of the relation between theories, their evidence, and their successors.* [748]

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[746] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 280.

[747] Bohm, "Classical and Non-Classical Concepts in the Quantum Theory…", p. 280 [emphasis in the original].

[748] Feyerabend, **Science in a Free Society…**, p. 116 and Feyerabend, **Against Method…** (1993), pp. 261–262 [emphasis inserted]. Compare: "Doubts about Popper had arisen in my debates with Bohm and then, again, with Kuhn" (Feyerabend to Bechler, 1 January 1976, in: Feyerabend, "Autobiographfical Remarks 1976…", p. 32). Feyerabend in "Dialectical Materialism…", recognizing Bohm's emphasis on constraints the present puts on future possibilities, eventually clearly retracted some limited aspects of his criticism of Bohm's views on necessity.

Of course, Feyerabend continued to reject Bohm's dialectical materialist philosophy at the time and afterwards. However, while Feyerabend clearly did not go Bohmian in methodology, it should be just as clear that Feyerabend's ERE (1962) is supposed to be a criticism of falsificationism that was unquestionably inspired by Bohm and his views.

Part of the confusion surrounding Feyerabend's debt to Bohm is due to the revised version of ERE published in Feyerabend's 1981 collected papers, in which Feyerabend's explicit debt to Bohm and Vigier with respect to the Brownian motion example gets lost, when entire consecutive paragraphs were deleted. However, in the original ERE (1962), Feyerabend, citing Bohm's *Causality and Chance* (1957), makes the close connection to Bohm's situation in the foundations of quantum mechanics and his argument very clear:

> it may well be, as has been pointed out by Bohm and Vigier, that the refutation of the quantum-mechanical uncertainties presupposes just an incorporation of the present theory into a wider context, which is not any more in accordance with the idea of complementarity and which therefore suggests new and decisive experiments. And it may also be that the insistence on part of the majority of contemporary physicists upon (5) ["only such theories are admissible (for explanation and prediction in a given domain which either contain the theories already used in this domain, or are at least *consistent* with them", p. 44] will, if successful, forever protect these uncertainties from refutation. This is how modern empiricism may finally lead to a situation where a certain point of view petrifies into dogma by being, in the name of experience, completely removed from any conceivable criticism. [749]

In ERE (1962), Feyerabend does not acknowledge Bohm and Vigier specifically as the source of the example of Brownian motion. But he does acknowledge them directly for the point that he is trying to make with the example: the idea that direct refutation of a theory may, in some cases, be impossible for empirical reasons:

> What I regard as a most important feature of the situation-a feature, by the way, that has been emphasized by Bohm and Vigieris that direct refutation of a fairly complicated theory may be impossible for empirical reasons. That this is so will be shown with the help of an [*their*] example. [750]

[749] Feyerabend, ERE (1962), p. 66.

[750] See Feyerabend, ERE (1962), p. 32.

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This acknowledgment simply disappeared in the 1981 version. [751]

In sum, Feyerabend's general methodology, as set out in ERE (1962) is modelled on Bohm's Brownian motion explanation of quantum indeterminacy in the dispute in the foundations of quantum theory about the methodological legitimacy of Bohm's "hidden variables" proposal strategy. In this way, Feyerabend's early philosophy of science was connected to its subject and so not a little-known form of insanity. It was a plea for tolerance with respect to Bohm's Brownian motion proposal strategy in the foundations of quantum theory, which was justified based on Bohm's Brownian motion example in methodology, which Bohm had used to illustrate his own argument for more tolerance with respect to his own proposal. Bohm's *ad hoc* introduction and development of that theory should not be dismissed, because one day some Einstein could come along and show how to make Bohm's "hidden variables" independently testable, as Einstein himself had showed how Brownian motion could be used to test kinetic theory, providing the justification for atomism, revolutionizing our worldview. Bohm motivated and shaped Feyerabend's criticism of falsificationism in ERE (1962) by suggesting to Feyerabend that falsification cannot take the refutation of classical mechanics into account, because classical mechanics was not given up because it was falsified, but because another theory was more successful. Feyerabend's test-model, in contrast to Popper's falsificationism, tried to develop the idea of crucial experiments between incommensurable theories based on novel corroborations, in an attempt to capture exactly this point.

Postscript: From Ehrenhaft's Effects to Bohm's Hidden Variables on The Road to "Anything Goes!"

Now for the backstory. In the late 1940s, when Feyerabend was still pursuing his ambition to become a scientist before he settled for philosophy of science, he worked as an assistant to Felix Ehrenhaft, a controversial experimental physicist, who had fought and lost to Millikan with respect to the electron and his oil-drop

[751] Compare the version of ERE (1962) published in: Feyerabend, **Realism, Rationalism and Scientific Method...**

experiments. [752] Ehrenhaft was dismissed as a charlatan, just as his experimental effects, which Feyerabend had painstakingly helped to set up, were dogmatically dismissed as *Dreckeffekte*. [753] Feyerabend wondered: How could Ehrenhaft be a charlatan and a scientist? On the one hand, Feyerabend still believed the theories, despite the effects that they could not seem to explain with them, on the other hand, Feyerabend thought Ehrenhaft was being unfairly ostracized by the scientific community, and his effects were being dogmatically dismissed. Part of what drew Feyerabend to Popper in the first place was Popper's instance that *ad hoc* hypotheses should not be used to protect theories from refutations. This clearly set the stage for Feyerabend's experience with Bohm, whose theory was dogmatically dismissed for being *ad hoc*. Feyerabend clearly appreciated the irony that he was now defending the use of ad hoc hypotheses to protect theories from refutation (so that they have a chance to prove their merits); and Rosenfeld's hypocrisy (first dogmatically protecting theories from refutation, then criticizing Bohm's proposal for being *ad hoc*). At the end of the letter to Watkins sent on the 17th of December announcing his break with Popper's school, Feyerabend wrote:

> Did I send you my translation of Ehrenhaft's lectures? Read the *second appendix* (the last seven pages) which is an eyewitness account of Ehrenhaft in Vienna and of a certain dramatic discussion between Ehrenhaft, Rosenfeld, Popper and others. I think you will be amused. You can show your gratitude by sending me an amusing letter in turn.

