CZASOPISMO INTERNETOWE/ONLINE JOURNAL

ISSN 2299-0356

Filozoficzne Aspekty Genezy

Philosophical Aspects of Origin

PÓŁROCZNIK/BIANNUAL

2023

tom vol. 20(2)

numer specjalny special issue 1

Understanding Reality

On the Centenary of Paul K. Feyerabend's Birth

Edited by

Gonzalo Munévar

Krzysztof J. Kilian

Grzegorz Malec

CZASOPISMO INTERNETOWE/ONLINE JOURNAL

ISSN 2299-0356

Filozoficzne Aspekty Genezy

Philosophical Aspects of Origin

PÓŁROCZNIK/BIANNUAL

2023

tom 20(2)
vol.

numer specjalny 1
special issue

Understanding Reality

On the Centenary of Paul K. Feyerabend's Birth

Instytut Filozofii
Uniwersytet Zielonogórski
Zielona Góra 2023

www.fag.ifil.uz.zgora.pl

PÓŁROCZNIK/BIANNUAL
2023
tom 20(2)
vol.
numer specjalny 1
special issue

Editorial Office
Philosophical Aspects of Origin
Institute of Philosophy
University of Zielona Góra
Al. Wojska Polskiego 71A
65-762 Zielona Góra, POLAND

EN ARCHE
wszechświat człowiek nauka

This volume was produced with financial support from the En Arche Foundation

Contents

Vol. 20, No. 2 2023 Special Issue 1

Contents

2023 Vol. 20, No. 2 Special Issue 1

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 5–6

https://fag.ifil.uz.zgora.pl/index.php/fag/issue/view/24/69

About the Journal

Published online: May 28, 2024.

Philosophical Aspects of Origin (Polish title: *Filozoficzne Aspekty Genezy*) (ISSN 2299-0356) has been published continuously since 2004. In 2022, the journal became a biannual. It is a highly specialized online philosophical journal which, since its inception, has provided free access to all of its contents. The main focus of the journal is examining the concept of *origin* in its broad sense: i.e. the origins of the Universe, of early and advanced life forms, humans, mind, consciousness, language, scientific theories, religion, etc. The contents of the journal also include reflections of a philosophical and methodological nature that concern theories and perspectives relevant to this topic.

While this field of philosophy covers a wide variety of important and interesting issues, the journal particularly concerns itself with the following: controversies stemming from such opposing world views as naturalism and anti-naturalism, evolutionary theory and the theory of intelligent design, or evolutionary theory and creationism, together with controversies that arise on the fringes of science, religion, ideology and world views, and also issues connected with the role of facts and non-empirical convictions in the formulation of scientific theories. *Philosophical Aspects of Origin* also publishes texts dealing with issues that fall within the scope of the natural and social sciences and the humanities. However, there is one caveat: these should also address philosophical problems (including meta-scientific reflections), and discuss topics useful to philosophical analyses. After all, a journal that is philosophical in nature is not supposed to serve as an

open-ended platform for attempts to determine the empirical adequacy of some theory or other.

An important part of the journal consists of translations of valuable foreign-language texts that deal with the aforementioned issues. Some of these were published quite a long time ago, and that is why they deserve to be recalled; others, that are more contemporary, take up issues relevant to a particular issue of the journal, and this is also a good reason to present them to the Reader.

The editorial board of *Philosophical Aspects of Origin* subscribe to a pluralistic, Feyerabendian approach to the pursuit of knowledge. We believe that each and every view has a right to be taken into account in discussion. Even if erroneous, it can still advance scientific progress through the clash of opposing views, which often brings about refinements in argumentation. Our goal is to create a space for open debate, in which many different voices can be heard. If an article is well-argued and well-written (which does not imply that the editorial board must agree with its premises), then it can be published on the pages of *Philosophical Aspects of Origin* — providing that it passes our peer-review process.

Krzysztof J. Kilian

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 7–11

https://fag.ifil.uz.zgora.pl/index.php/fag/issue/view/24/70

Introduction

Published online: May 28, 2024.

Paul K. Feyerabend is both one of the most important philosophers of the 20th century and a thinker who is difficult to interpret. He worked for almost forty years, changed his views, and did not seek to present them systematically. He has been labelled in various ways, ranging from "logical empiricist" to "postmodernist," from "scientific realist" to "scientific anti-realist," from "rationalist" to "irrationalist," from Popperian to Wittgensteinian, and from neo-Kantian to neo-Hegelian or Marxist.

For some, Feyerabend was the worst enemy of science, whilst for others he represents the worst enemy of the scientific establishment — the fiercest critic of all shades of conformity and of the *status quo*. This diversity of labels and assessments suggests that one cannot encounter his philosophy while remaining indifferent. Such a conclusion is borne out by the enormous amount of literature devoted to him, along with discussions and controversies, divergent interpretations, and hasty judgements.

Feyerabend's work has been the subject of many studies and polemics, both because of the issues he addressed and the way he wrote, which is often rather obscure and not very attentive to detail. As a philosopher of science, he challenged the idea of its cumulative development, questioning the hitherto commonly accepted assumption of its rationality, along with the convictions that modern theories are better than their predecessors because they are devoid of superstition, broader because they cover a wider range of phenomena, and deeper because they explain those aspects of the world into which research is conducted with

fewer, more basic principles. He defended the position that some successive theories, which he called "universal theories", are incommensurable: that is, incomparable in some important respects.

Feyerabend interpreted many important episodes in the history of science in ways that sparked controversy. He maintained that traditional approaches to scientific knowledge and methodology are flawed, because scientists do not act "rationally" as philosophers construe this word.

He demanded that science, like other institutions of a free and democratic society, should be subject to democratic control. He argued that cultural diversity brings benefits, while monotony limits humanity. As a philosopher, Feyerabend searched throughout his life for a worldview in which pluralism and diversity of ideas would play a significant role, calling in this context for a reshaping of society.

The 100th anniversary of his birth offers an appropriate occasion for revisiting his philosophy and highlighting its most valuable aspects.

In preparing this volume, we noticed that, in principle, the common denominator for all the texts presented here is the idea of understanding reality. Feyerabend's propagation of pluralism, his emphasis on the importance of diversity, his constant readiness to change his own beliefs, his justification of the conviction that there are satisfactory forms of knowledge other than science, his attempts to defend science against various ideologies that impose a single understanding on it can be read as extravagant philosophy. However, they can also be read as attempts to eliminate all those constraints that attempt to reduce reality, expressed in various forms of knowledge, science and culture, to some single idea representing the favoured beliefs of those who promote that idea.

This richness of Feyerabend's philosophy is also evident in the texts presented in this volume.[1] They are divided into four sections.

The first section — **Interpretations** — consists of five texts.

Francesco Coniglione, in his paper "Pluralism and Mysticism in the Thought of Paul K. Feyerabend", proposes a new and different periodisation of Feyerabend's

[1] One of the texts was extensive, we decided to publish it as a separate special issue. The essay by Eric Oberheim, titled: "On the Limited Validity of Falsificationism: Feyerabend's Theoretical Pluralism and its Relation to Popper, Wittgenstein and Bohm" will soon be available to our readers.

oeuvre from those commonly known. This interpretation of Feyerabend's work supports Cogniglione's justified thesis that Feyerabend's mature and late views focus on four issues: (a) the thesis of methodological pluralism; linked to this thesis is (b) the thesis of scientific pluralism (there are many ways of modelling and scientifically investigating reality); (c) the thesis of the impossibility of fully understanding and articulating the method used in scientific research; (d) the thesis of the existence of many forms of life that do perfectly well without science.

Gonzalo Munévar in his paper "Feyerabend: The Most Valuable Philosopher of the Twentieth Century" justifies the thesis that Feyerabend is such an important philosopher in the 20th century because he offered the fullest understanding of how science is practised and also explained the impact of science on the rest of culture. Of critical importance in this regard is Feyerabend's case for theoretical pluralism, which overturned key ideas from analytical philosophy by demonstrating that all scientific rules, no matter how sound and empirically fruitful, must allow for exceptions. Munévar compares Feyearabend's achievements with those of other important philosophers such as Thomas S. Kuhn, Karl R. Popper, Ludwig Wittgenstein, Martin Heidegger, John Rawls, Rudolf Carnap, Willard Van Orman Quine, Bertrand Russell and John Dewey.

Matteo Motterlini in a letter to the editor entitled "The Legacy of Paulus Empiricus" presents a philosophical profile of Paul K. Feyerabend, demystifier of three idols: the idea of fundamentalist epistemology; the idea of practising science according to a historically immutable set of rules; the idea of the unity of science.

Yuanlin Guo and Chubi Yan in their paper "Paul Feyerabend and Marxism" discuss the relationship between Feyerabend views and Marxism. The authors juxtapose Feyerabend's views with those of many Marxists and leftists. They also attempt to answer the questions: was Feyerabend a Marxist; was he a dialectical materialist? The authors present arguments for the thesis that Feyerabend was primarily a Dadaist, which allows them to argue that he could have been both a Marxist and an anti-Marxist.

Donald Gillies in his paper "Feyerabend's Criticism of Kuhn", argues that this Feyerabendian critique is largely valid, while Kuhn failed to answer all of Feyerabend's fundamental objections. The author also responds to Feyerabend's critique by reviving the empiricist idea of the inductive justification of scientific theories by observation statements. This allows him to argue that there are rational

reasons for choosing among competing paradigms. In turn, this leads him to the thesis that scientific revolutions are rational.

The second section — **Early Philosophy** — consists of two texts.

David Lamb in a letter to the editor entitled "Feyerabend Letter: Some Thoughts on the Two Context Distinction" analyses Popper's and Feyerabend's positions on distinguishing the context of discovery from the context of justification.

George Couvalis, in his paper "Riffing on Feyerabend: Direct Observation, Paraconsistentist Logic, and a Research Immanent Account of the Rationaliy of Science", analyses some of the themes in Feyerabend's early writings and in his *Opus magnum* that have not been analysed in detail before. These analyses are enriched by references to those authors (Jerry Fodor, Dudley Shapere, Chris Mortensen, Willard Van Orman Quine) in whom considerations very reminiscent of those of Feyerabend can be found. Couvalis also presents the thesis that Feyerabend never succeeded in answering the challenge to his relativism posed by Shapere, and the latter's work "The Character of Scientific Change" provides a well-worked out alternative to Feyerabend's relativism.

The third section — **Mature Philosophy** — consists of two texts.

Sergio Benventuro, in his paper "Paul Feyerabend's Contribution: the Anarchic Sunset of the Philosophy of Science", presents arguments in favour of the thesis that Feyerabend contributed to overcoming two opposing models of knowledge; one, the contemplative one, which assumes the objectivity of knowledge and the other, which makes knowledge a very human tool for power, domination and survival. Benventuro also argues that Feyerabend's significance lies not in the fact that he proposed a new account of the philosophy of science but in the fact that he led to the collapse of the most influential current of twentieth-century epistemological thought marked by names such as Mach, Popper, Quine, Kuhn and Lakatos.

Krzysztof J. Kilian, in his paper "What is Epistemological Anarchism?", substantiates the thesis that epistemological anarchism is methodological fallibilism, i.e. an approach according to which all scientific knowledge is not only provisional, but so are the methods of acquiring it. Not only are we doomed to a provisional character of knowledge, but that we have no permanent guide to this provisional knowledge.

The fourth section — **Late Philosophy** — consists of three texts.

Ian James Kidd, in his paper "Feyerabend on Pluralism, Contingency, and Humility" substantiates the thesis that throughout the writings of Paul Feyerabend, there are constant references to the historical contingency of the scientific enterprise, often accompanied by philosophical claims about the significance of that contingency. Kidd's paper presents those contingentist claims, situates them in the context of more recent work on the contingency of science, and offers an interpretation of their significance. The author suggests that Feyerabend's sense of contingency was connected to his defences of pluralism, and also to the "conquest of abundance" narrative developed in the very late writings.

In a letter to the editor entitled "We Can Choose to Live in a World that Makes Sense to Us", Grazia Borrini-Feyerabend writes about how the publication of Paul K. Feyerabend's latest book **Conquest of Abundance: A Tale of Abstract versus the Richness of Being** came about and the role Bert Terpstra played in its publication. Feyerabend did not complete the writing of this book. This unfinished manuscript was supplemented by several other texts written by Feyerabend that dealt with the issues raised in the book **Conquest of Abundance**. It tells the story of certain particular moments in evolving Western culture, times in which complex worldviews, filled with an abundance of possible interpretations of being – and thus of reality – gave way to a few abstract concepts and stereotypical descriptions.

Paul K. Feyerabend in his paper "Knowledge without Epistemology" substantiates the thesis that knowledge without epistemology is possible. According to the author the universality of scientific principles, theories, laws is never purely "objective", it has a strong anthropological component. A theory of knowledge invoking transhistorical agencies is therefore not only dead — it was never alive; its so-called successes are nothing but an immense chimera. Scientific research knows no universal boundary conditions or standards whether of a conventional, aprioristic, or empirical kind but uses and invents rules according to circumstance without regarding the selection as a separate "epistemic" act and often without realising that an important choice is being made.

Gonzalo Munévar
Krzysztof J. Kilian
Grzegorz Malec

Interpretations

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 15–54

https://doi.org/10.53763/fag.2023.20.2.227

ARTYKUŁ ORYGINALNY / ORIGINAL ARTICLE

Francesco Coniglione

Università di Catania

Pluralism and Mysticism in the Thought of Paul K. Feyerabend

Received: November 8, 2023. Accepted: December 6, 2023. Published online: March 22, 2024.

Abstract: Feyerabend's positions regarding methodological pluralism and the consequent critique of the monism of Method proposed by the Popperian school are well known. Less analyzed is the significance of his scientific pluralism and the idea that a multiplicity of cognitive approaches to reality is possible, especially in relation to its "abundance" — the many ways in which it presents itself, its complexity, and the fact that consequently it can be approached and interpreted from different points of view. This aspect has led Feyerabend's reflections to emphasize what is typically relegated to the realm of the irrational, and has enabled him to discover the extent of the unspoken and implicit aspects of scientific knowledge, thereby emphasizing the mystical dimension of humanity's relationship with the world, which usually escapes rational analysis.

Keywords:

Feyerabend; mysticism; pluralism

Feyerabend's role in shaping the discourse on science since the dissolution of the Popperian approach and the crisis of the so-called Received View [1] is well known. Therefore, in this essay I will not focus on his contributions to the discussion of problems in the theory and methodology of science. Instead, I will explore the phase of his thought that marks a gradual shift of interest from them [2] to an ever more careful exploration of both the rich material to be found in the history

[1] See, for this expression, Hilary Putnam, "What Theories Are Not", in: Ernest Nagel, Patrick Suppes, and Alfred Tarski (eds.), **Logic, Methodology and Philosophy of Sciences**, Stanford University Press, Stanford 1962, pp. 240–251.

of science (in order to show the narrowness and myopia of any methodological rule) and in the cultures considered "non-scientific" (in order to critique the claims to absoluteness of contemporary science and Western rationality). This transition is a manifestation of the "turn towards history" [3] that has characterized epistemology since Kuhn. In this way, Feyerabend reveals a vast culture that extends beyond his previous specialist field; he is now able to master with admirable competence areas such as general philosophy, anthropology, history (not exclusively related to science), politics, as well as sectors traditionally alien to the rationalist approach, such as myth, art, religion, esoteric doctrines, etc., towards which his colleagues in philosophy of science departments are — in his opinion — "donkeys with shoes".

Therefore, this study will relegate to the background Feyerabend's well-known critical positions on the specialized and "internal" issues of the philosophy of science, and concentrate on his mature thought, beginning after the death of Imre Lakatos in 1974.

In fact, Feyerabend's intellectual development can be divided into three phases. In the *first*, his interest was directed toward the "internal" problems of philosophy of science and Popperian rationalism. His critical engagement with these issues led him to develop an increasingly critical attitude, which he further refined after meeting and becoming acquainted with the work of Lakatos. This marked the beginning of what might be called the *second phase* of his thought. The work that marks the culmination and at the same time the conclusion of this second period is represented by **Against Method**, published in 1975 but written in the years 1972–1974, before the death of Lakatos, and conceived as the first part of a book on rationalism, the second part of which would have to must be written

[2] "[G]radually I became suspicious about regulating knowledge from afar, with the help of principles and abstract models. The world and human lives especially, I thought, are much too complex for that". (Paul K. Feyerabend, "Concluding Unphilosophical Conversation", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Springer Science+Business Media B.V., Dordrecht 1991, pp. 492 [487–527]).