Watkins should find it amusing because now Feyerabend is no longer on Popper's side criticizing Rosenfeld's dogmatic dismissal of Ehrenhaft's effects, but on Bohm's side defending Bohm's a*d hoc* proposal yet again from Rosenfeld's dogmatic dismissals. Recall, Feyerabend awoke from his Popperian slumber into his living nightmare while watching Lakatos lecture Popper's falsificationism, presumably including Popper's admonition against using *ad hoc* hypotheses to protect theories, when Feyerabend concluded that all methodological rules only have a limited validity, not just falsificationism. He was now defending the *ad hoc* introduction and development of a theory from dogmatic dismissal of it. As we are about to see, the close connection between the two cases involving Rosenfeld becomes very clear in Feyerabend's test-model of conjectures and novel corrobora-

[752] See Feyerabend, "Autobiographfical Remarks 1976…", pp. 29–30.

[753] See Collodel, "Ehrenhaft's Experiments on Magnetic Monopoles…".

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tions in contrast to falsificationism. First, it is important to stress that Feyerabend's emphasis that even *ad hoc* hypotheses should be accepted in some contexts contributed to his conclusion that not just falsificationism has only a limited validity but more generally all methodological rules can have only a limited validity. Moreover, because they only have a limited validity, without the specific context, they are empty because only the context can determine if they are appropriate in any particular case. This is why philosophy of science disconnected from its subject became, for Feyerabend, a form of lunacy. [754] When Feyerabend disconnected from his direct engagement with Bohm, he felt increasingly disconnected from the Popperian school, just as Bohm increasingly emphasized interconnectedness with his search for a new topological conceptual order for *all* physics in the early 1960s and beyond. [755] Ultimately, Feyerabend recognized that connectedness with other people is love and that is what should guide our decisions about what to believe, while for Bohm connectedness always interconnected all his various ideas and relations, from his cosmology to his theory of plasma and the 14th Dalai Lama.

As we have seen, in ERE (1962), Feyerabend, following Bohm, is trying to argue that without the development of incommensurable alternatives (such as Bohm and Vigier's proposal), astounding effects like Brownian motion prior to Einstein (r quantum indeterminacy without something like Bohm and Vigier's sub-quantum Brownian motion proposal that attempts to explain it, or Ehrenhaft's effects) go unexplained and are impotent. They cannot refute the established view. That is how new incommensurable theories can increase the empirical content of their rivals, and why incommensurable proposals should always be welcome. In his simplified summary of ERE (1962), published in 1963, made the connection between his test-model and his Ehrenhaft experience very clear:

> the Brownian particle would have been regarded as an oddity much in the same way

[754] See the title of See Feyerabend's, "Die Wissenschaftstheorie…", pp. 88–124 ("Philosophy of Science: Until Recently an Obscure form of Lunacy? (my translation)").

[755] Cartesian coordinates locate an object in space in relation to a framework. Bohm seems to be suggesting that an object's location should be understood in relation to other objects in a framework, not in relation to a framework (which is more abstract). Bohm seems to have been drawn to topological concepts because he thought that they could be used to redescribe location and motion in a way that is consistent with the concepts of both quantum theory and relativity theory.

> in which some of the late Professor Ehrenhaft's astounding effects [were]. [756]

As Feyerabend saw it, from a methodological perspective, both Ehrenhaft and Bohm were ostracized unjustly and ERE (1962) tried to develop and recommend a test-model that is much more tolerant of both of their strategies. Feyerabend based his test-model for a crucial experiment between incommensurable rivals exactly on the kind of unexplained effects he had helped Ehrenhaft to try to demonstrate (but which were dismissed was *Dreckeffekt*, and follwoing Bohm and Vigier's lead, he argued for more tolerance towards their *ad hoc* proposal based on that test-model. This is how Feyerabend's Ehrenhaft experience in 1949 set the stage for his Bohm experience in the mid-to late 1950s that led to Feyerabend's criticism of Popper's methodology in ERE (1962), which lead to his conclusion that "Anything goes!" in 1967, as first published in 1970.

As Collodel points out:

> Oberheim argues that the hotly debated experiments performed by Ehrenhaft at the 1949 European Forum Alpbach were "the decisive episode that shaped Feyerabend's philosophical development" and "taught Feyerabend a lesson about experience and its relation to theory that grounded his entire philosophical outlook" (Oberheim, [...] [**Feyerabend's Philosophy**], p. 116; italics inserted) so that "[t]he effects that this episode had on Feyerabend's conception of the relationship between theory and experiment should not be underestimated" (p. 120). [757]

Collodel suggests that it is more likely that only after 1967, after Feyerabend adopted his epistemological anarchism, when Feyerabend concluded that charlatanism should be part of science with "anything goes", did Feyerabend belatedly appreciate the lessons he learned from his Ehrenhaft experience. (To borrow a response from Watkins: "Balls!".) As we have seen, conflating critical rationalism with falsificationism leads Collodel to underestimate Bohm's deep influence on Feyerabend's methodological views, which Collodel takes to be unreconcilable with Feyerabend's support for Popper's critical rationalism — as if that philosophical tradition belonged solely to Popper — over Bohm's dialectical materialism. In a talk entitled "A Sorcerer's Apprentice or How Feyerabend Transmuted Popper's Critical Rationalism into Theoretical Pluralism and Got Cursed with Incommensurability" (2015), Collodel suggested that Feyerabend got cursed with

[756] Feyerabend, "How to Be a Good Empiricist...", p. 23.

[757] Collodel, "Ehrenhaft's Experiments on Magnetic Monopoles...", p. 10.

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incommensurability because the idea does not make sense. Instead of recognizing Feyerabend's criticism of Popper's methodology (that it cannot take testing incommensurable universal theories into account due to meaning variance), Collodel sees Popperstein's baby resulting from black magic sorcery. He neither highlights what Watkins recognized in 1960, that is, that ERE was *intended* as a criticism of falsificationism, nor how the criticism is supposed to work; nor that Feyerabend's pluralism is based on a test-model that puts the corroboration of novel effects *such as Ehrenhaft's* front and center in crucial experiments between universal incommensurable theories in contrast to Popper's account of refutation by falsification based on formal relations of consistency and contradiction between corresponding predictions deduced from both theories.

Collodel's proposal, similar to his view of Feyerabend's relation to Popper's methodology, is that Feyerabend only retrospectively reinterpreted *ERE* in this way, that is both with respect to it as a criticism of Popper, and as based on lessons he learned from Ehrenhaft:

> Feyerabend's methodological proposal of a theoretical pluralism at the start of the 1960s, rather than being brought about by Ehrenhaft's lesson of a decade earlier, was advanced fairly independently of Ehrenhaft's influence and yet facilitated *Feyerabend's rediscovery and reevaluation of Ehrenhaft's effects in light of that proposal*. More precisely, only from the early 1960s, in the process of giving shape to his theoretical pluralism and, *more likely, as he was improving it in response to objections* [that is after *ERE* (1962)], Feyerabend *resumed his Ehrenhaft experience* and made sense of it with the help of the conceptual tools he was developing at the time. [758]

Just as it only looks like a retrospective projection of a criticism of Popper's methodology if you miss the big reveal, it only looks like a "rediscovery and reevaluation" of Ehrenhaft's effects that "resumed" after ERE (1962), if you do not see how Feyerabend's argument for theoretical pluralism based on Feyerabend's test-model based on Brownian motion in ERE (1962), which was written between early 1960 and early 1961, relates directly to the lessons he had learned from his Ehrenhaft experience.[759] It was not a retrospective reinterpretation of the original paper after he broke in late 1967 with Popper's school. It is what eventually led to

[758] Collodel, "Ehrenhaft's Experiments on Magnetic Monopoles…", p. 16 [emphasis added].

[759] Given Ehrenhaft's horrible reputation and Feyerabend's lack of tenure at the time, there is no need for an a further explanation of why Feyerabend did not invoke Ehrenhaft's effects in ERE (1962), which was initially drafted in spring of 1960 and finished by the spring of 1961.

that break via his Bohm experience and ERE (1962). The break was not primarily about Feyerabend's criticism of Popper's propensity interpretation from Feyerabend's perspective. For consider, Collodel's assessment of the evidence:

> it must be stressed that in none of his recollections does Feyerabend seem to endorse a *direct, simple and straightforward*, connection between his theoretical pluralism and the lesson he allegedly learnt from Ehrenhaft's marginalisation. In addition, Feyerabend *never mentioned Ehrenhaft as a significant source of inspiration in any of his papers from the first half of the 1960s in which he advanced and developed his methodological proposal of a theoretical pluralism.* [760]

However, as we have seen, in a 1963 paper that is explicitly supposed to be a simplified summary of the views developed in ERE (1962), Feyerabend explicitly made such a direct connection between the lessons he had learned from his experience with impotence of Ehrenhaft's effects and the example that he used throughout his philosophy of science from ERE (1962) through to **Against Method** (1993) to illustrate this theoretical pluralism and his methodological argument for pluralism:

> the Brownian particle would have been regarded as an oddity much in the same way in which some of the late Professor Ehrenhaft's astounding effects. [761]

Collodel does not overlook this evidence, as in the case of the Watkins letter with respect to Feyerabend's break with Popper's school. Instead, he presents this evidence to criticize my account for *not making more of it* in support of my account (although this evidence was, as he notes, cited in my account in a different section, so he is right in so far as there should have been a cross-reference). He just does not appreciate the significance of this evidence to Feyerabend's test-model in contrast to Popper's, because he does not read ERE (1962) as a criticism of Popper's methodoloigcal views, but as merely an attempt to repeat it. Collodel acknowledges that, in 1963, Feyerabend already clearly emphasized that:

> Having witnessed these effects under a great variety of conditions, I am much more reluctant to regard them as mere curiosities than is the scientific community of today. [762]

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[760] Collodel, "Ehrenhaft's Experiments on Magnetic Monopoles…", p. 13 [emphasis added].

[761] Feyerabend, "How to Be a Good Empiricist…", p. 23.

[762] Feyerabend, "How to Be a Good Empiricist…", p. 23.

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When Feyerabend recollects that Ehrenhaft's lessons did not sink in until much later, he seems to mean until between 1957 and 1960, and not between 1967 and 1970. Furthermore, there is simply no lack of references in Feyerabend's work on theoretical pluralism in general methodology in the first half of the 1960 with respect to Ehrenhaft, which is not merely limited to general unspecific acknowledgments as a teacher (as Collodel would seem to have it), because the benefits of how Ehrenhaft taught were part of the lessons Feyerabend learned with respect to theoretical pluralism. Established theories only seem empirically secure when there is a lack of alternative theories that could show how they are wrong.