[3] See Hans-Jörg Rheinberger, **On Historicizing Epistemology: An Essay**, Stanford University Press, Stanford 2010; John H. Zammito, **A Nice Derangement of Epistemes: Post-positivism in the Study of Science from Quine to Latour**, The University of Chicago Press, Chicago — London 2004, pp. 90–111.

by the friend, in the meantime deceased. [4] The death of Lakatos, »one of the best friends I ever had«, [5] marks the beginning of the *third and final phase* of his thinking, which has a turning point since Lakatos, as long as he was alive, had ensured Feyerabend's closeness to the Popperian group at the London School of Economics: "If Feyerabend before had defended science against philosophy of science, he now criticizes mainly science itself because it would be a main obstacle on the road towards a free society", [6] threatened above all by the power of the experts. During this last phase Feyerabend underwent a gradual radicalization and a broadening of horizons: works like "Erkenntnis für freie Menschen" [7] and "Farewell to Reason" [8] were published, reaching a climax in the last works, especially the posthumous **Conquest of Abundance**. [9]

This periodization — which differs from the others proposed [10] — is primarily characterized by the transition from critique of methodology, with the subsequent advocacy of pluralism, to the critique of science itself and the questioning of the notion of scientific monolithicality. As a result, Feyerabend increasingly delves into topics that extend beyond the boundaries of the philosophy of science. How-

[4] Paul K. Feyerabend, **Against Method. New Edition**, Verso, London — New York 1993, p. vii. A first exposition of the ideas presented in this volume can be found in the long essay Paul K. Feyerabend, "Against Method: Outline of an Anarchistic Theory of Knowledge", in: Michael Radner and Stephen Winokur (eds.), **Analyses of Theories and Methods of Physics and Psychology,** Vol. 4, *Minnesota Studies in Philosophy of Science*, University of Minnesota Press, Minneapolis 1970, pp. 17–130.

[5] Feyerabend, **Against Method...**, p. vii.

[6] Werner Diederich, "Obituary on the »Anarchist« Paul Feyerabend", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Springer Science+Business Media B.V., Dordrecht 1991, p. 214 [213–224].

[7] See Paul K. Feyerabend, **Erkenntnis Für Freie Menschen**, Suhrkamp Verlag, Frankfurt am Main 1979; this is the German version of **Science in a Free Society**, New Left Books, London 1978, but very different, being in some respects expanded and in others reduced; I will therefore quote from one or the other as needed.

[8] Paul K. Feyerabend, **Farewell to Reason**, Verso, London — New York 1987.

[9] Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago — London 1999.

[10] See Matthew J. Brown and Ian J. Kidd, "Reappraising Paul Feyerabend", *Studies in History and Philosophy of Science* 2016, Vol. 57, p. 3 [1–8], https://doi.org/10.1016/j.shpsa.2015.11.003; John Preston, **Feyerabend: Philosophy, Science and Society,** Polity Press, Cambridge 1997, *passim*; Eric Oberheim, **Feyerabend's Philosophy**, *Quellen und Studien zur Philosophie*, Band 73, Walter de Gruyter, Berlin — New York 2006, *passim*.

ever, it is essential to acknowledge that this schematization is a rough approximation and is often problematic, as Feyerabend, in the more mature stages of his thought, takes up, reuses and recontextualizes arguments from earlier phases.

1. Methodological Mysticism

Feyerabend's critique of the methodology of science, influenced also by authors outside the mainstream of epistemology such as Michael Polanyi and Ludwik Fleck [11] (the latter very influential on Kuhn), particularly emphasizes the contrast between concreteness and abstraction, between the richness of the historical process and the poverty of methodological reflection. This critique leads Feyerabend to formulate the slogan that made him famous: "Anything goes".

Nevertheless, it would be incorrect to assume that Feyerabend is thereby claiming that "we must live without rules". [12] On the one hand, because this statement "was meant for the sciences, but not for everything", [13] and on the other hand, because it would be a mistake to think that scientific research proceeds haphazardly, without any guidelines or rules, or to suggest that there are no rules and norms that are respected in certain periods, sectors, or traditions of research: "anything goes — anything, that is, that is liable to advance knowledge *as understood by a particular researcher or research tradition*". [14] What Feyerabend is committed to, at heart, is the rejection of two typical theses of Popperianism and the Received View:

(a) *Firstly*, the belief that science is guided by universal standards that remain constant across time and space. These standards are viewed as defining science and serving as a kind of identity card, in line with the old traditional Cartesian project and the thesis of so-called »methodological monism«, [15] which implies the detachment and superiority of science relative to research traditions that do not apply the »scientific method«.

[11] Feyerabend, **Farewell to Reason**..., pp. 190, 282.

[12] Paul K. Feyerabend, "Die »Rationalität« der Forschung", in: Paul K. Feyerabend, **Der wissenschaftstheoretische Realismus und die Autorität der Wissenschaften**, Friedr. Vieweg & Sohn, Braunschweig — Wiesbaden 1978, p. 345 [339–350].

[13] Renato Parascandalo and Vittorio Hösle (eds.), "Three Interviews with Paul K. Feyerabend", *Telos* 1995, p. 129 [115–128].

[14] Feyerabend, **Farewell to Reason**..., p. 36 [emphasis added].

For advocates of methodological monism, abandoning or denying it is tantamount to embracing relativism and thus descending into irrationalism: the development of science is no longer explained in terms of rational progress. Feyerabend, on the other hand, advocates for "methodological pluralism": methods are always context-dependent, possess different ranges of application, are historically determined and evolve across different research traditions.

(b) *Secondly*, Feyerabend argues that these criteria are often difficult to formulate explicitly anyway, whether they be the universal and normative methods envisioned by the rationalists or the contextual and historically evolving methods he advocates, as well as the Popperian "rules of thumb". [16] Therefore, there is no comprehensive "Methodology" or Theory of Science that gathers them together and makes them available to researchers, ready to be applied like instructions on how to use a microwave oven. Just as the meaning of concepts and ideas arises from examples (or their use, as argued by Wittgenstein [17]), similarly, it is the examples and the concrete practice that provide the majority of the method's principles, which are immanent to it and are learnt automatically by living within a tradition. Consequently, they possess an inevitable "existential" component: [18] "to do scientific work one has to immerse oneself into the relevant research situation". [19]

In short, although Feyerabend acknowledges that there are rules governing science followed by "real" scientists, these rules cannot be "codified". It is impossible to construct a comprehensive "theory of science" or "method" based on them. What guides the scientist is a "practical logic", capable of producing results, [20] but one which cannot be understood by the scientists themselves who want to pontificate on the correct method. It is even more obscure to philosophers of science,

[15] See, in this regard, one of the founding fathers of the Vienna Circle, Moritz Schlick, **General Theory of Knowledge**, Springer-Verlag, New York — Vienna 1974 (First edition 1925), pp. 326–327. At the same time, the idea that it is possible to apply in any field, even that of the humanities, the same method that is used in the natural sciences was one of the most deeply rooted ideas of the RV, including Popper.

[16] See Feyerabend, **Farewell to Reason**..., pp. 38, 281–283; Feyerabend, **Conquest**..., p. 265; Parascandalo & Hösle (eds.), "Three Interviews ...", p. 119.

[17] See Paul K. Feyerabend, **Problems of Empiricism: Philosophical Papers**, Vol. 2, Cambridge, pp. vii–viii.

[18] See Paul K. Feyerabend, "Rationalism, relativism and scientific method" (1977), in: Paul K. Feyerabend (ed.), **Knowledge, Science and Relativism. Philosophical Papers,** Vol. 3, Cambridge U.P., New York 1985, pp. 210–211 [200–211].

[19] Feyerabend, "Concluding...", p. 503.

[20] Feyerabend, **Erkenntnis für freie Menschen...**, p. 242.

whom Feyerabend describes as "timid academic rodents" [21] practicing a "disreputable profession" [22] and populating modern universities, "who sing airs about rationality and have no idea of concrete problems". [23] It is, instead, a distinct sensitivity, akin to a flair or "tact" that can be cultivated in the concreteness of practice:

> the knowledge we need to understand and to advance the sciences does not come from theories, it comes from participation. The examples, accordingly, are not details that can and should be omitted once the "real account" is given — they are the real account.[24]

So, "[i]n the case of science the necessary tact can be developed only by direct participation". [25]

Finally, Feyerabend asserts that the initiation of new researchers into the realm of science does not occur through abstract rules governing what one should or should not do to become a "good" scientist. Instead, it transpires through hands-on training. The art of "doing science", known as "know how" or expertise, [26] is acquired solely through practical experience alongside other scientists. Just as Zen wisdom cannot be acquired through precepts but exclusively through

---

[21] Feyerabend, **Erkenntnis für freie Menschen...**, p. 252.

[22] Paul K. Feyerabend, **The Tyranny of Science**, Polity Press, Cambridge — Malden 2011, p. 64. This work is the translation of the first Italian edition, Paul K. Feyerabend, **Ambiguità e armonia: Lezioni trentine**, edited by F. Castellani, Editori Laterza, Rome — Bari 1996. (In the English version, however, all the chapter titles have been changed). It collects the lectures held by Feyerabend at the University of Trento in May 1992, which were recorded and transcribed, and then revised by the author. Therefore, it can be said to represent Feyerabend's final word (with the exception, of course, of the autobiography, on which he worked even on his in deathbed).

[23] Feyerabend, **Erkenntnis für freie Menschen...**, p. 262.

[24] Feyerabend, **Farewell to Reason**..., p. 284.

[25] Feyerabend, "Against Method…", p. 19.

[26] As regards these concepts, and the connection between "tacit knowledge", expertise and "know how", the literature is now conspicuous. For a first and more comprehensive approach I refer to classic works such as those by Harry Collins, **Tacit and Explicit Knowledge**, The University of Chicago Press, Chicago — London 2010; Harry Collins and Robert Evans, **Rethinking Expertise**, University of Chicago Press, Chicago — London 2007; Jason Stanley, **Know How**, Oxford University Press, Oxford 2011. I have provided some insight into these issues, in relation to the so-called "knowledge society" and the importance of these concepts in contemporary economy, in Francesco Coniglione, "Science and the Knowledge Society in Europe", *Nauka* 2015, Vol. 2, pp. 7–23; Francesco Coniglione, "Quale conoscenza per la »Società della conoscenza«?", *Bollettino della Società Filosofica Italiana* 2015, Vol. 216, September-December, pp. 3–24.

firsthand encounters with concrete and paradoxical scenarios under the guidance of a master, [27] Feyerabend's viewpoint highlights his awareness of the "tacit dimension" — a concept inherent in Kuhn's paradigm and explicitly explored by other authors whom he was studying contemporaneously, such as the aforementioned Michael Polanyi. [28] This aspect is illustrated by Feyerabend in relation to experimental situations, providing an illuminating example:

> Every experimenter dealing with an instrument has a lot of what Polanyi calls "tacit knowledge", like a racing car driver: he could not tell you in detail all the things he knows; he can show you by driving in certain extreme situations. The same happens with scientists. [29]

Therefore, tacit knowledge is an integral component of that "concreteness" that Feyerabend aims to juxtapose with the caricature of science constructed by the rationalists. It is precisely tacit knowledge that delineates a field of research, [30] transcending any conceivable manual-like definition.

What has been said so far leads me to the conclusion that it is more accurate to describe Feyerabend's position as "methodological mysticism" than "methodological anarchism", as it is commonly labeled. [31] The method is, in fact, something ineffable — neither communicable nor rationalizable, but nonetheless existing, to which one is led by a personal approach, a direct involvement in laboratories and interactions with eminent scientists. In essence, it is akin to a "practical initiation" rather than a theoretical one, resembling the journey of a Zen monk more than that of someone trained through manuals and methodological indoctrination.

---

[27] See Fritjof Capra, **The Tao of Physics: An Exploration of the Parallels between Modern Physics and Eastern Mysticism**, Shambala Publications, Boulder 1975, pp. 121–129.

[28] See Michael Polanyi, **Personal Knowledge: Towards a Post-Critical Philosophy**, Routledge, London 1962; Michael Polanyi, **The Tacit Dimension**, Doubleday & Company, New York 1966.

[29] Parascandalo & Hösle, "Three Interviews…", p. 122. The text says "tested knowledge", but this must surely be a transcription error, as can be inferred from knowledge of Polanyi's work and the example given, which refers precisely to "tacit knowledge". See also Feyerabend, **The Tyranny of Science…**, pp. 106, 123; Feyerabend, "Concluding…", pp. 492, 498, 501.

[30] See Feyerabend, "Miseria dell'epistemologia", *Lettera internazionale internazionale* 1991, Vol. 30, p. 58 [55–60].

[31] See Francesco Coniglione, "La ragione ineffabile di Feyerabend e il destino dell'epistemologia contemporanea", in: AA.VV., **Oltre la crisi della ragione. Itinerari della filosofia contemporanea**, Galatea, Acireale 1991, pp. 71–118.

This represents a dimension of knowledge (or would it perhaps be more appropriate to call it "wisdom"?) that had been marginalized as a result of the rise of the Greek *logos*. It has been partially recognized (we do not know with how much awareness of this more general theoretical framework) in Kuhn's concept of "paradigm", as well as by Ludwik Fleck [32] and in Polanyi's "tacit" or "unspoken knowledge". It has been hypothesized [33] that Feyerabend was influenced by the mysticism found in Pseudo-Dionysius the Areopagite's work on the **Divine Names**, [34] particularly concerning the concept of the ineffability of God, which transcends all possible discourse and, as a result, cannot be adequately encapsulated within concepts and language: "According to Pseudo-Dionysius the Areopagite [...], ultimate reality (God, Being) is ineffable. Trying to grasp it directly we face darkness, silence, nothingness". [35] This thesis undoubtedly possesses true merit, though there is a noteworthy point to consider: the reference to Pseudo-Dionysius emerges much later than the diagnosis of the ineffability of method. While this ineffability is already clearly present in **Against Method**, Pseudo-Dionysius is briefly alluded to in a somewhat insignificant way. [36] His name is more extensively used in Feyerabend's final posthumous work, **Conquest of Abundance**, specifically in two essays contained therein. [37] The incidental nature of Feyerabend's encounter with Pseudo-Dionysius is confirmed by the fact that there is no mention of it in his au-

[32] See Ludwik Fleck, **Entstehung und Entwicklung einer wissenschaftlichen Tatsache. Einführung in die Lehre vom Denkstil und Denkkollektiv**, B. Schwabe & Co. Verlagsbuchhandlung, Basel 1935; Robert S. Cohen, Thomas Schnelle (eds.), **Cognition and Fact — Materials on Ludwik Fleck**, Reidel, Dordrecht 1986.

[33] See Ian J. Kidd, "Feyerabend, Pseudo-Dionysius, and the Ineffability of Reality", *Philosophia* 2012, Vol. 40, No. 2, pp. 365–377, https://doi.org/10.1007/s11406-011-9322-9.

[34] See Pseudo-Dionysius, **The Divine Names**, in: Pseudo-Dionysius, **The Complete Works**, trans. Colm Luibheid, Paulist Presse, New York — Mahwah 1987, pp. 47–131.

[35] Paul K. Feyerabend, "Art as a Product of Nature as a Work of Art", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, pp. 233 [223–241].

[36] See Feyerabend, **Against Method**..., pp. 248, 272.

[37] Paul K. Feyerabend, "Realism", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, pp. 195–196 [178–196], Paul K. Feyerabend What Reality?", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, p. 214 [206–216]. A positive mention of Pseudo-Dionysius and Meister Eckhart is also included in Feyerabend, "Concluding...", p. 516.

tobiography or in his unfinished work, **Naturphilosophie**. [38] In fact, it seems that Feyerabend had an indirect acquaintance with Pseudo-Dionysius through a work by Erwin Panofsky. [39]

My opinion is therefore that the direct influence of Pseudo-Dionysius primarily served to strengthen and substantiate positions that Feyerabend had developed earlier in his intellectual journey by giving them metaphysical support. The distinctive form of methodological mysticism that Feyerabend developed had already taken shape through personal inner reflection and his familiarity with the works of Kuhn and Polanyi. It was further enriched by the extensive range of readings he had delved into, covering philosophy, religion, and mythology.