In addition to the explicit reference in "How to Be a Good Empiricist: A Plea for Tolerance in Matters Epistemological" (1963) that connects his theoretical pluralism directly to Ehrenhaft's effects in a "direct, simple and straightforward manner" and so indirectly to ERE (1962) in a straightforward manner, Feyerabend also mentions Ehrenhaft, or at least his physics, in his next paper on general methodology, "Realism and Instrumentalism: Comments on the Logic of Factual Support" published the very next year (1964), which is used to illustrate Feyerabend's views of "The Force of Empirical Objections" (which is the title of section 11 of the paper). [763] This time Feyerabend's reference appears to be very negative towards Ehrenhaft (as Collodel would have it, until his later retrospective projection that charlatanism is part of science) by lumping it together with "the hollow earth theory and astrology" in contrast to "the hypothesis of the moving Earth in the infinite universe". [764] Collodel seems to be implying that for Feyerabend, Ehrenhaft was a "crank" up until 1967 when Feyerabend converted to epistemological anarchism, and allowed charlatanism scientific status. But this is just misunderstanding Feyerabend's point in ERE (1962) compounded by misunderstanding its direct connection to his theoretical pluralism as clearly indicated in "How to be a Good Empiricist" (1963), and now again misrepresenting his views on Ehrenhaft as expressed in "Realism and Instrumentalism" (1964). Feyerabend is *not* implying that Ehrenhaft was a crank. He was saying we cannot know if Ehrenhaft really is a crank in advance of developing his ideas and testing them, which at that point he was still trying to do. Feyerabend's point in 1964 is that one cannot distinguish between science and charlatanism (between Copernicus and

[763] See Feyerabend, "Realism and Instrumentalism…", pp. 280–308.

[764] Feyerabend, "Realism and Instrumentalism…", pp. 280–308.

potentially Ehrenhaft) until after seemingly crazy new proposals have been given a chance to be developed and tested (hence his plea for more tolerance), not that he thought that Ehrenhaft was a charlatan and there was nothing to his effects. Quite to the contrary, Feyerabend is suggesting that Ehrenhaft should only be seen as a "crank" if he is unwilling to test his radical views, while emphasizing that he is not a crank just for proposing them.

The very next year, Feyerabend published "Problems of Empiricism" (1965), his next paper on general methodology. In this paper, Feyerabend repeats entire paragraphs verbatim [765] taken directly from his simplified summary of ERE (1962), entitled "How to be a good empiricist" (1963). As we have seen, How to be a Good Empiricist" (1963) Feyerabend clearly indicated the close connection between his test-model as initially set out in ERE (1962), and Ehrenhaft's effects, exactly with respect to the example he used to illustrate his test-model of a crucial experiment between incommensurable theories in contrast to Popper's falsificationism, which, he had argued, is limited to modeling testing commensurable theories within frameworks. This can more clearly be seen by situating Feyerabend's reference to Ehrenhaft (repeated verbatim in his 1963 simplified summary of ERE and his next major paper on empiricism published in 1965) in more context:

> As is well known, the Brownian particle is a perpetual motion machine of the second kind and its existence refutes the phenomenological second law. It therefore belongs to the domain of relevant facts for this law. Now, [...] (1) Could the relevance of the Brownian motion have been discovered in ["a direct manner, i.e. [...] by an investigation of the observational consequences of the phenomenological theory that did not make use of an alternative account of heat"]? (2) Could it have been demonstrated that it actually refutes the second law? The answer to the first question is that we do not know. It is impossible to say what would have happened had the kinetic theory not been considered by some physicists. It is my guess, however, that in this case the Brownian particle would have been regarded as an oddity much in the same way in which some of the late Professor Ehrenhaft's astounding effects are regarded as an oddity, and that it would not have been given the decisive position it assumes in contemporary theory. The answer to the second question is simply — No. Consider what the discovery of the inconsistency between the Brownian particle and the second law would have required! It would have required [...] measurements [which] are beyond

[765] Recall that Feyerabend pointed out to Watkins that he had repeated his criticism of Popper's methodology almost verbatim three times. Compare Feyerabend, "Explanation, Reduction and Empiricism..." (1962), Feyerabend, "How to Be a Good Empiricist...", and Feyerabend, "Problems of Empiricism...".

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> experimental possibilities [...]. Hence a "direct" refutation of the second law that considers only the phenomenological theory and the "facts" of Brownian motion is impossible. And, as is well known, the actual refutation was brought about in a very different manner. It was brought about via the kinetic theory and Einstein's utilization of it in the calculation of the statistical properties of the Brownian motion. [766]

Feyerabend does not mention Ehrenhaft by name explicitly in ERE (1962), but he explicitly discussed lessons he had learned from his Ehrenhaft experience more than once in that paper. For example, he had learned from the impotence of Ehrenhaft's effects that dogmatic acceptance of an established theory can lead to a situation in which:

> Disagreement with facts may of course occur, but, being now convinced of the truth of the existing point of view, its proponents will try to save them with the help of *ad hoc* hypotheses. Experimental results that cannot be accommodated, even with the greatest ingenuity, will be put aside for later consideration. [767]

Feyerabend thought that this is exactly what had happened to Ehrenhaft's effects, which he had indicated that he wanted to repeat in the mid-1950s and again in the mid- to late 1960s. And as we have seen, for Feyerabend, that is why incommensurable proposals that could lead to explanations of unexplained effects should always be welcome, even if they are *ad hoc*, that is, intentionally designed to take account of the known experimental results (like Bohm and Vigier's proposal to explain quantum indeterminacy as Brownian motion caused by a sub-quantum fluid). Universal theories can be protected from direct refutation by introducing *ad hoc* hypotheses, and they cannot be falsified by the confirmation of predictions deduced from rivals due to the meaning variance in the terms used to state them. Feyerabend's test-model based on conjectures and the corroboration of novel effects shows how there can still be crucial experiments between incommensurable rivals. According to Feyerabend's test-model, Ehrenhaft should have tried to potentially explain his effects by deducing them from a rival theory, as opposed to merely trying to refute established theories with them, while Bohm should have tried to predict and thereby discover a novel effect and not just try to re-explain a known effect (like quantum indeterminacy) and the successful pre-

[766] See Feyerabend, "How to Be a Good Empiricist...", pp. 23–24 and Feyerabend, "Problems of Empiricism...", pp. 175–176 and p. 249, fn. 119, as cited by Collodel, "Ehrenhaft's Experiments on Magnetic Monopoles...".