As his thought matured, Feyerabend eventually adopted an attitude toward science similar to that of the mystic toward the totality of the world. The key distinction lies in the nature of Feyerabend's mysticism, which is predominantly immanent and doesn't overtly propose a transcendent reality or a God forever beyond reach, as in the case of Pseudo-Dionysius. On this matter, Feyerabend's thoughts appear to have been somewhat indefinite, and it seems that only in the final stage of his life did he pose the problem of theism or the existence of God. In fact, when directly questioned about his belief in God he responded in an interlocutory manner:

> I don't know. But I'm certainly not an atheist or a conceited agnostic; it takes a whole lifetime to find out these matters. I have a feeling that some kind of supreme bastard is around there somewhere. I'm working on it. [40]

However, he seems to have labeled himself a Catholic to his closest friends, including Paul Hoyningen-Huene and Gonzalo Munévar. [41] This stance was

[38] Paul K. Feyerabend, **Naturphilosophie**, Suhrkamp Verlag, Frankfurt am Main 2009 (English trans. **Philosophy of Nature**, Polity Press, Cambridge — Malden 2016). This work was written in the 1970s while he was working on **Against Method** and appears to have been conceived as its companion. Its writing was later abandoned, and its manuscript seemed to have been forgotten by Feyerabend himself. For further information on this see Helmut Heit and Eric Oberheim, "An Introduction", in: Paul K. Feyerabend, **Philosophy of Nature**, Polity Press, Cambridge — Malden 2016, pp. Vii-xxvii.

[39] Kidd, "Feyerabend, Pseudo-Dionysius…", p. 366.

[40] Feyerabend, **The Tyranny of Science**…, pp. 26–27.

[41] This is attested by Eric C. Martin, "Late Feyerabend on Materialism, Mysticism, and Religion", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 134 [129–136], https://doi.org/

markedly different from the position he had embraced in his youth, during which he expressed a different view: "[w]hen somebody was talking about God or beauty or truth, I usually got up and told him he did not know anything about science, that all he said was nonsense. I also admired the positivism of the Vienna Circle". [42]

It is important to note, however, that Feyerabend does not deny the existence of science as a "reality", just as he does not reject the idea of an independent reality outside of the human mind. [43] In fact, he only denies that it is possible to have adequate knowledge of the method of science, which he claims is not fully accessible through rational means. Similarly, in mysticism reality — whether it be the Absolute, God, Nothingness, or any other superordinary entity — can be grasped through such experiences as *nirvana*, *satori* or mystical illumination that can be found in many Eastern and Western religious doctrines. This reality undeniably *exists*, and individuals can directly experience it. It is attainable by progressing through increasingly higher levels of perfection, a journey undertaken by both great mystics and, according to Feyerabend, exceptional scientists like Einstein, Galileo, and Boltzmann, who perform a role akin to the gurus of Eastern mysticism. Still, despite its accessibility, it remains indescribable. Feyerabend makes a clear distinction between

> [T]he practice of science, which is complicated, not entirely transparent — but seems to get results and philosophical ideas about it which may be right, which may be wrong, but which have no influence whatsoever on that practice. [44]

Science is, for Feyerabend, a text that discloses its meaning only to those concretely engaged in it, rather than to those who engage in philosophical discussions about it. This mirrors the nature of sacred texts in Zen practice:

> They have the peculiarity of disclosing their life-giving meaning only to those who have shown themselves worthy of the crucial experiences and who can therefore extract from these texts confirmation of what they themselves already possess and are, independently of them. To the inexperienced, on the other hand, they remain not only

10.1016/j.shpsa.2015.11.017.

[42] Parascandalo & Hösle (eds.), "Three Interviews...", p. 116.

[43] See Kidd, "Feyerabend, Pseudo-Dionysius...", p. 369.

[44] Feyerabend, "Concluding...", p. 491.

> dumb — could he ever be in a position to read between the lines? — but will infallibly lead him into the most hopeless spiritual confusion, even if he approaches them with wariness and selfless devotion. Like all mysticism, Zen can only be understood by one who is himself a mystic and is therefore not tempted to gain by underhand methods what the mystical experience withholds from him. [45]

In essence, science can only be understood by those who are active scientists themselves. But this understanding is primarily *tacit*, more an effective exercise based on examples than the ability to articulate a rational discussion. Any rational discourse on science is, in fact, impracticable, much like trying to verbally articulate the experience of mystical enlightenment. It is only acceptable to actively engage in science, much like practicing yoga, rather than constructing rational theories about its structure and development. [46] In sum, there exists an insurmountable divide between the language of the methodologist and the practice of the scientist, similar to the gap observed in Zen or Pseudo-Dionysius and other mystics from both Eastern and Western traditions. It is noteworthy that this same critique of the scientific method is found within Zen, echoing Feyerabend's perspective:

> The main distinguishing feature of science's attitude towards reality is to describe an object, talk about it, walk around it, record everything that excites our senses and our minds, abstract it from the object itself and, when it feels it has finished, synthesize these analytically formed abstractions in order to consider the result as the object itself. [47]

In Feyerabend's specific type of immanent mysticism, the "ineffable" is not associated with a reality separate from the world as it is in certain (though not all) types of mysticism, but rather with the concrete practice of scientists and the methods they actually use. It is the latter that are ineffable, and any theorization that seeks to grasp this discursively ends up perverting and deforming it, leaving us with only a pale phantom.

Feyerabend does not merely signify the culmination of an epistemological tra-

[45] Eugen Herrigel, **Zen in the Art of Archery**, first edition 1953, Vintage Books, New York 1971, p. 24.

[46] See Feyerabend, "Concluding…", p. 503.

[47] Daisetz T. Suzuki, "Über Zen-Buddhismus", in: Erich Fromm, Daisetz Teitaro Suzuki, and Richard De Martino (eds.), **Zen-Buddhismus und Psychoanalyse**, first edition 1960, Suhrkamp, Frankfurt am Main 1971, p. 21 [9–100].

dition founded on specific ontological and methodological assumptions: he also embodies a broader disposition of the human spirit, which finds its purest expression in the manifestations of Eastern and Western mysticism, and its "impure" and philosophically tainted forms in various classics of Western thought (Bergson, Husserl and so on). However, Feyerabend's journey, which in other thinkers unfolded along more internally philosophical paths, has followed the original route of contemporary epistemology: he first delegitimizes this field by exposing its inherent contradictions and then emphasizes an aspect of it — the unspoken and indisputable dimension — whose delegitimization has always been considered by scientific rationalism and the founding fathers of modern epistemology a prerequisite for a scientific approach to reality.

2. Cognitive Mysticism

The questioning of a general theory of science gradually led Feyerabend to realize that it is science itself, understood as the capacity to grasp and describe the structures of reality — in other words as its "knowledge" in the most profound sense of the term — that falls short in relation to its self-assigned task.

A good vantage point for understanding Feyerabend's critique of the very possibility of science is the distinction he draws between "abstract (or theoretical) traditions" and "historical (or empirical) traditions". [48] By the former Feyerabend refers to "those traditions in which the logical aspect stands out"; by the latter he denotes traditions with local laws that often allow for exceptions and are influenced by random elements. [49] To the question of how the distinction between them, i.e. between "pure" reason and "irrational" material that needs to be processed and "rationalized" (material that could be identified as "praxis") originated, Feyerabend answers that "reason" is not a natural phenomenon, but rather a tradition that has imposed itself on other traditions, ultimately assuming a hegemonic function. In particular, reason and praxis are not two fundamentally different realities, but two different types of tradition.

[48] See Feyerabend, **Farewell to Reason**..., pp. 118–119, 166 and *passim*.

[49] Feyerabend, **Erkenntnis für freie Menschen**..., p. 49.

> Traditions of the first type exhibit clear and easily reproducible formal aspects, which sometimes lead us to overlook the intricate and poorly understood processes that ensure this simplicity and reproducibility. In contrast, traditions of the second type are much more complex, both on the surface and in depth; their formal features are covered with all kinds of casual cloths, so much so that they seem not to exist. [50]

Now, while the historical traditions, which rationalists often oppose, have concepts that are well suited to the circumstances of everyday life, on the contrary

> [a]bstract traditions, on the other hand, have no such concepts. They may enhance the situation in specific, limited fields, like mathematics and astronomy (and even here only after much difficulty), but in politics, art, ethics, religion, and the doctrine of the soul, they only create confusion. [51]

An effort to bridge the gap between appearance and reality and, in a way, to give reason to the realm of experience by recovering typical instances of the archaic tradition — aiming to "reconcile the abstract schemes of Parmenides (and Plato) with the richness of everyday experience" [52] — was pursued by Aristotle's scientific approach grounded in common sense. The underlying cosmology that underpins his scientific perspective is rooted in a fundamental harmony between man and the cosmos, a harmony that is disturbed only in specific instances, without globally undermining perceptual knowledge.

The consequence of the victory of the abstract tradition over the historical ones has been that just as any methodology distorts the actual process of science and cannot fully capture its effective mode of inquiry, so any scientific theory is a distortion or a deceptive alteration of the empirical material it seeks to explain; just as the theory of science is a caricature of science, science itself (and especially its queen, physics, and in any case the science that has developed since Galileo) is now considered a caricature of reality. The methodological anarchism expressed in "anything goes" thus presupposes "scientific anarchism", and while the former had its outcome in methodological mysticism, the latter leads to the mystical contemplation of reality. It is important to note that these two facets of inquiry (com-

[50] Feyerabend, **Erkenntnis für freie Menschen** ..., p. 48.

[51] Feyerabend, **Erkenntnis für freie Menschen**..., p. 223.

[52] Paul K. Feyerabend, "Nachtrag 1977" to "Realismus und Instrumentalismus: Bemerkungen zur Logik der Unterstützung durch Tatsachen", in: Paul K. Feyerabend (ed.), **Der wissenschaftstheoretische Realismus und die Autorität der Wissenschaften**, Friedr. Vieweg & Sohn, Braunschweig — Wiesbaden 1978, p. 109 [339–350].

paring methodologies and science, and comparing science with reality) are frequently interwoven in Feyerabend's works: established inadequacies in methodology serve to highlight deficiencies in science and vice versa. However, for the sake of conceptual clarity we will address them separately here.

In order to show the inadequacy of methodologies Feyerabend employed a rich array of materials available from the history of the sciences (which we have refrained from citing here for the sake of brevity). In this second undertaking pursued in his thought he performs a comparison between scientific theory and reality, using as a benchmark alternative worldviews and cognitive approaches to reality — ones usually considered unscientific or pre-scientific by the Western scientific tradition. The result of this approach is a re-evaluation of alternative sciences, "knowledge" and cultures, which often prove better suited to the complexity of human experience.

Of course, Feyerabend does not want to deny the fact that science "works": it cannot be denied that it enables accurate predictions and serves as the basis for a multitude of practical applications, a point often emphasized by its defenders when countering criticism. [53] In other words, Feyerabend is not claiming that science does not allow the human mind to "get at reality": the need for an open-minded approach to cultural traditions and cognitive practices does not imply a discrediting or delegitimizing of Western science, as if it were regressing to pre-Enlightenment superstitions. Rather he is always careful to emphasize that such knowledge is, *first of all*, always "local": both because it is specific to a tradition (in that science has an "exceptional role in the West as being best adapted to the situation here" [54]) and because it involves limited parts of space-time and, moreover, is very deformed and simplified; scientific laws are, in fact, "abstractions" and "idealizations" that have little to do with reality. *Secondly*, Feyerabend emphasizes that "there are other ways of living in this world", [55] so it is completely wrong to believe that only "scientific objects" are real, while those belonging to other cultures are mere illusions: one cannot make the "success of science a measure of the reality of its ingredients", [56] adopting a form of "theoretical monism" or "scientific

[53] Feyerabend, **The Tyranny of Science**..., pp. 35–36.

[54] Feyerabend, "Concluding...", p. 516.

[55] Feyerabend, "Concluding...", p. 516.

[56] Feyerabend, **Farewell to Reason**..., p. 125.

imperialism": "[...] the idea that there can be only one science — one physics, one biology, one chemistry [...] is again but a result of insufficient analysis". [57] Feyerabend thus seeks to evaluate the epistemic value of the different modes of inquiry that have emerged from different cultures throughout history. [58]

For example, his defense of astrology does not stem from a particular love for the discipline, but rather serves as an illustration of "the way in which scientists treat phenomena that fall outside their sphere of competence: they do not study them, but simply swear at them, insinuating that their imprecations are based on strong arguments and are purposeful", [59] which is the true essence of fanaticism. [60] Furthermore, this defense of alternative traditions to science, such as alternative medicine, is part of the usual strategy of giving the various approaches time to prove their worth, rather than dismissing them on the basis of prevailing biases: [61] "one should not deny the factual content to a point of view that seems to fall under the section myth-invention-religion-fables", [62] and "is it not the case that the revival of such traditions has on occasions shown their superiority in domains in which science makes definite claims (acupuncture, Taoism as a philosophy of science and a social philosophy, etc. etc.)?" [63]

Feyerabend's considerations regarding the alternative cognitive traditions have a dual aspect. On the one hand, he claims that these traditions can lead to

[57] Feyerabend, "On the Critique of Scientific Reason", in: Colin Howson (ed.), **Method and Appraisal in the Physical Sciences: The Critical Background to Modern Science, 1800–1905**, Cambridge University Press, Cambridge 1976, pp. 309–339.

[58] Ian J. Kidd, "Feyerabend on the Ineffability of Ultimate Reality", in: Jeanine Diller and Asa Kasher (eds.), **Models of God and Alternative Ultimate Realities**, Springer, Dordrecht — Heilderberg — New York — London 2013, pp. 849–850 [849–859].

[59] Paul K. Feyerabend, **Dialogo sul metodo**, transl. by R. Corvi, Laterza, Rome — Bari 1993, p. 41. Although the Italian translation states that this dialogue reproduces the "Dialogue on Method", that can be found in: Gerard Radnitzky, Gunnar Andersson (eds.), **The Structure and Development of Science**, Reidel, Dordrecht 1979, pp. 63–131, it is, however, very different from the English original, containing additions and expansions; hence I prefer to quote from the Italian version.

[60] Feyerabend, **Dialogo sul metodo**..., p. 42.

[61] Feyerabend, **Dialogo sul metodo**..., pp. 21–32, 38–42.

[62] Feyerabend, **Dialogo sul metodo**..., p. 75; see also Feyerabend, **Farewell to Reason**..., p, 33.

[63] Paul K. Feyerabend, "Life at the LSE?", *Erkenntnis* 1978, Vol. 13, No. 2, p. 298 [297–304] (published under the pseudonym "Fantomas" and later included in **Science in a Free Society**..., pp. 210–217).

forms of knowledge comparable to that of science, contributing to its development and valuable from a "scientific" point of view; in essence, the alternative traditions can prove to be a forerunner of new advances in science, expanding its field of application much as many scientists and philosophers had already emphasized, first of all Popper. On the other hand, Feyerabend notes how science has been judged by rationalists on the basis of the adequacy of the results it achieves with respect to the objectives it has set for itself — namely, the increase in cognitive content and the possibility of manipulating nature. But this kind of evaluation cannot always be extended to other cognitive practices, like the wisdom of witches and wizards, who set themselves other goals, different from those of science. In fact, the presumed excellence of science in all contexts and throughout history remains unproven, accepted only on the basis of the *opinio communis* of the majority of scientists (their "basic scientific wisdom"). Philosophers of science (even the more "sophisticated" ones, such as Lakatos and his followers [64]) merely strive to rationally reconstruct this practice and then assert, in an imperialistic manner, that it must be universally applied to all other areas of human activity, on the assumption that they have the same goals as science.