[767] Feyerabend, "Explanation, Reduction and Empiricism..." (1962), pp. 69–70.

dictions of the established theory within the limited domain in which it had been successful so far.

In addition to his major published papers on methodology in the first half of the 1960s after ERE (1962), Feyerabend's references to Ehrenhaft and his effects in his correspondence also show that his "renewed" interest in them happened more than year before his break with Popper's school in December 1967 and his shift to his epistemological anarchism. On the 13th of October 1966, in a letter to Watkins, Feyerabend wrote:

> I have *at last* found my notes on Ehrenhaft's lectures, I shall translate them into English + add a preface praising Ehrenhaft. Nobody has done this. Everyone, even his former pupils now smile contemptuously when they hear his name. But by attacking everything under the sun — Maxwell's equations, law of inertia, etc. He made us study the "classical" stuff he attacked much more attentively than we would have done otherwise. [768]

As we have seen, one of the main ideas Feyerabend initially developed in ERE (1962), which permeates his philosophy of science publications thereafter is that incommensurable alternatives (radically different conceptual approaches based on ontologically incompatible worldviews) can increase each other's empirical content as illustrated by the Brownian motion example. Conceptually incompatible theories can lead to new kinds of tests of established theories that are otherwise unavailable because the new theory is needed to discover the phenomenon that refutes the established theory by demonstrating that it implies the wrong ontology. As can be seen from the passage quoted from the letter to Agassi above, Feyerabend clearly also thought that he had learned from Ehrenhaft that the development of alternatives can help to improve our understanding of established theories, which is a step in that direction. Moreover, the formulation "I have *at last* found my notes on Ehrenhaft's lectures" implies that Feyerabend had been looking for them from even earlier. As we have seen, Feyerabend then even invoked Ehrenhaft in the Watkins letter in December 1967, while trying to explain that he is breaking from Popper's school with respect to Popper's methodology, given the unresolved dispute surrounding his criticism of it based on his idea of incommensurability since ERE. Watkins replied: "I read your Ehrenhaft appendix twice; it

[768] Feyerabend to Agassi, 13 October 1966, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...** [emphasis added].

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gave a charming picture". [769] In 1968, Feyerabend was happy to report to Agassi that:

> the same group have interested one of the major physicists here, Alvarez, in Ehrenhaft and there is a good chance that Ehrenhaft's experiments will be repeated right here in Berkeley as a result of my plea for a little charlatanry in general, and for Ehrenhaft in particular (*I translated Ehrenhaft's lectures for this express purpose*). [770]

So it would seem that Feyerabend had been hoping to repeat Ehrenhaft's experience since before October 1966, when he "finally" found his notes. Moreover, this was not the first time that Feyerabend had been eager to try to demonstrate some of Ehrenhaft's effects. Back in October of 1955, Feyerabend wrote to Popper from Bristol:

> I announced a special lecture on the foundations of quantum mechanics next term and that physicists are prepared to come. They are also prepared to repeat Ehrenhaft's experiments, especially the one with graphite dust. [771]

And on the 23rd of December 1955, Feyerabend was clearly still hopeful:

> There is a very definite hope to repeat Ehrenhaft's experiments here. I am now trying to get hold of the literature and if I have achieved that I shall try to get a working-place in the physics-department. [772]

At the turn of 1956, Feyerabend still clearly wanted to be a scientist, not a charlatan. At a minimum, he wanted to understand what can be learned from Ehrenhaft's effects with respect to methodology concerning the force of empirical objections" to established theories, as set out more generally in his views on theoretical pluralism in ERE (1962). He was still interested in 1963 and in 1965 he brought up the same point refencing Ehrenhaft very explicitly in conjunction with his conjectures and novel corroborations test-model for comparing incommensu-

[769] Watkins to Feyerabend, 30 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...**

[770] Feyerabend to Watkins, undated 1968, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...** [emphasis added].

[771] Feyerabend to Popper, 14 October 1955, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 196.

[772] Feyerabend to Popper, 23 December 1955, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)...", p. 208.

rable universal theories that was developed in contrast to and as part of a criticism of falsificationism in ERE (1962).

Unfortunately, it seems by February 1956, Feyerabend still had not dared to ask for permission to perform experiments in the laboratory:

> I did not yet dare to ask him [Pryce] for allowance to perform some experiment in his laboratory — what I would very much like to do. [773]

(Perhaps he did not *dare* to ask because he had failed to achieve the Ph.D. in physics he had sought). None of this is to say that Feyerabend did not later somehow re-evaluate his relationship to Ehrenhaft and his effects in the long term. In a letter to Watkin's in 1976, that is after **Against Method** (1975), Feyerabend wrote to Watkins: "I had many live father figures. Ehrenhaft was one of them and I *still* praise him". [774] By then, Feyerabend seems to have been much more comfortable giving Ehrenhaft this status.

One reason Ehrenhaft counts as one of Feyerabend's father figures is that Feyerabend's experience with the impotence of Ehrenhaft's effects in the late 1940s, which were dismissed with *ad hoc* hypotheses, motivated Feyerabend's investigations in methodology that led to his experience with Bohm in the late 1950s, whose theory was dismissed for being *ad* hoc, which led to ERE. When Feyerabend awoke from his Popperian slumber, he was watching Lakatos lecture on Popper's methodology, and realizing how far he had strayed from it:

> a change to a more Popperian face would most likely affect science in a very undesirable manner. For example, it would make it impossible for us to retain refuted theories with the help of *ad hoc* hypotheses until a new interpretation of experimental results is available that shows that they are right after all. Considering all this Popperianism seems now to me like *a nightmare.* [775]

In this way, Feyerabend's awakening seems to have been triggered by Lakatos's rendition of Popper's views on the same topic in methodology that di-

[773] Feyerabend to Popper, 6 February 1956, in: Feyerabend and Popper, "The Feyerabend–Popper Correspondence (1948–1967)…", p. 229.

[774] Feyerabend to Watkins, 13 January 1976, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians…** [emphasis added], as this implies that nothing had changed in this regard.

[775] Feyerabend to Watkins, 17 December 1967, in: Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians…**, see the "Appendix".

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rectly connects Feyerabend's Ehrenhaft experience to what he learned through his experience with Bohm and his hidden variables proposal, which led to the test-model Feyerabend developed in contrast to falsificationism in ERE (1962). Sometimes *ad hoc* maneuvers should be allowed, sometimes not, and only time will tell because revolutionary ideas always look irrational until a new framework ecomes accessible, with which it can be understood, and within which it can be fairly tested.

Summary of Part 3

In 1952, Feyerabend, who had just completed his Ph.D. in philosophy, having been disappointed in his attempt to become a physicist, witnessed an historic event, the reaction to Bohm's "hidden variables" in Copenhagen. After Bohr, who seemed astounded, had left the room, the Copenhagen school's reaction to Bohm's "hidden variables" proposal was dismissive. They cited von Neumann's proof, but did not seem to be able to explain it. This event shaped both areas of Feyerabend's active research in philosophy of science, in which he tried to contribute to promoting scientific progress in the foundations of quantum theory and more generally in methodology in both cases by criticizing dogmatic views purportedly holding it back. Feyerabend set out to get to the bottom of the dispute, and studied von Neumann's book very carefully in 1954 and 1955, and then lectured on it and Bohm's proposal in a course on the foundations of quantum mechanics in Bristol 1956. In 1957, Feyerabend tried to develop what he found out was a Bohrian strategy for solving the measurement problem, which he presented at the Colston conference where he first met Bohm. Feyerabend tried to develop this strategy and was very critical of Popper's propensity interpretation of quantum theory through the late 1960s. Feyerabend thought propensity interpretation does nothing to resolve the measurement problem. While Feyerabend's criticisms of Popper's views in the foundations of quantum theory in his publications in this period were clearly a source of tension in their relations, when Feyerabend awoke from his Popperian slumber, he announced his break with Popper's school because of the unresolved tension with respect to Feyerabend's Bohmian criticism of Popper's methodological views (falsificationism) that he had launched in ERE (1962), and what he took to be Watkins's dogmatic rejection of his proposal to have

a heart-to-heart on his criticism of Popper's methodology by running a seminar together on them. Watkins, who understood that Feyerabend was trying to criticize Popper's methodology in ERE since a draft of it in 1960, still could not see how Feyerabend's criticism applies to Popper's methodology views given that for Popper theories should not be deductively related just because otherwise deriving a contradiction for a falsification would be impossible and Popper readily acknowledges meaning variance. Watkins found Feyerabend's attempt to criticize Popper's views on method and progress unfruitful, and so proposed that they run a seminar together on his criticism of Popper's views in the foundations of quantum theory *instead*. Feyerabend accused Watkins of being stuck in a cloud of Popperian ideology and he announced his break to with the Popperian school to Lakatos and Watkins on the same day: 17 December 1967 because of his rejection of Popper's methodology as "anti-humanitarian" and not because of his criticism of Popper's views on the foundations of quantum theory.

Feyerabend had eagerly read Bohm's **Causality and Chance** (1957) shortly before the conference, which was the next historic event that shaped Feyerabend's developing views. Feyerabend found that he and Bohm shared many similar ideas, although they came from very different philosophical backgrounds. And while Feyerabend rejected Bohm's dialectical philosophy, he was clearly eager to take up Bohm's cause in the battle to fight the dogmatism of the proponents of the Copenhagen interpretation, and their dismissal of Bohm's "hidden variables" proposal as *ad hoc*. By 1960, he was also clearly eager to take up Bohm's cause with respect to criticizing Popper's methodology given that universal "framework" theories are not falsified. According to Feyerabend, following Bohm away from Popper, they are superseded by better corroborated (more general) rivals, as illustrated by Bohm's example of Einstein's predictions of Brownian motion as a crucial experiment corroborating kinetic theory. There was a crucial experiment despite the meaning variance in the terms used to organize the observations into evidence, rendering the rival theories deductively disjoint or "incommensurable".

Einstein, Bohr, and Bohm were among Feyerabend's philosopher-scientist heroes. They were *worldview* shattering intellectual giants. Feyerabend wanted to show how they did it. He was trying to help Bohm revolutionize quantum theory in the same way Einstein had revolutionized classical mechanics. Feyerabend's advice to Bohm seems to boil down to that Bohm should not bother with trying to falsify quantum theory by predicting statements from his proposal that contradict

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some untested predictions deduced from the established theory, but rather try Bohm should try to challenge its ontological principles by trying to predict some disturbing new effect that the established theory could never by itself have discovered that proves that it is wrong. In this way, one could potentially explain something the established theory cannot while also showing that the established theory is fundamentally wrong about. This can be done introducing incompatible rules of usage to the terms used for state the established theory. In that way, you can replace the entire conceptual framework at once, with a new framework that can be used to state a new theory based on incompatible ontological principles. This is just what Bohm and Vigier seem to have been trying to do. However, the dogmatism of the Copenhagen interpretation had accepted the indeterminacy in question as an irreducible element of quantum theory, which they were trying to use and further develop, and had no interest in seeing replaced. The Copenhagen school wanted to free physics from philosophy and not philosophize about physics. From Feyerabend's perspective, they were dismissing Bohm's revolutionary proposals on questionable methodological grounds, and as mere attempts at a determinist interpretation of quantum theory, when his proposal was neither deterministic, nor hould it have been just another interpretation of the established quantum theory. It was a proposed explanation of the queerly indeterministic results successfully predicted from the established theory that was intended to replace the established theory and provide a new realistic interpretation of the quantum and sub-quantum levels.