The liability of this approach is, I think, valuable in so far as it warns against the danger of conflating and overlapping different forms of local knowledge produced throughout history. We cannot replace scientific methods and their capacity to address specific phenomena with astrological practices, which have their own domains and types of relevance. In the same way, we cannot expect to solve a mechanical problem by applying the formulas of electromagnetism, and we cannot defend ourselves against the bullets of colonial armies with tribal totem-pole dances. An example that highlights the need to separate different domains of reality and address them with distinct, non-interchangeable methods can be found in the case of Galileo. He employed astrological methods to elaborate genethliacs, [65] but was meticulous in distinguishing between different fields: he rejected the idea that planetary influences affected the motion of physical bodies, which he believed should strictly adhere to mechanical causes (as exemplified in the fourth day of the **Dialogue on the Two Chief World Systems**). At the same time, he recognized the planets' influence on the individual's character (*mores*) and mind (*in-*

[64] Feyerabend, "On the Critique of Scientific Reason…", p. 319.

[65] See Darrell H. Rutkin, "Galileo Astrologer: Astrology and Mathematical Practice in the Late-Sixteenth and Early-Seventeenth Centuries", *Galiæana* 2005, II, 2005, pp. 107–144.

genium). In essence, astrology should confine itself to its own realm, the spiritual domain, a view supported by Galileo, who was a believer himself, and it should refrain from encroaching upon the territory of natural science, which is concerned with the mechanical interactions of physical bodies, understood through mathematics. This separation is analogous to distinguishing between iron bullets and magical rituals.

The interpretation of Aristotle and the critique of abstract traditions embodied in modern science is noteworthy because it sheds further light on Feyerabend's mysticism, which we have so far associated with his views on methodology. In fact, for the mystic as well, knowledge possesses an entirely empirical nature, meaning it is not filtered through abstract conceptual frameworks. The quieting of the rational mind leads to a state of awareness in which the environment is perceived directly, without the interference of thought. In this state, the mind "also takes in all the sounds, sights, and other impressions of the surrounding environment, but it does not hold the sensory images to be analyzed or interpreted". [66] Essentially, this aligns with the paratactic approach to experience that Feyerabend advocates, contrasting it with the formal thinking of modern science. [67] The latter replaces the world of perceptions with an artificial world, leading to the disarticulation of the various spheres of human experience, hindering the emergence of a complete human being and giving rise to a new groups of specialized individuals, such as theologians, intellectuals, artists, scientists, each of whom has "developed fragments of their being to a high degree of perfection". [68]

Now Feyerabend expands his mysticism by not only claiming the pluralism and ineffability of method, but also by underscoring the existence of a multitude of cognitive approaches to a reality that is, as a whole, ineffable. As he puts it, "[s]cience speaks in multiple voices, and is comprised by a changing constellation of theories, practices, and institutions [...]". [69] Despite being a realist (in the sense of not considering reality an objectification of the mind or ego), he argues that

[66] Capra, **The Tao of Physics**..., p. 40.

[67] See Feyerabend, **Conquest of Abundance**..., pp. 21–35; Feyerabend, **The Tyranny of Science**..., pp. 84–85.

[68] Paul K. Feyerabend, "In Defence of Aristotle", in: Gerard Radnitzky, Gunnar Andersson (eds.), **Progress and Rationality in Science**, Reidel, Dordrecht 1978, pp. 70 [143–180].

[69] Martin, "Late Feyerabend...", p. 131.

there is no singular path by which to approach reality because nature responds in many ways to our inquiries, including the non-scientific approaches found in various cultures and traditions. This concept aligns with the idea that, as in Pseudo-Dionysius' conception of the divine names, God manifests Himself with a plurality of attributes and in various ways, all of which are intelligible to humanity. However, He remains "forever shrouded in darkness" [70] because no single attribute can encompass the entirety of His being. [71] This is also true for the physical world: "[w]e never have an overall view of reality, not even approximately for this would mean that we have gone through all possible trials, i.e. that we know the history of the world before the world has come to an end." [72]

In this manner, Feyerabend's mysticism — as well as that of numerous other mystics, including Pseudo-Dionysius himself — does not lead to cognitive nihilism or absolute silence. Instead, it encourages the proliferation of endeavors, the promotion of methodological pluralism, and the coexistence of diverse worldviews. None of these perspectives, on its own, provides an exhaustive understanding, but each captures some facet or small fragment of reality. While comprehending the entirety of Being (God, Reality) remains an insurmountable challenge, in that "[t]he being as it is, regardless of any kind of approach, can never be known [...]", [73] it is nevertheless always possible to attain local, partial knowledge of it:

> Ultimate Reality, if such an entity can be postulated, is ineffable. What we do know are the various forms of *manifest reality*, i.e., the complex ways in which Ultimate Reality acts in the domain (the "ontological niche") of human life. Many scientists identify the particular manifest reality they have developed with Ultimate Reality. This is simply a mistake. [74]

In this context, we witness the profound essence of the pluralism advocated by Feyerabend: it does not function as a heuristic strategy aimed at a potential and easier convergence towards the True; it is not merely a catalyst for creativity to discover theories that asymptotically approach the Truth or progressively ap-

[70] Feyerabend, "What Reality?...", p. 213.

[71] Feyerabend, "Realism...", pp. 195–196.

[72] Feyerabend, "Concluding...", p. 516.

[73] Parascandalo & Hösle (eds.), "Three Interviews...", p. 205.

[74] Feyerabend, "What Reality?...", p. 214 [emphasis in the original].

proximate it in a Popperian manner. Instead, it highlights the unattainability of such an ideal and the limitation of all human cognitive efforts by an insurmountable partiality:

> 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 alternatives*, each single theory, each fairytale, 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. [75]

Therefore, we can only grasp a fragment of the Truth, as the complete Truth can only be unveiled through a mystical vision of reality. This vision can only be attained when we learn to relinquish our cognitive endeavors and acknowledge their inherent partiality and paradoxical nature. In the end, Feyerabend's continuous shifting of positions, his chameleon-like nature akin to Woody Allen's Zelig, [76] appears to serve the purpose of dismantling dogmatic structures and revealing the potential for alternative perspectives, akin to the *kōan* of Zen. [77] This, in turn, opens the mind to a vision of method and reality that only intuitive knowledge can provide.

In the end, science is only one of the many possible ways to approach and understand the world, existing alongside countless other modes of engagement with it that have been preserved in the various traditions and diverse cultures that have shaped human history. The arrogance of scientists, much like that of any particular culture, lies in the belief that there is only one path to grasp reality — their own — and that it is both correct and universally applicable in the realm of knowledge and in ensuring "progress" and human happiness. This is the central flaw in Platonism: the inclination to absolutize a particular perspective as the only

[75] Feyerabend, **Against Method**..., p. 21; see also Feyerabend, "Concluding...", p. 514. Oberheim (**Feyerabend's Philosophy**..., p. 83) shows that "the pluralist conception of scientific knowledge Feyerabend made famous in **Against Method** is basically the same pluralist conception of knowledge from his pre-1970s publications. [...] The real difference between Feyerabend's pre- and post-1970s views is that he drastically increased his rhetoric and tried to base his points more firmly on historical considerations, and less so on abstract methodological considerations" [emphasis in the original].

[76] See Oberheim, **Feyerabend's Philosophy...**, p. 24.

[77] See Suzuki, **The Zen Kōan as a Means of Attaining Enlightenment**, Charles E. Tuttle Co., Boston — Rutland — Vermont — Tokyo 1994, p. 85.

valid one. Moreover, it is where Feyerabend and Pseudo-Dionysius find their deepest resonance:

> Feyerabend clearly concurred with Denys' pluralistic emphasis upon the receptivity of Being to multiple, mutually-incompatible "approaches" (or epistemic activities). This chimed well with his longstanding commitment to pluralism. [...] Such epistemic pluralism also indicates the "manifold" and "abundant" character of Being, especially considering its amenability to a multitude of mutually-incompatible "approaches" [...]. The point that Feyerabend emphasises is that any given epistemic activity can only disclose or provide knowledge of certain aspects of the world. Therefore one must employ a plurality of epistemic activities to maximise our epistemic engagement with the world. [78]

Mysticism (in its dual sense of the ineffability both of reality as a whole and of method) and cognitive/methodological pluralism are thus closely intertwined in Feyerabend's thought: "Reality should be construed as »ineffable«, insofar as it is understood to be amenable to representation by multiple concepts or theories". [79] Furthermore, the awareness of the infinite "abundance" of reality, with the consequent impossibility of capturing it within a single theory — the mythical "theory of everything" [80] — and hence its ineffability, aligns with the idea of broadening and expanding the typologies of our cognitive engagements, without being limited to a single type. [81] This is a "perspectivist" view that rejects the claim to find a harmony in the multiplicity of reality, a coherent and unitary description in which everything fits together perfectly, [82] and is somewhat akin to the Hindu *darśana* conception, [83] as well as to Taoism or, in the Western sphere, to Nietzsche's concep-

[78] Kidd, "Feyerabend, Pseudo-Dionysius...", pp. 370–371.

[79] Kidd, "Feyerabend, Pseudo-Dionysius...", p. 375.

[80] See Feyerabend, "Historical Comments on Realism", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago — London 1999, p. 204 [197–205]; John Preston, "Introduction to Volume 3", in: Paul K. Feyerabend, **Knowledge, Science and Relativism. Philosophical Papers**, Vol. 3, Cambridge University Press, New York 1999, p. 5 [1-15].

[81] See Feyerabend, "Concluding...", p. 514; also Ian Hacking, "Screw You, I'm Going Home", *London Review of Books* 2000, Vol. 22, No. 12; Daniel D. Hutto, "Review of Paul Feyerabend **Conquest of Abundance**", *Philosophical Investigations* 2002, Vol. 25, No. 4, pp. 365–370; Kidd, "Feyerabend on the Ineffability...", p. 855.

[82] See Feyerabend, **The Tyranny of Science**..., pp. 9–10.

tion of knowledge — and, more recently, even Cassirer's insights regarding quantum mechanics. [84]

This inclination towards mysticism is explicitly acknowledged by Feyerabend in a letter he sent to Isaac Ben-Israel in 1990, in which he emphasizes his general metaphysical conception of reality, which developed in his later years, underscoring its connection to mysticism and pluralism:

> My argument is a metaphysical argument: reality (or Being) has no well-defined structure but reacts in different ways to different approaches. Being approached over decades, by experiment of ever increasing complexity, it produces elementary particles; being approached in a more "spiritual" way, it produces gods. Some approaches lead to nothing and collapse. So I would say that different societies and different epistemologies may uncover different sides of the world, provided Being (which has more sides than one) reacts appropriately. I know, all this sound quite mystical but I think it can be worked out to sound more plausible. [85]

This acceptance of mysticism within the context of pluralism is even more evident in the "Letter to the Reader" intended for inclusion in **Conquest of Abundance** but not published until 2000, when it was included in an issue of the *London Review of Books*. It is now reproduced in Ian Hacking's "Introduction" to the fourth edition of **Against Method** and is virtually Feyerabend's spiritual testament:

> Reality, or Being, or God, or whatever it is that sustains us cannot be captured that easily. […] Is there a way of identifying what is going on? There are many ways and we are using them all the time, though often believing that they are part of a stable framework which encompasses everything. Is there a name for an attitude or a view like this? Yes, if names are that important I can easily provide one: mysticism, though it is

[83] See René Guénon, **Introduction to the Study of the Hindu Doctrines,** first edition 1921, trans. Marco Pallis, Luzac and Co., London 1945, pp. 230–238; Leonardo V. Arena, **La filosofia indiana**, Newton Compton, Rome 1995, pp. 1–3; Jay Stevenson, **The Complete Idiot's Guide to Eastern Philosophy**, Alpha Books, Indianapolis 2000, p. 79.

[84] See Ernst Cassirer, **Determinism and Indeterminism in Modern Physics**, first edition 1937, Yale University Press, New Haven 1956, pp. 189–192.

[85] Feyerabend, quoted from Isaac Ben-Israel, "Philosophy and Methodology of Military Intelligence: Correspondence with Paul Feyerabend", *Philosophia* 2001, Vol. 28, No. 1–4, 2001, pp. 97–8 [71–101], https://doi.org/10.1007/BF02379770.

> a mysticism that uses examples, arguments, tightly reasoned passages of text, scientific theories and experiments to raise itself into consciousness. [86]

The fact that reality reacts differently to different human cognitive endeavors shows that it still presents resistance, which can vary in intensity, and thus may mark the dissimilar effectiveness of different approaches. [87] While this may result in the decline of certain cognitive practices and cultures, [88] it does not rule out the existence of a multitude of them. These practices and cultures may be more or less suitable for different life contexts and may respond more or less effectively to the various value requirements embraced in a particular lifestyle, since "nature seems to respond positively to many approaches, not only to one". [89] With this kind of ineffability of Being, nothing more can be said except that it presents different levels of resistance; this "surely suggests that certain manifest realities closer resemble »ultimate reality« than others", [90] but it does not exclude the possibility that humans can experience and weave various relationships with reality. This lends support to a substantial realism in Feyerabend's later work, [91] for "[n]ature is not something formless that can be turned into any shape; it resists and, through its resistance, reveals its properties and laws". [92] His realism, however, is not conceived as the ability of science or any of its theories to describe reality by reaching some ultimate level of it, but rather as the simple acknowledgment of the presence of a reality independent of human beings, the full disclosure of which is possible only through a mystical vision. Within science, or any other knowledge that can be articulated in discourse, we can only have multiple possible perspectives on it.

It is in the context of this overarching framework that one can adequately un-

---

[86] Paul K. Feyerabend, **Against Method**, New Edition, introduced by I. Hacking, Verso, London — New York 2010, p. xvi.

[87] Feyerabend, "Historical Comments…", p. 204; Feyerabend, "Concluding…", p. 516.

[88] Feyerabend, "Art as a Product …", in: Feyerabend, **Conquest...**, p. 240 [223–241].

[89] Feyerabend, "What Reality?…", p. 212.

[90] Kidd, "Feyerabend on the Ineffability…", p. 856.

[91] See Luca Tambolo, "Pliability and Resistance — Feyerabendian Insights into Sophisticated Realism", *European Journal for Philosophy of Science* 2014, Vol. 4, No. 2, pp. 197–213, https://doi.org/10.1007/s13194-014-0082-9.

[92] Feyerabend, "Art as a Product of Nature…", p. 238.

derstand what has been most vehemently criticized by Feyerabend's rationalist detractors as his primary flaw: relativism. [93] This charge finds ample support in Feyerabend's works prior to **Farewell to Reason**. However, he later claims to have changed his views, so that by August 1989 he is prepared to state that "[l]ots of things have changed, and my opinions have changed with them". [94] The relativism he now feels comfortable endorsing is closely tied to the acceptance of pluralism, meaning the idea that reality itself encourages multiple approaches, one of which is indeed science. [95] Just as, for rationalists, science justifies its excellence on the basis of the fact of its existence and success, [96] relativism is grounded in the existence of pluralism: "It is an attempt to make sense of the phenomenon of cultural variety". [97] The discovery of the undeniable discord between method and the history of science, and the presence of multiple scientific theories and diversified scientific approaches, imply the necessity of accepting a form of relativism. In this way, the latter is not a thesis to be rationalized or justified (with the inevitable consequence of its self-refutation), [98] but a consequence to be accepted on the basis of an existing fact and the acknowledgment of the diversity of traditions. It is precisely this phenomenon that makes the thesis of epistemic relativism possible. It is therefore not possible to conceive of the existence of ideas and conceptions in terms not linked to specific human cases: there "exist many different ways of living and of building up knowledge" [99] each depending on a particular context or "tradition". Thus, "the idea of a situation-independent objective truth has limited

[93] See Lisa Heller, "Between Relativism and Pluralism: Philosophical and Political relativism in Feyerabend's Late Work", *Studies in History and Philosophy of Science* 2016, Vol. 57, June — Special Issue: Reappraising Feyerabend, pp. 96–105 and 106–113; Martin Kusch, "Relativism in Feyerabend's Later Writings", Studies in History and Philosophy of Science 2016, Vol. 57, June — Special Issue: Reappraising Feyerabend, pp. 106–113, https://doi.org/10.1016/j.shpsa.2015.11.010.

[94] Feyerabend, "Concluding...", p. 507. The date in the text indicates the time when the fictitious dialogue, published two years later, was written.

[95] Feyerabend, "Concluding...", p. 519.

[96] See John H. Zammito, **A Nice Derangement...**, pp. 118–189.