In 1957 at the Colston conference, Feyerabend met Bohm and tested out his ideas on meaning change and methodology with respect to the basic concepts used to test theories empirically and their methodological implications. Feyerabend and Bohm encouraged each other, and they worked closely over the next couple of years. ERE (1962) is clearly the result of this collaboration. It seems likely that Bohm suggested that Feyerabend look more closely at Einstein's accomplishment and develop the Brownian motion example as a criticism of Popper's falsificationism, as he himself was trying to do (although Bohm had not read **The Logik**, which only appeared in English in 1959). That is likely how Feyerabend ran into the term "incommensurable" in Einstein's autobiographical remarks on his Brownian motion prediction, which Feyerabend used to express the idea that the meanings of basic concepts change through revolutionary transition of universal framework theories and the worldviews they are taken to imply,

which fit with the ideas he had been developing since the early 1950s, which he only developed into a criticism of falsification in ERE 1962.

Initially, Feyerabend brought Bohm to philosophy through Popper and his views on propensity and methodology. In 1960, Feyerabend published a review of Bohm's book on **Causality and Chance in Modern Physics** (1957), criticizing Bohm's dialectical methodology, while explicitly supporting *Popper's falsificationism*. In 1959, Bohm wrote a 38-page response to Feyerabend's review that had not yet been published. It is an extensive critical comparison of his own methodological views with his understanding of falsificationism. Feyerabend discussed Popperss falsificationism at length with Bohm, trying to ameliorate their positions. ERE (1962) develops and sharpens Bohm's attempt to criticize falsificationism in his 1959 response to Feyerabend's critical review, and Bohm's first philosophy paper in 1961. Feyerabend suggested that there is a problem in Popper's falsificationist account. There is an irresolvable tension between meaning variance and formal relations required for a falsification with respect to testing incommensurable rivals. Feyerabend tried to show how there is a problem of explanation due to meaning variance in Popper's empiricism, which gets glossed over in Popper's account of basic statements. Bohm and Feyerabend worked as compatriots in the philosophy of science until they parted ways in the early 1960s Bohm's criticism of falsificationism, according to which Newton's framework theory was never falsified, led to Feyerabend's criticism of falsificationism based on incommensurability. It was that criticism of falsificationism that was at the heart of Feyerabend's break with Popper's school on 17 December 1967.

Acknowledgements

I would especially like to thank Matteo Collodel without whose careful and diligent work I would never have tried to develop my account in such detail. By collecting and editing Feyerabend's correspondence, the biographical details that he has brought to light and made available to me, as well as the many discussions we have had about Feyerabend during our ongoing collaborations have helped to shape and inform my views in too many ways to try to specify. I would also especially like to thank Wolfram Eilenberger, whose insightful talk, "December 1967 — Feyerabend's Awakening" at the ETH Zurich Paul Feyerabend centennial work-

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shop on the 22nd of March 2024, inspired the use of that phrase. Thanks to Matteo Collodel for his very helpful comments and suggestions on a nearly complete draft, and for providing the two new letters from Feyerabend to Kuhn in the 1980s (at the last minute) and especially for suggesting that a more fine-grained approach to Popper would clarify my account of Feyerabend's relation to him. Thanks also to Marij van Strien for helpful comments on the Bohm section, and Gunter Teubner for a long discussion of it. I would also like to thank Paul Hoyningen-Huene, Karim Bschir, and Leandro Giri more generally for helpful discussions on many of these issues. I would also like to thank Matteo, Stefano Gattei, Joseph Agassi, Friedrich Stadler and Daniel Kuby for motivating me to write this essay. I would like to express my gratitude to Grazia Borrini-Feyerabend for her trust and support (Paul's skin is intact) and to Jochen Dreher (at the philosophical archive at the University of Konstanz where the Feyerabend *Nachlass* is stored) for his. Special thanks to the German Research Council, Olaf Muller, and the philosophy department at the Humboldt University of Berlin for supporting the research that led to Part 3, as part of a project to finish **Feyerabend's Formative Years**. Finally, thanks to Anna Malec for helpful suggestions on the final draft and especially to Grzegorz Malec and *Filozoficzne Aspekty Genezy* for encouraging me to write this essay and the patience to wait for me to finish it.

I would also like to take this opportunity to dedicate this work to my family, especially my wife Niki and my three children Trek, Benji and Paul, and to thank them for their support.