[97] Feyerabend, **Farewell to Reason**..., p. 19. The entire first chapter of this work is devoted to discussing relativism. See also Feyerabend, **Erkenntnis für freie Menschen...**, pp. 54–64, 118–141.

[98] "[I]f all truth is relative to system, scheme, paradigm, form of life, then the status of the relativistic claim itself is problematic" (Robert P. Farrell, **Feyerabend and Scientific Values. Tightrope-Walking Rationality**, Kluwer, Dordrecht 2003, p. 103).

[99] Feyerabend, **Farewell to Reason**..., pp. 74–75; Feyerabend, "Concluding...", p. 516.

validity [...]; it rules in some domains (traditions), but not in others". [100] The typical skeptical conclusion that follows is that "for every statement, theory, point of view believed (to be true) with good reasons, there exist arguments showing a conflicting alternative to be at least as good, or even better". [101] This kind of relativism logically entails the equivalence of traditions belonging to different cultures. They cannot be judged according to the criterion of one being superior to the other, because "[*t*]*raditions are neither good nor bad, they simply are*. [...] [R]ationality is not an arbiter of traditions, it is itself a tradition or an aspect of a tradition. It is therefore neither good nor bad, it simply is". [102] Ultimately, this culminates in "political relativism", advocated particularly in *Science in a Free Society*. It involves criticism of the power of experts, and a demand for democratization and discussion of the various solutions being proposed.

Epistemic relativism is not absolute, however. Later, recognizing the resistance that nature offers to human attempts to intervene in it, Feyerabend argues for a form of relativism where "[there] is more than one way of living and, therefore, more than one type of reality". [103] Whilst it is true that not every way of life succeeds in being effective and receiving positive feedback from nature, there is always a wide variability of perspectives compatible with nature's responses. Thus, "despite first appearances, epistemic relativism in **Conquest of Abundance** is not rejected in favor of a stronger realistic standpoint". [104] This is a kind of relativism that harmonizes with ontological relativism; it entails the rejection of any fundamental distinction, for instance, between the realms of art and science. On this view, "different worlds, such as the world of the Homeric gods or the world of quarks, are equally real because they have been originated and are sustained by the same nature". This follows from the fact that "it is impossible to unravel the mechanism or the laws, and on their basis nature, or Being, or God — or whatever one wants to call the interlocutor of our actions. This interlocutor, fundamentally, is unknown and will always remain so". [105] The existence of a multitude of "points

[100] Feyerabend, **Farewell to Reason**..., p. 73.

[101] Feyerabend, **Farewell to Reason**..., p. 76.

[102] Feyerabend, **Erkenntnis für freie Menschen**..., p. 68.

[103] Feyerabend, "Historical Comments on Realism...", p. 124.

[104] Heller, "Between Relativism...", p. 103.

[105] Feyerabend, "Dialogo con la natura", *Prometeo* 33, 1991, p. 13 [6–13].

of view" is, of course, what emerges from the pluralism of different worldviews — itself motivated by Feyerabend's concept of the "disunity of science" (the impossibility of a single theory). This, however, need not prevent one from considering these different approaches (within epistemic relativism) equally real in the sense of their being capable of grasping aspects of reality that are not merely illusory and that impact and influence human life in its entirety. Nor does it exclude the possibility of a total and complete view of this reality, itself partially reflected in these various epistemic approaches and arrived at through an extraordinary vision accessed via the mystical dimension proposed, though not fully developed, by Feyerabend in his later writings. In this way, the various forms of relativism advocated by Feyerabend come together harmoniously only within the framework offered by the mysticism of his final works.

3. Beyond Mere Reason, Toward a Non-Unidimensional Man

The criticisms directed at the Method and science on the grounds of their partiality and abstraction led Feyerabend to broaden his perspective to a more comprehensive consideration of man, no longer conceived solely as, according to famous Aristotle's definition, the "rational animal". The expansion of his horizons becomes evident for the first time in "On the Critique of Scientific Reason", [106] where, alongside the traditional question concerning the nature of science and the critique of Lakatos' conceptions, another more radical question is posed: "What is so great about science?" Here, "greatness" refers not only to its cognitive content, but also to other values deemed essential for a meaningful life. In essence, Feyerabend raises the question of whether the emphasis on the preferability and excellence of science is indeed well-founded and can be better justified than other forms of life and alternative cognitive approaches, such as those exemplified by Aristotelian science or Azande conceptions. [107] This newfound interest is particularly evident in his posthumous work **Conquest of Abundance**, [108] where he aims to illustrate

[106] See Feyerabend, "On the Critique of Scientific Reason…", pp. 309–339.

[107] See Feyerabend, "On the Critique of Scientific Reason …", p. 310.

[108] Feyerabend, **Conquest of Abundance...**

> how specialists and common people reduce the abundance that surrounds and confuses them, and the consequences of their actions. It is mainly a study of the role of abstractions — mathematical and physical notions especially — and of the stability and "objectivity" they seem to carry with them. It deals with the ways in which such abstractions arise, are supported by common ways of speaking and living, and change as a result of argumentation and/or practical pressure. In the book I also try to emphasize the essential ambiguity of all concepts, images, and notions that presuppose change. Without ambiguity, no change, ever. [109]

In this way, Feyerabend turned his back, so to speak, on his fellow philosophers of science, also as a result of the criticism and misunderstandings that followed the publication of **Against Method**, thus seeing his fortunes decline among the "philosophers". [110] However, simultaneously, he gained significant success and provided support to various forms of relativism in numerous other domains, [111] especially in the social sciences, archaeology and the emerging field later recognized as "Science and Technology Studies". [112] This new perspective is supported by an interest directed towards other disciplines (such as anthropology, art, politics and history in general). Within this meta-scientific dimension, [113] questions regarding the meaning of man's life, happiness, and the possibility of a free society come into play, and alternative modes of accessing reality outside of science, such as myth, tradition, and so on, are explored with empathy. This becomes possible

[109] Paul K. Feyerabend, **Killing Time: The Autobiography of Paul Feyerabend**, The University of Chicago Press, Chicago — London 1995, p. 179. See also Feyerabend, "Concluding...", p. 515.

[110] See Jamie Shaw, Karim Bschir, "Introduction. Paul Feyerabend's Philosophy in the Twenty-First Century", in: Karim Bschir and Jamie Shaw (eds.), **Interpreting Feyerabend: Critical Essays**, Cambridge University Press, Cambridge 2021, pp. 5 [1–10].

[111] See Preston, **Feyerabend...**, *passim*.

[112] See Ulrike Felt, Rayvon Fouché, Clark A. Miller, Laurel Smith-Doer (eds.), **The Handbook of Science and Technology Studies**, The MIT Press, London 2017; Sergio Sismondo, **An Introduction to Science and Technological Studies**, Blackwell, Oxford 2010.

[113] See Daniel Kuby, "Decision-Based Epistemology: Sketching a Systematic Framework of Feyerabend's Metaphilosophy", *Synthese* 2021, Vol. 199, pp. 3271–3299, https://doi.org/10.1007/s11229-020-02934-3, who highlights Feyerabend's general metaphilosophical approach, where this first and foremost concerns decisions regarding epistemological problems that are basically to be traced back to choices that lie outside of methodology. That is what Feyerabend explicitly states: "[...] the »facts«, »laws«, »principles« of science and, for that matter, of any system of knowledge are the results of practical decisions, or simply of living in a certain way — not of theoretical insight alone" ("Concluding...", p. 508). This is all the more valid when it comes to choosing between science and the other ways in which humanity relates to reality.

by moving from the narrow field of epistemic evaluation of theories (i.e. *from within* science) to a broader view in which different worldviews, including science, can be compared and evaluated not only on the basis of their cognitive performance, but also for their contribution to human happiness. Indeed, human life is not characterized solely by a cognitive relationship, but expresses a complex style involving other values and aspects of human personality, of man as a whole. Man is not only *logos*, but is also composed of feelings and passions, engaging in a dialectic of reasons of the heart and the intellect, as "knowledge without a heart is an empty thing". [114] It is funny that Pascal is unjustly overlooked in this regard.

The undeniable "achievements" of science are not necessarily capable of giving sense to human life; the "wonderful products" that technology offers us are not the ultimate goal in which Feyerabend is now interested. Instead, the focus is on questions such as the following: Will this lead to greater happiness? Do these advances contribute to a better humanity? What is preferable? Which way of life should we choose — the one that Aristotle's conception of science presupposes or the one that modern science leads us to? Similarly, in the journey of spiritual elevation through yoga, the practitioner attains many abilities (levitation, the ability to move objects and other "magical" phenomena). While these are remarkable discoveries, they are merely signs of the progress being made, indications that one is on the right path, not the ultimate goal to be reached. [115] Both in Feyerabend and in yogic spirituality the methodological and pragmatic aspects of (scientific and yogic) techniques are subordinated to an axiological perspective. [116] When this is dominant, Feyerabend recognizes that

> [m]any traditions and cultures, some of them wildly "unscientific" (they address divinities, consult oracles, conduct "meaningless" rites to improve mind and body) succeed in the sense that they enable their members to live a moderately rich and fulfilling life. [117]

---

[114] Feyerabend, "Dialogue on Method", in: Gerard Radnitzky and Gunnar Andersson (eds.), **The Structure and Development of Science**, Reidel, Dordrecht 1979, pp. [130] 63–131.

[115] See Mircea Eliade, **Yoga: Immortality and Freedom**, Routledge & Kegan Paul, Carter Lane 1958, pp. 52, 85–90.

[116] See Kuby ("Decision-Based Epistemology…", pp. 3275), who claims that Feyerabend never made a clear distinction between methodology and axiology. However, while this undoubtedly holds true for theorizing of the kind proper to science, it appears to miss the mark once Feyerabend extends his interest to values outside of the narrow domains marked out by the latter.

So, we are faced with a choice: either we accept science with all its shortcomings and the ensuing consequences, or we rely on a mode of experience that is part of alternative traditions that are in alignment with it. And this is not a decision that can be made based on rational standards or by applying a particular methodology (none of which exists); it is a genuine "life choice". [118] In the end, this is precisely what Tolstoy had observed when he argued that we cannot find in science answers to the fundamental questions of human life, such as how we should live and what choices will lead to a peaceful and happy life — unless, of course, we accept as the only values the ever-increasing accessibility of material goods and technological products, just as the prevailing "unique thought" gently advocates.

If we acknowledge that these are indeed the crucial questions, then the appropriate attitude towards science aligns with what the Buddha indicated for his own teaching: it is only a raft that allows us to cross the river and thus lead us to salvation, but which must be thrown away once we have reached it. [119] Similarly, this is in line with what Wittgenstein also says when he wants to summarize the meaning of his **Tractatus**: it is a ladder to reach the vision of the Mystic, and thus we are to regard as nonsensical the propositions uttered to reach it, and then discard them. [120] This means that the "ladder" — the Buddhist teaching, as well as science — is only valuable to the extent that it enables us, and as long as it does, to arrive at the objectives mentioned earlier. They have no value in themselves, they cannot be fetishized as "doctrines" containing wisdom about the world. Instead, they possess instrumental value, serving as a means to other ends (happiness, *nirvana*, ataraxia, etc.). Furthermore, this also implies that there can be more than one "ladder", as Kidd emphasizes with regard to the infinity of God's names in Pseudo-Dionysius. [121] In the Eastern religious tradition this leads to mutual tolerance across and between various techniques for salvation and spiritual elevation, all of

[117] Feyerabend, "Realism...", p. 195.

[118] See Feyerabend, **Farewell to Reason**..., pp. 28–30, 32.

[119] See Majjhima Nikāya, **The Middle Length Discourses of the Buddha: A Translation of the Majjhima Nikaya,** trans. Bhikkhu Nanamoli and Bhikkhu Bodhi, *The Teachings of the Buddha*, Wisdom Publications, Somerville 1995, pp. 228–238 (i135–i144).

[120] See Ludwig Wittgenstein, **Tractatus Logico-Philosophicus**, first edition 1921, trans. David F. Pears and Brian F. McGuinness, with an introduction by Bertrand Russell, Routledge, London — New York 2001, § 6.54.

[121] See Kidd, "Feyerabend, Pseudo-Dionysius...".

which are suitable means for the purpose, with the choice depending solely on the specific inclinations of the practitioner. Similarly, in Feyerabend's mysticism there initially arises a position of methodological tolerance, followed by an aspiration towards the proliferation and acceptance of diverse cultural traditions accompanied by a firm belief in their equal dignity, where this stands in opposition to the hegemonic claims of Western science and culture.

Science and technology can thus be an instrument of human liberation, but only on condition that they are not understood as the sole dimension within which human life is confined and the reason of which they are realizations is not construed as the only aspect that makes man a worthy being — that one-dimensional thinking "in which ideas, aspirations, and objectives that, by their content, transcend the established universe of discourse and action are either repelled or reduced to terms of this universe". [122]

Feyerabend did not hold Marcuse in high regard, and he rejected the juxtaposition of his own thought with the latter's, yet there can be no doubt that he also reflected the influence of the cultural climate underlying youth rebellion and counterculture — something with which Feyerabend had sympathized during his years at Berkeley and of which Marcuse had been the tutelary deity. In addition, Feyerabend's critique of the abstractness of science and the crudeness of the concepts established within abstract traditions was countered by the revival of Hegelian dialectics, which seemed to provide a more comprehensive and expansive conception of rationality than the rationalistic methodological approach and which he surely became acquainted with by reading Marcuse's **Reason and Revolution**. [123]

After abandoning his fascination with Hegelian thinking [124] and limiting science to "local knowledge" as the sphere in which it can fully express its efficacy and productivity, Feyerabend was able to embrace a vision that encompasses the whole range of phenomena and experiences that engage human life in its entirety

[122] Herbert Marcuse, **One-Dimensional Man: Studies in the Ideology of Advanced Industrial Society**, first edition 1964, Routledge, London — New York 2002, p. 14.

[123] See Feyerabend, **Against Method**..., p. 18.

[124] On the meaning and limits of this fascination, see Francesco Coniglione, "Hegel in Feyerabend", in: Annamaria Anselmo and Francesco Crapanzano (eds.), **La presenza di Hegel nei pensatori contemporanei**, Vol. I, Armando Siciliano Editore, Messina — Vittoria 2023, pp. 239–273.

and totality. He was thus gradually led to be interested less in "theories" (scientific or otherwise) and more in comprehensive "worldviews" that express different ways of life and are less amenable to rational arguments. [125] This led him to a careful examination and evaluation of the implications and benefits that science has brought to modern society since its inception.

The critique of what Kidd calls "scientific modernity" [126] now takes on a metascientific character, insofar as it questions the impact of science on people's lives and society as a whole. It highlights the negative effects of science on the cultures of other peoples, its impact on the destruction of the natural environment, its turn toward scientism with a consequent disregard for art and human culture in favor of what is useful in the sense of being economically productive. (Here the influence of the later Wittgenstein's thought is evident, [127] as Feyerabend himself admitted on several occasions. [128]) Moreover, science is the source of the disenchantment of the world and its loss of everything not reducible to brutely manipulable physical naturalness: the destruction of the connection with the totality has led, in Monod's words, to the end of the animistic alliance between man and nature and has produced a "cold universe of solitude". The question then arises as to whether, and to what extent, this destruction in the name of scientific progress "helped humanity (or a privileged part of it), how much damage was done, and what is the balance". [129]

Even so, this critique of "scientific modernity" gains full meaning only in the context of a complex, multifaceted view of reality with its infinite aspects: one which, above all, is not deprived of spaces for imagination, fantasy, and emotion, all of which science has traditionally exorcized because of their perceived threat

[125] See Martin, "Late Feyerabend...", p. 134.

[126] Kidd, "Feyerabend, Science and Scientism", in: Karim Bschir and Jamie Shaw (eds.), **Interpreting Feyerabend: Critical Essays**, Cambridge University Press, Cambridge 2021, pp. 181–184 [172–190].

[127] See Ludwig Wittgenstein, **Culture and Value**, Georg Henrik von Wright (ed.), Blackwell, Oxford 1980.

[128] See Feyerabend, "Concluding...", p. 489. On this see also Ian J. Kidd, "Reawakening to Wonder: Wittgenstein, Feyerabend and Scientism", in: Jonathan Beale and Ian J. Kidd (eds.), **Wittgenstein and Scientism**, Routledge, Abington — New York 2017, pp. 101–115.

[129] Paul K. Feyerabend, "Introduction", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, p. 6.

to its objectivity and pursuit of abstract truth. This vision of reality characterizes the thought of the last Feyerabend: much like the God of the mystics, it responds in various ways to our inquiries, whether they be of an epistemic-cognitive nature or centered on values crucial for spiritual well-being (of a kind not limited to its material aspects) and the overall happiness of human communities. Compared to the "abundance of reality", the concepts developed by abstract traditions, and accordingly by scientific rationalism, are reductive, approximate, unrealistic, coarse, and insensitive to nuances and all those "subtle" aspects that carry significant weight in human life and human experience. This is the shortcoming of materialism, which Feyerabend, after supporting it at the beginning of his intellectual career, came to criticize intensely in his later works, with reference to scientists like Monod or Weinberg. Materialism assumes a monistic and one-dimensional view of reality and consequently excludes those facets that make it interesting and livable for human beings. Hence Feyerabend's radical question: "Are we prepared to view ourselves in the manner suggested by scientists, or do we prefer to make personal contact, friendship, etc., the measure of our nature?" [130] Essentially, the defense of alternative knowledge systems (such as astrology, non-scientific approaches to medicine, etc.) can only be understood when placed within this broader context. It doesn't just refer to physical health, for instance, but encompasses overall well-being within a tradition. It is within this frame of reference that health and illness take on precise and concrete meanings, directly related to the entirety of an individual's life. [131] Philosophy, too, is not exempt from criticism, since throughout its history and right from its inception it has sought to supplant the infinite abundance of life with objective and stable knowledge, often disregarding the diversity and multiplicity of human experience and excluding poetry and art (as with Plato).

Among the elements that Feyerabend considered "important ingredients of a rewarding human life" [132] are poetry, common sense, the world of emotions,

[130] Feyerabend Paul K.,"Has the Scientific View of the World a Special Status, Compared with Other Views?", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, p. 157 [223–241].

[131] See Feyerabend, **Dialogo sul metodo**..., pp. 41–42.

[132] Paul K. Feyerabend, "Concerning an Appeal for Philosophy", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, p. 269 [269–273].

love, and mystery. Mystery, in particular, serves as a reminder that our capacity for understanding falls infinitely short of the "abundance" of reality, which we can never fully fathom. The compulsive pursuit of truth can "make us forget that a life without mystery is barren and that some things, for example our friends, should be loved rather than understood completely". [133] On the other hand, love and emotion provide us with insights into reality, especially the human experience, that go beyond what scientific knowledge can offer. Empathy between human beings gives us access to dimensions of reality that sterile descriptions based solely on rational arguments cannot provide. [134] It is not a matter of rejecting rational discourse, but rather about recognizing its limitations and demonstrating that it cannot always comprehend everything or completely replace an empathetical approach to reality. In particular, this approach helps us to recognize that

> [E]motionally decontaminated "objective" knowledge is only one form of knowledge and by no means the most important. Human relations are created and maintained by empathy which, to please objectivists, might be regarded as a special operation, like the use of a microscope, leading to special insights not available to other operations [...] Arguments do have power — I admit this — but they affect only a small minority and they affect their brains not their heart unless we find ways to combine reason and emotion... [135]

Otherwise, "[t]oo much "rational", i.e., emotionally decontaminated discourse endangers the subtle connections that exist between knowledge, emotion, action, hope, love, and fragments our lives". [136] Feyerabend is particularly intrigued by these "subtle connections" in the final phase of his life. It is no coincidence that the last word in his autobiography is "love", the only legacy he wishes to leave of his ideas.

What has been said so far highlights the centrality of the idea of the inexhaustibility of Being in Feyerabend's thought: something which makes it elude all discourse and logic, but which from time to time takes on forms historically shaped within traditions and among people in their mutual interaction and daily connection with the abundance of a world in which "[t]here are trees, dreams,

[133] Feyerabend, "Dialogue on Method", p. 68.

[134] See Feyerabend, "Concluding...", p. 497.

[135] Feyerabend, "Concluding...", pp. 500, 512.

[136] Feyerabend, "Concluding...", p. 499.

sunrises; there are thunderstorms, shadows, rivers; there are wars, flea bites, love affairs; there are the lives of people, Gods, entire galaxies". [137] All of these events are equally real "in the sense that they occur, are noticed, and have effects", just as the ancient Greeks considered their gods to be "as "real" as dreams and rainbows", without any "grand dichotomy, with a solid, trustworthy, genuine reality on one side and deceiving appearances on the other". [138] The boundary between reality and non-reality cannot be defined simplistically, but is something fluid, dependent on cultures and traditions. "There are many different types of events, and »reality« is best attributed to an event together with a type, not absolutely". [139] Each culture and tradition has its own ontology, consisting of different entities that interact with each other and have an impact on individuals and society. Even dreams, with their supposedly illusory nature, as well as other aspects excluded from the scientific view such as "pain, the feelings of friendship, fear, happiness, and the need for salvation", [140] affect reality. However, this need not preclude distinguishing them from events in the waking state or differentiating their different ways of interacting with the human world. Some cultures even explain this diversity by invoking different levels of reality. [141] The crucial point is not to deny their influence and importance in human life, discrediting their role to the extent that that life is considered all the more rational the less it is influenced by them.

Feyerabend's entire discourse is evidently motivated not only by intellectual considerations but also by humanitarian and ethical concerns. It aims to acknowledge the full dignity of human beings as complete entities, encompassing both *logos* and *pathos*, reason and emotion, as well as imagination. Such a perspective consistently guided his life's work and is also evident in his empathetical and open approach to other cultures: "[...] his aim was to challenge the displacement and destruction of the ways of life of global indigenous peoples by Western scientific and political agencies". [142] This aspect of his thinking has significantly influenced the field of thought known as "postcolonial science and technology studies".

[137] Feyerabend, "Introduction…", p. 3.

[138] Feyerabend, "Introduction…", p. 9.

[139] Feyerabend, "Introduction…", p. 10.

[140] Feyerabend, **Farewell to Reason**..., p. 259.

[141] See Feyerabend, "Introduction…", p. 9.

[142] Kidd, "Feyerabend on the Ineffability…", p. 851.

However, this goal, in my opinion, does not amount to delegitimizing science as a form of knowledge with a well defined scope in respect of its efficacy and application, focusing on a particular type of reality — what Wittgenstein referred to as the "world of facts". Instead, it questions the imperialistic assertion of science's universal validity for every type of reality, to which its method should exclusively be applied.

The fact remains that in his final reflections on these issues Feyerabend remains rather rhapsodic, failing as he does to develop a coherent and well-argued stance capable of engaging with the other positions that were developing concurrently in the culture and philosophy of his time. He could be criticized for failing to engage with feminist and social epistemology, postcolonial theories of science, or thinkers such as Heidegger and Horkheimer-Adorno (and thus with the continental European tradition). His later reflections are more like hints at a path he was about to take but did not have enough time to fully explore, given how long he lived for. Nevertheless, his reflections have opened up a vast terrain of new and intriguing cultural experiences, so that one could say that "many contemporary movements in philosophy of science have been in the direction of this sort of project. If that is so, perhaps much of philosophy of science today is, to the surprise of many, strikingly Feyerabendian". [143]

In conclusion, Feyerabend's mature and late thought includes four basic motifs: (a) the thesis of methodological pluralism (going back to his Lakatosian phase) and the "disunity" of science, dependent on a socio-political context imbued with values, which remains a legacy inherited from contemporary philosophy of science and many schools of thought; (b) the linking of this at a certain point, also, to the thesis of scientific pluralism claiming that there are multiple ways to model and scientifically study reality, with diverse theories that cannot be reduced to a singular framework; (c) its being accompanied by the idea that it is not possible either to fully grasp and articulate the method applied (since it largely results from tacit learning) or to exhaust the abundance of reality (this being the realm of his mysticism, directed both toward method and toward the world); and, finally, (d) the idea that science alone does not encompass human existence, as there are possible forms of life and communities that are more reward-

[143] Kidd, "Feyerabend, Science and Scientism...", p. 190.

ing and make people happier even without it. This is the final outcome of his intellectual journey.

Francesco Coniglione

References

1. Arena Leonardo V., **La filosofia indiana**, Newton Compton, Rome 1995.
2. Ben-Israel Isaac, "Philosophy and Methodology of Military Intelligence: Correspondence with Paul Feyerabend", *Philosophia* 2001, Vol. 28, No. 1–4, 2001, pp. 71–101, https://doi.org/10.1007/BF02379770.
3. Brown Matthew J. and Kidd Ian J., "Reappraising Paul Feyerabend", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 1–8, https://doi.org/10.1016/j.shpsa.2015.11.003.
4. Capra Fritjof, **The Tao of Physics: An Exploration of the Parallels between Modern Physics and Eastern Mysticism**, Shambala Publications, Boulder 1975.
5. Cassirer Ernst, **Determinism and Indeterminism in Modern Physics**, first edition 1937, Yale University Press, New Haven 1956.
6. Cohen Robert S. and Schnelle Thomas (eds.), **Cognition and Fact — Materials on Ludwik Fleck**, Reidel, Dordrecht 1986.
7. Collins Harry, **Tacit and Explicit Knowledge**, The University of Chicago Press, Chicago — London 2010.
8. Collins Harry and Evans Robert, **Rethinking Expertise**, University of Chicago Press, Chicago — London 2007.
9. Coniglione Francesco, "Hegel in Feyerabend", in: Annamaria Anselmo and Francesco Crapanzano (eds.), **La presenza di Hegel nei pensatori contemporanei**, Vol. I, Armando Siciliano Editore, Messina — Vittoria 2023, pp. 239–273.
10. Coniglione Francesco, "La ragione ineffabile di Feyerabend e il destino dell'epistemologia contemporanea", in: AA.VV., **Oltre la crisi della ragione. Itinerari della filosofia contemporanea**, Galatea, Acireale 1991, pp. 71–118.
11. Coniglione Francesco, "Quale conoscenza per la »Società della conoscenza«?", *Bollettino della Società Filosofica Italiana* 2015, Vol. 216, September-December, pp. 3–24.
12. Coniglione Francesco, "Science and the Knowledge Society in Europe", *Nauka* 2015, Vol. 2, pp. 7–23.

13. Diederich Werner, "Obituary on the »Anarchist« Paul Feyerabend", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Springer Science+Business Media B.V., Dordrecht 1991, pp. 213–224.

14. Eliade Mircea, **Yoga: Immortality and Freedom**, Routledge & Kegan Paul, Carter Lane 1958.

15. Farrell Robert P., **Feyerabend and Scientific Values: Tightrope-Walking Rationality**, Kluwer, Dordrecht 2003.

16. Felt Ulrike, Fouché Rayvon, Miller Clark A., and Smith-Doer Laurel (eds.), **The Handbook of Science and Technology Studies**, The MIT Press, London 2017.

17. Feyerabend Paul K., **Against Method**, New Edition, introduced by I. Hacking, Verso, London — New York 2010.

18. Feyerabend Paul K., **Against Method. New Edition**, Verso, London — New York 1993.

19. Feyerabend Paul K., "Against Method: Outline of an Anarchistic Theory of Knowledge", in: Michael Radner and Stephen Winokur (eds.), **Analyses of Theories and Methods of Physics and Psychology,** Minnesota Studies in Philosophy of Science, Vol. 4, University of Minnesota Press, Minneapolis 1970, pp. 17–130.

20. Paul K. Feyerabend, **Ambiguità e armonia: Lezioni trentine**, edited by F. Castellani, Editori Laterza, Rome — Bari 1996.

21. Feyerabend Paul K., "Art as a Product of Nature as a Work of Art", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, pp. 223–241.

22. Feyerabend Paul K., "Concerning an Appeal for Philosophy", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, pp. 269–273.

23. Feyerabend Paul K., "Concluding Unphilosophical Conversation", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Springer Science+Business Media B.V., Dordrecht 1991, pp. 487–527.

24. Feyerabend Paul K., **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago — London 1999.

25. Feyerabend Paul K., "Dialogo con la natura", *Prometeo* 1991, Vol. 33, pp. 6–13.

26. Feyerabend Paul K., **Dialogo sul metodo**, Ital. transl. of "Dialogue on Method…" by R. Corvi, Laterza, Rome — Bari 1993.

27. Feyerabend Paul K., "Dialogue on Method", in: Gerard Radnitzky and Gunnar Andersson (eds.), **The Structure and Development of Science**, Reidel, Dordrecht 1979, pp. 63–131.

28. Feyerabend Paul K., "Die »Rationalität« der Forschung", in: Paul K. Feyerabend, **Der wissenschaftstheoretische Realismus und die Autorität der Wissenschaften**, Friedr. Vieweg & Sohn, Braunschweig — Wiesbaden 1978, pp. 339–350.

29. Feyerabend Paul K., **Erkenntnis für Freie Menschen**, Suhrkamp Verlag, Frankfurt am Main 1979.

30. Feyerabend Paul K.,"Has the Scientific View of the World a Special Status, Compared with Other Views?", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, pp. 223–241.

31. Feyerabend Paul K., **Farewell to Reason**, Verso, London — New York 1987.

32. Feyerabend Paul K., "Historical Comments on Realism", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago — London 1999, pp. 197–205;

33. Feyerabend Paul K., "In Defence of Aristotle", in: Gerard Radnitzky, Gunnar Andersson (eds.), **Progress and Rationality in Science**, Reidel, Dordrecht 1978, pp. 143–180.

34. Feyerabend Paul K., **Killing Time: The Autobiography of Paul Feyerabend**, The University of Chicago Press, Chicago — London 1995.

35. Feyerabend Paul K., "Life at the LSE?", *Erkenntnis* 1978, Vol. 13, No. 2, pp. 297–304.

36. Feyerabend Paul K., "Miseria dell'epistemologia", *Lettera internazionale* 1991, Vol. 30, pp. 55–60.

37. Feyerabend Paul K., "Nachtrag 1977" to "Realismus und Instrumentalismus: Bemerkungen zur Logik der Unterstützung durch Tatsachen", in: in: Paul K. Feyerabend (ed.), **Der wissenschaftstheoretische Realismus und die Autorität der Wissenschaften**, Friedr. Vieweg & Sohn, Braunschweig — Wiesbaden 1978, pp. 339–350.

38. Feyerabend Paul K., **Naturphilosophie**, Suhrkamp Verlag, Frankfurt am Main 2009 (English transl. **Philosophy of Nature**, Polity Press, Cambridge — Malden 2016).

39. Feyerabend Paul K., "On the Critique of Scientific Reason", in: Colin Howson (ed.), **Method and Appraisal in the Physical Sciences: The Critical Background to Modern Science, 1800–1905**, Cambridge University Press, Cambridge 1976, pp. 309–339.

40. Feyerabend Paul K., **Problems of Empiricism: Philosophical Papers**, Vol. 2, Cambridge 1985.

41. Feyerabend Paul K., "Rationalism, Relativism and Scientific Method" (1977), in: Paul Feyerabend, **Knowledge, Science and Relativism. Philosophical Papers,** Vol. 3, Cambridge University Press, New York 1985, pp. 200–211.

42. Feyerabend Paul K., "Realism", in: Paul K. Feyerabend (ed.), **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, pp. 178–196.

43. Feyerabend Paul K., **Science in a Free Society**, New Left Books, London 1978.

44. Feyerabend Paul K., **The Tyranny of Science**, Polity Press, Cambridge — Malden 2011.

45. Feyerabend Paul K., "What Reality?", in: Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus the Richness of Being**, The University of Chicago Press, Chicago and London 1999, pp. 206–216.

46. Fleck Ludwik, **Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv**, B. Schwabe & Co. Verlagsbuchhandlung, Basel 1935.

47. Guénon René, **Introduction to the Study of the Hindu Doctrines,** first edition 1921, trans. Marco Pallis, Luzac and Co., London 1945.

48. Hacking Ian, "Screw You, I'm Going Home", *London Review of Books* 2000, Vol. 22, No. 12.

49. Heit Helmut and Oberheim Eric, "An Introduction", in: Paul K. Feyerabend, **Philosophy of Nature**, Polity Press, Cambridge — Malden 2016.