Eric Oberheim

Appendix: The Watkins Letter: Feyerabend to Watkins 17 December 1967 [776]

University of California, Berkeley, Department of Philosophy
17 December 1967

Dear John,

the day after our big meal at Bertorelli's (for which, I am sorry to say, I had to pay myself) I took a look at the sky, packed my suitcases, and hopped on the next plane available — which I caught at a margin of 10 minutes. I had a marvell ous flight — 10 hours and not a second more to Los Angeles, a whole row of seats to myself but when I arrived in San Francisco I was greeted by a storm of 80 miles/ hour and 25 degrees temperature. The fence in front of my house is blown to pieces, in the back a big eucalyptus has fallen over and has smashed the platform. Ludwig's fountain at the university is frozen — a historical event unknown to the present generation and to its predecessors also. Which is more than can be said about my alleged change from Bohm to Bohr. For I discovered to my surprise that I know my own writings as badly as you do. There exist articles in 1959, and there is especially my long article "Problems of Microphysics" of 1962 where my position is as follows: Bohr's interpretation of quantum theory is the best interpretation on existence; it is better than Popper's; but the attempt to show that it is not only excellent, but also the only possible interpretation must be rejected. There is a criticism of Popper already in my paper of 1959 and it is very close to the criticism I am now using against him. It was *in 1959* that Hanson recorded "surprise, and, indeed, a great shock" at my change of position (he was the commentator on a paper of mine which was read at the Chicago meeting of the American Association for the Advancement of Science). So there *was* a change of position. There *was* surprise at this change of position. But all this took place in 1958 or even 1957 *and not* in 1967. And if you look at my anti-Karl paper a little more carefully you will discover that I have taken 1 and ½ pages *verbatim* from my 1962 paper (which contains verbatim passages from my 1959 paper — another fine occasion to say that I say something, but now only "for the third time": Do you think I am doing such things only in "Problems of Empiricism"?). All these things I had for-

[776] JW 170, excerpted from Collodel and Oberheim, **Feyerabend's Formative Years, Volume 3, Feyerabend among Popperians...** (with minor corrects).

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gotten when we discussed the matter. I can even tell you that one of the reasons why I was mad at Popper was that his paper did not pay any attention to my criticism of 1962. Maybe he had not read my paper (which I sent him); maybe he did not like it. But I thought I had refuted him and not a word from him on this matter. Well, so much about that.

Of course, there still remains the problems, discussed by us in the restaurant, whether changes of position should be *announced*. Now I can quite understand your position and I am going to describe a situation in which I would be prepared to accept it. Assume there is a Popperian journal in the philosophy of science and the contributors know each other very well. Then it would be quite reasonable to expect from me some explanation as to why I had changed my position on a certain matter, an explanation that goes beyond the use of arguments for my new position. Friends, after all, have a right to know, or rather, they do not have a *right*, but they may reasonably expect to be told. This is the reason why I told the class whose last meeting you attended why I could no longer follow certain tenets of Popperianism (students of Popperianism I regard as friends in a wider sense). I told them what my problems were and why I believed that Popperian methodology did not solve them. It is reasonable to expect that one will proceed in this way. But to adopt this procedure *with perfect strangers* seems to me to be going too far. And the fact that these strangers belong to a certain *profession* does not impress me in the least (you know what I think about professions). You might object that I publish in the professional journals. My reply is that I do not publish in the professional journals but in "friendly" publications such as the Pittsburgh Studies, the Minnesota Studies and so on. I also wanted to publish the anti-Karl paper in what I regarded a "friendly" journal, viz. BJPS only to hear that BJPS was not a friendly journal after all. And now I sent it to the American Journal, a first instance of a publication in an unfriendly journal, but the situation here is very peculiar at any rate.

Now to move on to the next point. You two — Imre and you — are *very lucky fellows*. You know what you want; you know roughly where the truth lies; and so you move towards it. You have found your style [though Imre almost lost it again through corroboration, confirmation, acceptance i, ii, iii, and other *puerilia*]. But I, right now, am at a complete loss. I have been teaching Popperianism for now about eight years, over 10 years when I include my time in Bristol. *I gradually changed as I went on. And now when I finally heard Imre's crystal clear lecture I re-*

alised why I had changed. I had a curious feeling of utter unreality. Here one was supposed to hear something about scientific theories about Einstein's theory of gravitation, about its relation to Newton, Kepler, etc. And not a single thing was correct. Moreover, a change to a more Popperian face would most likely affect science in a very undesirable manner. For example, it would make it impossible for us to retain refuted theories with the help of ad hoc hypotheses until a new interpretation of experimental results is available that shows that they are right after all. Considering all this Popperianism seems now to me like a nightmare, not at all better than Hegelian philosophy of Nature. What now? In addition to being unrealistic the Popperian philosophy of science is also antihumanitarian: it stops midway between human interest and professional interest (the interest in "Truth" is a purely professional interest). All these things are not arguments, but series of motives. Popper's philosophy is to me the one most acceptable. And yet it now turns out to be just another nightmare. So, for the time being, I am lost and can only look with admiration to people such as you who are lost only locally but whose ideology is otherwise quite safe.

Considering this — what was your impression of my last "lecture"? It is a pity that you did not hear more of them, for I would have liked to have your judgement. Imre who was there did not give me any criticism. Nor did I hear any criticism from the students. Helena, beautiful blond Helena kept silent — and I would have liked to know what a clever girl like her thought about the whole jazz (perhaps you can ask her? *I would be very interested in her opinion* — and not merely because I am attracted to her). But you — can you give me a few helpful hints concerning the things you *did* hear?

I have now four months entirely to myself. I should write 3 books. Two in German (a history of the Philosophy of Nature, 300 pages; a history of 20th century physics in its relation to philosophy; also 300 pages) one in English (an introduction into the philosophy of science). In my present mood I think the best thing will be to write the *historical books*.

Did I send you my translation of Ehrenhaft's lectures? Read the *second appendix* (the last seven pages) which is an eyewitness account of Ehrenhaft in Vienna and of a certain dramatic discussion between Ehrenhaft, Rosenfeld, Popper and others. I think you will be amused. You can show your gratitude by sending me an amusing letter in turn.

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All the best, also to Micky and hope to see you soon with a new and offensive ideology on my back,

Paul

P.S. Have just read an earlier anti-Feigl paper of mine which is much ruder than my anti-Karl paper — and yet, no trouble. Same with Putnam.

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