50. Heller Lisa, "Between Relativism and Pluralism: Philosophical and Political Relativism in Feyerabend's Late Work", *Studies in History and Philosophy of Science* 2016, Vol. 57, June — Special Issue: Reappraising Feyerabend, pp. 96–105.

51. Herrigel Eugen, **Zen in the Art of Archery**, first edition 1953, Vintage Books, New York 1971.

52. Hutto Daniel D., "Review of Paul Feyerabend, *Conquest of Abundance*", *Philosophical Investigations* 2002, Vol. 25, No. 4, pp. 365–370.

53. Kidd Ian J., "Feyerabend on the Ineffability of Ultimate Reality", in: Jeanine Diller and Asa Kasher (eds.), **Models of God and Alternative Ultimate Realities**, Springer, Dordrecht — Heilderberg — New York — London 2013, pp. 849–859.

54. Kidd Ian J., "Feyerabend, Pseudo-Dionysius, and the Ineffability of Reality", *Philosophia* 2012, Vol. 40, No. 2, pp. 365–377, https://doi.org/10.1007/s11406-011-9322-9.

55. Kidd Ian J., "Feyerabend, Science and Scientism", in: Karim Bschir and Jamie Shaw (eds.), **Interpreting Feyerabend: Critical Essays**, Cambridge University Press, Cambridge 2021, pp. 172–190.

56. Kidd Ian J., "Reawakening to Wonder: Wittgenstein, Feyerabend and Scientism", in: Jonathan Beale and Ian J. Kidd (eds.), **Wittgenstein and Scientism**, Routledge, Abington — New York 2017, pp. 101–15.

57. Kuby Daniel, "Decision-Based Epistemology: Sketching a Systematic Framework of Feyerabend's Metaphilosophy", *Synthese* 2021, Vol. 199, pp. 3271–3299, https://doi.org/10.1007/s11229-020-02934-3.

58. Kusch Martin, "Relativism in Feyerabend's Later Writings", *Studies in History and Philosophy of Science* 2016, Vol. 57, June — Special Issue: Reappraising Feyerabend, pp. 106–113, https://doi.org/10.1016/j.shpsa.2015.11.010.

59. Marcuse Herbert, **One-Dimensional Man: Studies in the Ideology of Advanced Industrial Society**, first edition 1964, Routledge, London —New York 2002.

60. Martin Eric C., "Late Feyerabend on Materialism, Mysticism, and Religion", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 129–136, https://doi.org/10.1016/j.shpsa.2015.11.017.

61. Nikāya Majjhima, **The Middle Length Discourses of the Buddha: A Translation of the Majjhima Nikaya,** trans. Bhikkhu Nanamoli and Bhikkhu Bodhi, *The Teachings of the Buddha*, Wisdom Publications, Somerville 1995, pp. 228–238 (i135–i144).

62. Oberheim Eric, **Feyerabend's Philosophy**, *Quellen und Studien zur Philosophie*, Band 73, Walter de Gruyter, Berlin — New York 2006.

63. Parascandalo Renato and Hösle Vittorio (eds.), "Three Interviews with Paul K. Feyerabend", *Telos* 1995, pp. 115–128.

64. Polanyi Michael, **Personal Knowledge: Towards a Post-Critical Philosophy**, Routledge, London 1962.

65. Polanyi Michael, **The Tacit Dimension**, Doubleday & Company, New York 1966.

66. Preston John, **Feyerabend: Philosophy, Science and Society,** Polity Press, Cambridge 1997.

67. Preston John, "Introduction to Volume 3", in: Paul K. Feyerabend, **Knowledge, Science and Relativism. Philosophical Papers**, Vol. 3, Cambridge University Press, New York 1999, pp. 1–15.

68. Pseudo-Dionysius, **The Divine Names**, in: Pseudo-Dionysius, **The Complete Works**, trans. Colm Luibheid, Paulist Presse, New York — Mahwah 1987, pp. 47–131.

69. Putnam Hilary, "What Theories Are Not", in: Ernest Nagel, Patrick Suppes, Alfred Tarski (eds.), **Logic, Methodology and Philosophy of Sciences**, Stanford University Press., Stanford 1962, pp. 240–251.

70. Rheinberger Hans-Jörg, **On Historicizing Epistemology: An Essay**, Stanford University Press, Stanford 2010.

71. Rutkin Darrell H., "Galileo Astrologer: Astrology and Mathematical Practice in the Late-Sixteenth and Early-Seventeenth Centuries", *Galiæana* 2005, II, 2005, pp. 107–144.

72. Schlick Moritz, **General Theory of Knowledge**, first edition 1925, Springer-Verlag, New York — Vienna 1974.

73. Shaw Jamie and Bschir Karim, "Introduction. Paul Feyerabend's Philosophy in the Twenty-First Century", in: Karim Bschir and Jamie Shaw (eds.), **Interpreting Feyerabend: Critical Essays**, Cambridge University Press, Cambridge 2021, pp. 1–10.

74. Sismondo Sergio, **An Introduction to Science and Technological Studies**, Blackwell, Oxford 2010.

75. Stanley Jason, **Know How**, Oxford University Press, Oxford 2011.

76. Stevenson Jay, **The Complete Idiot's Guide to Eastern Philosophy**, Alpha Books, Indianapolis 2000.

77. Suzuki Daisetz T., **The Zen Kōan as a Means of Attaining Enlightenment**, Charles E. Tuttle Co., Boston — Rutland — Vermont — Tokyo 1994.

78. Suzuki Daisetz T., "Über Zen-Buddhismus", in: Erich Fromm, Daisetz Teitaro Suzuki, and Richard De Martino (eds.), **Zen-Buddhismus und Psychoanalyse**, first edition 1960, Suhrkamp, Frankfurt am Main 1971, pp. 9–100.

79. Tambolo Luca, "Pliability and Resistance — Feyerabendian Insights into Sophisticated Realism", *European Journal for Philosophy of Science* 2014, Vol. 4, No. 2, pp. 197–213, https://doi.org/10.1007/s13194-014-0082-9.

80. Wittgenstein Ludwig, **Culture and Value**, Georg Henrik von Wright (ed.), Blackwell, Oxford 1980.

81. Wittgenstein Ludwig, **Tractatus Logico-Philosophicus**, first edition 1921, trans. David F. Pears and Brian F. McGuinness, with an introduction by Bertrand Russell, Routledge, London — New York 2001.

82. Zammito John H., **A Nice Derangement of Epistemes: Post-positivism in the Study of Science from Quine to Latour**, The University of Chicago Press, Chicago — London 2004.

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 55–69

https://doi.org/10.53763/fag.2023.20.2.221

PRZEKŁAD / TRANSLATION

Gonzalo Munévar

Lawrence Technological University

Feyerabend: The Most Valuable Philosopher of the Twentieth Century [*]

Received: August 4, 2023. Accepted: September 4, 2023. Published online: November 10, 2023.

Abstract: This chapter will argue that Paul K. Feyerabend is the most valuable philosopher of the twentieth century. Given the extraordinary importance of science in the twentieth century, the most valuable philosopher of the century should be someone who has given us the most significant understanding of the nature of science and its impact on the rest of the human experience — Feyerabend did precisely that. I will contrast his accomplishments with those of other important philosophers such as Kuhn, Popper, Wittgenstein, Heidegger, Rawls, Carnap, Quine, Russell, and Dewey. Of critical importance in this regard will be Feyerabend's case for theoretical pluralism, which overturned key ideas from analytical philosophy by demonstrating that all scientific rules, no matter how sound and empirically fruitful, must allow for exceptions. Science as we know it could not have progressed without scientists breaking well-established methodological norms. He argues, with Galileo, that observation assumes theory. As Feyerabend tells us, "We need a dream-world in order to discover the features of the real world we think we inhabit (and which may actually be just another dream-world)".

Keywords:

Against Method; incommensurability; scientific revolution; scientific rationality; theoretical pluralism; theoretical assumptions in observation

[*] The present article was originally published in Spanish as Chapter 1 of book Gonzalo Munévar, **Variaciones sobre Temas de Feyerabend**, Programa Editorial, Universidad del Valle, Cali (Colombia) 2006. It was machine translated into English using ChatGPT (OpenAI, 2023. ChatGPT (May 24 version) [Large language model]. https://chat.openai.com/chat), with subsequent post-editing by Phillip McMurray and Gonzalo Munévar.

I consider Paul Feyerabend to be the most valuable philosopher of the twentieth century. This judgment is, of course, highly subjective and strongly influenced by my own philosophical inclinations. Nonetheless, it is a judgment that can be supported with some very good reasons, which I will present immediately after certain biographical comments.

Feyerabend was born in Vienna in 1924. During World War II, three Russian bullets left him permanently disabled. He managed to recover enough to study physics and astronomy at the University of Vienna. At that time, Vienna was still a city of geniuses. Feyerabend had a remarkable voice and even sang at the Vienna Opera House. At one point, Bertolt Brecht asked him to be his assistant. He also met Konrad Lorenz, who also requested his assistance. Although it was evident that he had many talents, he ended up writing his doctoral thesis in philosophy under Viktor Kraft. After meeting Ludwig Wittgenstein, he made arrangements to work with him in Cambridge, but Wittgenstein's death compelled him to become Karl Popper's assistant instead. All these Viennese figures influenced the young Feyerabend significantly, and ultimately this influence played a crucial role in the intellectual revolution he forged with Thomas Kuhn in the 1960s and 1970s.

I met him in Berkeley in 1972, during my second year of doctoral studies, three years before the publication of his extraordinary book **Against Method** propelled him to worldwide fame. [1] I attended his seminar, initially intending to be just an observer, as many students were intimidated by his critical mind. As soon as he entered and took his seat, he asked me, "What will be the topic of your presentation?". I replied, "I'm just sitting in", "If you want to stay, you'll have to give a presentation", he insisted. "But all my ideas are bizarre", I told him. "Par for the course", he replied, taking out his notebook. "When will you present them?".

During my presentation, I experienced firsthand the disconcerting nature of his criticism — a feeling I would not have wished upon my worst enemy, or even upon myself if I had truly believed back then that criticism is the main source of progress. Feyerabend questioned everything; he would discuss and sometimes mock even the most seemingly obvious assertions. In a conversation with him, no idea could be taken for granted. That day, I criticized him as much as he criticized me, but I left thinking that I had come across as a fool. However, immediately after my presentation, Feyerabend spoke highly of it, was very friendly, and invited me

[1] See Paul K. Feyerabend, **Against Method**, Verso, London — New York 1975.

to lunch at the Golden Bear restaurant. That would be the first of many meals where his insightful remarks would jump from philosophy and science to music, art, or theater, only to circle back to philosophy again. It was the first of many conversations in which we would talk about women or mock each other. He was as captivating in conversation as in lectures. It was difficult, then, to notice his crutches or the constant pain and poor health he had to overcome throughout his adult years. Even prior to his great fame, he was clearly an intellectual giant. I remember his animated face, his contagious laughter, and that extraordinarily sharp mind that delighted his students, colleagues, and friends — a mind worthy of the greatest admiration.

Two physicists once wrote in the famous journal *Nature* that Feyerabend was the worst enemy of science. [2] But on the contrary, what Feyerabend actually did was demonstrate how complex and human science can and should be. Among his many contributions, perhaps the most significant is the recognition that there is no method or rule that can fully capture what science is. Even the most insightful idea regarding the practice of science must allow for exceptions. When we examine the history of science, we discover not only that great scientists violated empiricist methods, but that they *had to* violate them; otherwise, they would not have achieved the great successes for which we know them today.

Until the publication of the works of Feyerabend and Thomas Kuhn, it had been assumed that scientific rationality consisted in behaving according to certain methodological rules. [3] Epistemology, in general, aimed to discover the rules of thought, and since science was traditionally considered the epitome of rationality, a philosophy like Feyerabend's that questioned such rationality would have profound repercussions throughout the field. However, Feyerabend's work goes far beyond skepticism. Until 1962, the main problem in the epistemology of science was that although we "knew" that the scientific method produced knowledge, we couldn't prove it. Karl Popper, another great philosopher of the twentieth century, told us that such a problem was misconceived because the method that created the problem — the method of induction — was not actually the method of science. We simply had to realize that science operated through trial and error, testing our

[2] See Theo Theocharis, Mihalis Psimopoulos, "Where Science has Gone Wrong?", *Nature* 1987, Vol. 329, No. 6140, pp. 595–598, https://doi.org/10.1038/329595a0.

[3] See Thomas Kuhn, **The Structure of Scientific Revolutions**, Chicago University Press, Chicago — London 1962.

theories. [4] However, Feyerabend's analysis of the history of science demonstrated that the supposed method of science, in all its varieties proposed by empiricists from Francis Bacon to Rudolf Carnap and Karl Popper, would hinder scientific progress. In other words, for science to advance, it occasionally needs to go *against the method.*

The reason is quite simple. All varieties of empiricism share something in common: sensory experience determines which of our scientific ideas are worthwhile. This dictum is justified on the grounds that through experience, scientists immediately learn what is written in the book of nature. For instance, if all observers see a stone falling vertically, the stone's vertical motion is a truth immediately given by observation, an immediate truth that our "deeper" hypotheses about the world must remain consistent with. If a hypothesis tells us that the stone does not fall vertically, our observations, our experience, refute it. Unfortunately for empiricism, as Feyerabend reminds us, the Copernican hypothesis — that the Earth moves around the Sun and rotates on its own axis to give us the cycle of day and night — is clearly refuted by such experience.

That was one of the main objections contemplated by Galileo when he defended Copernicus in 1632. If we drop a stone from a tall tower, we see it fall vertically, parallel to the tower, and we see it hit the ground near the tower. Now, let's suppose that the Earth is rotating. In that case, at the moment the stone begins to fall, the tower continues moving with the Earth, and consequently (if we choose the direction appropriately), the tower will have moved a considerable distance before the stone hits the ground. The only way for the stone to fall next to the tower is by moving in a parabolic trajectory, but we all see it fall vertically. It is clear, then, that the Earth cannot be rotating. What did Galileo say in the face of such a direct refutation of Copernicus' theory? He refused to accept the verdict of experience. If the Earth is not moving, of course, the stone falls vertically. But if the Earth is rotating, then the stone must fall in a parabolic path. The reason we

[4] See, for example, his: Karl R. Popper, **Objective Knowledge: An Evolutionary Approach**, Oxford University Press, Oxford 1972. Although for Popper observational sentences are interpreted by low-level theoretical generalizations, which causes a paradox in his philosophy, his viewpoint is very different from Feyerabend's or Galileo's, as we will see later. For a critique of the analogy Popper draws between natural selection and his trial-and-error method, the reader can refer to my article "Karl Popper's Evolutionary Epistemology", in: Andrés Rivadulla (ed.), **Hypothesis and Truth in Science: Essays on the Philosophy of Karl R. Popper**, Editorial Complutense, Madrid 2004, pp. 49–66.

see it fall vertically is that the stone's motion has two components: one shared with the Earth, the tower, and the observer, and the other directed towards the center of the Earth. However, the observer does not perceive the shared motions. (Today, for example, we do not "see" the other passengers on our airplane flying while seated at nine hundred kilometers per hour). That is why it appears to the observer that the stone falls vertically.

Galileo tells us that the motion we choose — or, rather, the facts we choose, pertaining to vertical motion or to parabolic motion — will depend on the theory we favor. Insisting that the stone falls vertically assumes beforehand that the Earth does not move. In other words, opponents of Copernicus assume the truth of what is in question — whether the Earth moves or not — when they declare their experience (i.e. that the stone falls vertically) as true. Their empiricist argument is nothing more than an instance of *petitio principii.* [5]

Feyerabend notes that the observer sees a phenomenon (the motion of the stone) and interprets it in a way that appears natural: the stone falls vertically. It is this "natural" interpretation of the phenomenon — rather than the phenomenon itself — that contradicts the Copernican theory. Galileo resolves the contradiction by offering us a different way to interpret the phenomenon. He thus provides us with a new empirical foundation, consisting in an interpretive theory that aligns with Copernican ideas.

These considerations do not imply that scientific hypotheses or theories always overturn the verdict of experience. However, they do imply that they can do so. And this result, in turn, implies that all empirical methodological rules must have exceptions. The reason is that such rules require the primacy of experience. We have seen, however, that the great Scientific Revolution would not have taken place if Galileo had not violated them. Similar results can be expected from many critical episodes in the history of science, as Feyerabend demonstrates in his work. It is worth emphasizing that it was not just that thanks to a couple of hunches, Galileo was led to take a shortcut which allowed him to obtain results that patient use of the method would eventually have provided. Not at all. If the method had insisted on the primacy of sensory experience, it would have forever closed off the path to a viewpoint that could not have been established without

[5] See Galileo Galilei, **Dialogue Concerning the Two Chief World Systems: Ptolemaic and Copernican**, Modern Library of Science, New York 2001, p. 162.

overturning previously accepted experience. If, in pursuing a theory refuted by experience, Galileo committed a grievous sin against philosophy and science, then we must not only love the sinner, but also the sin.

Feyerabend also notes that often we cannot even uncover significant evidence against our favored theories unless we seriously consider alternative ones that make sense of such evidence, as was the case with the Copernican theory and the composite motion of bodies. Our science, therefore, has greater opportunities for progress if we embrace theoretical pluralism. This is Feyerabend's second important historical contribution to philosophy. No matter how confident we are about the truth of our most favored theory, the scientist who does not accept it and develops a different one is doing a favor to science. As Feyerabend tells us, "We need a dream-world in order to discover the features of the real world we think we inhabit (and which may actually be just another dream-world)". [6]

This second philosophical contribution by Feyerabend goes not only against Newton but also against the important tradition of Plato and Descartes, whose obsession was with discovering the correct path for arriving at a single truth. Throughout the centuries, generation after generation of skeptics cast doubt on the paths to truth suggested by the great philosophers, but Mill was the first significant philosopher to rebel against the goal itself. In his essay "On Liberty", he argued that society should not compel its members to accept the official point of view, no matter how true it may seem. By allowing the development of different viewpoints, society benefits, because if the official perspective turns out to be false, we replace falsehood with at least partial truth. And if the official viewpoint proves to be true in the end, the comparison with other viewpoints enables us to understand it better. Feyerabend's achievement lies in extending Mill's philosophy so that it applies also to science. The latter also benefits by allowing the development of different viewpoints that are not "in agreement with the facts". One of the best examples of how science benefits is precisely the case of Galileo and his defense of the Copernican Revolution. [7]

---

[6] Gonzalo Munévar, **A Theory of Wonder: Evolution, Brain, and the Radical Nature of Science**, *Philosophy of Science*, Vernon Press, Wilmington — Malaga 2021.

[7] See Gonzalo Munévar, "Science in Feyerabend's Free Society", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, pp. 179–198 and Elizabeth A. Lloyd, "Feyerabend, Mill, and Pluralism", in: John Preston, Gonzalo Munévar, and David Lamb (eds.), **The Worst Enemy of Science?**, Oxford University Press,

Feyerabend's sense of irony, so rare in academic writing today, led him to proclaim anarchy in the philosophy of science and suggest that "anything goes". However, he never actually offered up anarchy as a kind of anti-method. Anarchy is the description that a traditional rationalist would give of Feyerabend's conception of science, particularly as it pertains to theoretical pluralism. To such a traditional rationalist, it seems obvious that rationality consists in one's abiding by the rules of the empiricist method. Therefore, for such a rationalist, the notion of "anything goes" in Feyerabendian science appears like the horror of all horrors. [8]

Just as an idea that was discredited for two thousand years can revolutionize science — the idea that the Earth moves — ideas from other cultures can also contribute to the progress of science. This implies that we must treat non-Western cultures with respect, not only despite our admiration for Western advances made possible by science, but precisely because such respect helps to maintain a climate of pluralism that is vital for the progress of our much-celebrated science. From this realization comes a third contribution on the part of Feyerabend.

The lack of respect for the traditions of ordinary people — "the vulgar" as philosophers used to say — and, especially, the unfounded lack of respect based on an empiricist conception of science, wreaks havoc. And when empiricist methods are blindly applied, this lack of respect can lead to intellectual arrogance that causes even greater damage. Let us consider for a moment that until relatively recently, a person could end up in jail for practicing acupuncture (which was considered a medical fraud), that in the name of "development", millions of women in the Third World were advised to stop breastfeeding their children, and a significant amount of money and effort was spent to provide them with powdered milk (which, of course, they mixed with contaminated water on more than one occasion). In the most advanced country in the world, meanwhile, a high percentage of people suffer from obesity and diabetes, due to a "scientific" diet, supported by the State, which forbade eating eggs (even though the human body is well-adapted for consuming these) and fervently emphasized eating refined carbohydrates

New York — Oxford 2000, pp. 115–124.

[8] I address this topic in my Gonzalo Munévar, "A Rehabilitation of Paul Feyerabend", 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 — Oxford 2000; the second appendix of my book Gonzalo Munévar, **Evolution and the Naked Truth**, Ashgate, Aldershot 1998, pp. 219–244.

(to which the human body is not adapted, causing various physiological problems, including the above). [9]

Feyerabend detected this intellectual arrogance in the disdain that many intellectuals feel towards ordinary people, their beliefs, and their traditional customs. That is why he ridiculed intellectuals, pulled apart their "reason", and called them "fanatics" and "criminals" who, by imposing their abstract "truths" on others, create pain and misery in the world. This reaction may seem exaggerated, but it must be understood in its proper context. Firstly, if a tradition has served a group of people well and allowed them to adapt effectively to their environment, we have no right to impose our truth upon them, no matter how scientifically established and well-confirmed it may appear to be. Secondly, many of the abstractions of intellectuals, even if they are labeled as "truth" or "justice", are the result of flawed reasoning (as Feyerabend demonstrated with numerous examples), while those that are valuable are only so within a limited practical context. In his final book, **The Conquest of Abundance**, published posthumously, he explains how scientific abstraction is often necessary to understand the world. The problem arises when philosophers and scientists decide that "reality" corresponds to the impoverished environment resulting from abstraction, while ignoring particular aspects of experience, many of which enrich and give value to our lives. As Feyerabend says, despite the intellectual and practical merits of various abstractions, "details continue to exist, just as people don't cease to have a nose when they step on a scale". [10]

The Platonic thesis that reality is abstract cannot be justified on the basis of the practical outcomes stemming from the most successful abstractions, or from experience or reason. Among the many reasons Feyerabend invokes to counter these rhetorical possibilities, I will choose three: one for each possibility. (1) The scientific theories that have achieved the most outstanding practical results —

[9] Although I do not recall Feyerabend referring to this specific example, it was in one of his seminars at Berkeley that a doctoral epidemiology student explained to us why the nutritional science of that time (the later so-called "food pyramid") had made serious mistakes in concluding that dietary cholesterol, including amongst other foods eggs, caused heart problems. It took thirty years before the trend changed.

[10] Paul K. Feyerabend, **The Conquest of Abundance**, Chicago University Press, Chicago 2000, p. 14. Feyerabend died in 1994. See my commentary, Gonzalo Munévar, "Conquering Feyerabend's Conquest of Abundance", *Philosophy of Science* 2002, Vol. 69, No. 3, pp. 519–535, https://doi.org/10.1086/342457.

quantum physics in our time, and Ptolemaic astronomy in ancient and medieval times — have been, in their official versions, anti-realistic. (2) It is incoherent to expect that experience supports the thesis that the real is abstract, because how can what is real and unmanifest be discovered or proven by what is manifest and unreal? (3) The preferred reasoning of philosophers uses deductive arguments — "proofs" — but proofs require stable and unambiguous concepts. Yet experience and tradition do not furnish such concepts, and, therefore, when intellectuals attack tradition, the "rigor" of their reasoning is likely nothing more than a fallacy of equivocation.

We may, for example, consider the famous argument of Xenophanes, which supposedly established that God is one and eternal, etc., in contrast to the multitude of gods of his time, which closely resembled their worshippers (so that if horses were to depict their gods, they would draw horses, while cows would provide us with paintings and statues of cows). [11]

Let us suppose that God began to exist (He is not eternal). Then He came from something like Himself or from something unlike Himself. If He came from something like Himself, then He already existed. If He came from something unlike Himself, then He came from something more powerful or something less powerful. If He came from something less powerful, then the extra power comes from nothing — but nothing can come from nothing. If He came from something more powerful, then He is not God. Therefore, God did not have a beginning.

However, the last premise here — stating that if He came from something more powerful, then He is not God — assumes that being divine implies having supreme power. Yet this notion of divinity was not accepted by the cultures that Xenophanes argued against, including Greek culture. Xenophanes' argument changes the meaning of key concepts in the debate. Therefore, it is a fallacious one.

At some point, the desire to defend the traditions of ordinary people led Feyerabend to embrace a form of radical relativism in both politics and epistemology. In his book **Science in a Free Society**, he outlined his belief that science was just

[11] For a more detailed review of Xenophanes and Presocratic Ancient Greek philosophy, see Richard D. McKirahan, "Xenophanes of Colophon", in: Richard D. McKirahan (ed.), **Philosophy before Socrates: An Introduction with Texts and Commentary**, Hackett Publishing Company, Indiana 1994, pp. 60–62.

another ideology or tradition, and that, therefore, in a free society it should not have more rights than other ideologies or traditions when it comes to influencing institutions such as medicine or education. [12] In a free society, there should be a clear separation between the State and ideology. This thesis is a generalization of the separation between the State and religion expected of any society that wants to call itself "free", but it is also a generalization of the principle that all citizens have equal rights before the State, giving each ideological grouping a status comparable to that of the individual citizen.

In the long run, Feyerabend abandoned this sensational relativism in response to objections from philosophers such as Margherita von Brentano and the present author. [13] Von Brentano reminded Feyerabend that some ideologies have as one of their main purposes the destruction of other ideologies and traditions. Nazism, for example, was based on hatred towards human beings of different races. The problem with Feyerabend's relativism was that it did not allow the rest of the world to interfere with the perfidy of the Nazis, a consequence he could not endorse. On the other hand, my objection reminded Feyerabend that he had only shown that no idea was inherently superior to all others, whereas he himself had argued (like Mill) that in certain historical situations some ideas were more useful than others. In that vein, where education is concerned, astronomy will function better than astrology, and the study of electricity and magnetism will be much more useful than black magic.

It is possible that Feyerabend would have abandoned his extreme relativism once he had discovered his main arguments against the reification of abstraction. He emphasized the fact that the meanings of words used by both ordinary people and scientists are quite flexible. This creates the possibility that, given human nature, any culture can evolve. In principle, any culture can become any other. He thought that this principle defeats relativism, because relativism (in the sense of something's being relative to theory, culture, ideology, or tradition) requires that the meanings of words be determined by the conceptual framework of a culture, or something similar. It seems to me, though, that there are forms of relativism that are not affected by Feyerabend's argument. Even so, such a discussion is not

[12] See Paul K. Feyerabend, **Science in a Free Society**, NLB, London 1978.

[13] See Margherita von Brentano, "Letter to an Anti-Liberal Liberal", in: Gonzalo Munévar (ed), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, pp. 199–212. My article in the same book has already been mentioned.

essential, given the purposes of this article. Two additional points, however, are relevant. The first is that the arguments against the arrogance of intellectuals towards ordinary people still stand. All human beings and the traditions they practice in good faith deserve respect.

The second point, meanwhile, is an important corollary for the interpretation of the famous problem of the incommensurability of theories, which was created by Feyerabend himself with his accomplice Kuhn in 1962. In his famous article "Explanation, Reduction and Empiricism", [14] Feyerabend toppled the logical approach in the philosophy of science when he showed that scientific explanations cannot be logical derivations, at least in the most interesting cases presented by philosophy and science: namely, those where one scientific theory is replaced by another (e.g., Newtonian mechanics by Einstein's Theory of Special Relativity). Explanation was supposed to be a form of logical reduction: the old theory was derived from the new theory as a special case of the new theory. The problem, Feyerabend told us, is that the meanings of various crucial terms often change when there is a change of theory. In Newtonian mechanics, the value of mass does not depend on the velocity between the object and the observer; in Einstein's theory, it does. In Newtonian mechanics, time is absolute; in Einstein's, it is relative to the frame of reference. What happens, according to Feyerabend, is that by introducing Einsteinian concepts, the use of Newtonian concepts is excluded. However, and here the problem becomes more acute, a derivation is not valid if the meaning of a term in the premises (the new theory) is different from the meaning of that term in the conclusion (the old theory). That is to say, either science explains nothing or the logical approach is useless. [15]

What Feyerabend tells us in his books is that this problem of the incommensurability of theories generally does not exist for scientists, as they use language in a flexible way that allows them to move from one theory to another. It does exist, however, for "rigorous" philosophers who believe that the "clarity" and "rigidity" of the terms they use are the philosophical tools par excellence.

[14] Paul K. Feyerabend, "Explanation, Reduction and Empiricism", *Minnesota Studies in the Philosophy of Science* 1962, Vol. 3, H. Feigl and G. Maxwell, Minnesota 1962, https://tiny.pl/c5jlk [04.09.2023].

[15] This was his favorite example in the various editions of **Against Method**.

In 1962, it was rhetorically prudent to express oneself as Feyerabend did because, at that time, "serious" philosophy (analytic philosophy) was completely dominated by logical and linguistic approaches. However, we may note that the problem of incommensurability has little to do with semantics. Let us go back to Galileo. He replaced one set of "facts" (the vertical fall of objects) with another (the parabolic motion of objects). If one starts by accepting that the Earth does not move, then the first set of facts will be preferred. Conversely, if one accepts that the Earth moves, the second will be. In other words, there is no set of facts that can decide between the two theories. This result means that there is no common measure that allows us to assign more points to one theory than to the other. To say that two theories do not have a common measure is simply to say that they are incommensurable.

This fourth contribution of Feyerabend greatly contributed to the shipwreck of analytic philosophy. The reaction of analytic philosophers has been very severe, of course. Feyerabend is often accused of using reason to attack reason (or using logic to attack logic, or argument to attack argument, etc.). Such objections have no merit. As Feyerabend states, he does not have to believe in reason to undermine it. He simply accepts his opponent's premises and methods in order to arrive at an absurd conclusion: one that appears absurd to the opponent, even though they do not know how to refute it. [16]

I do not want to suggest that Feyerabend has already said everything, nor that I agree with everything he said. My own approach to philosophy is quite different from his, but it definitely recognizes the profound change that he brought about in philosophy. What I do want to assert is that these four contributions from him that I have briefly discussed, among many others that I have not even had time to mention, are so important, so revolutionary, so brilliant, that they clearly make him the most valuable philosopher of the twentieth century.

That century gave us other important philosophers, no doubt, but I do not believe that they reached the same heights as him. Some because they contributed little or nothing to the elucidation of science, a definitive aspect of the human experience for the last four centuries, which dominated the thinking of many great philosophers during the first three of those centuries. (I am referring to thinkers like Descartes, Berkeley, Hume and Kant). In the twentieth century, science played

[16] I address this topic in more detail in my Munévar, "A Rehabilitation of...".

an especially important role. The paramount philosopher of the twentieth century must necessarily be someone who has given us a very significant revelation about the nature of science and its impact on the rest of human experience. Feyerabend did that. In contrast, Wittgenstein sought to exclude science from philosophy, and Heidegger had very little to say about it. Therefore, I exclude them both, although I believe they also made great contributions to philosophy. And for similar reasons, I exclude Rawls, the leading thinker of the twentieth century in the field of moral and social philosophy. Carnap and Quine simply made too many mistakes. Russell and Dewey never participated in the most critical debates. Of the three great philosophers who did, Popper is the most popular philosopher among scientists, but we have already seen that his method does not withstand the criticism of Feyerabend and Kuhn. As for the revolutionary Kuhn, although I find him highly admirable, I believe that his emphasis on the dogmatism of science falls short in the face of Feyerabend's arguments in favor of theoretical pluralism and all that it implies.

I conclude where I began: Paul Feyerabend was the most valuable philosopher of the twentieth century. I must add, however, that Feyerabend would have been greatly annoyed with me for defending him in such a manner. Such an honor would have seemed intolerable to him — an instance of academic pomposity that would have driven him crazy. In that sense, he was not like others. In many ways he was not. When Russian doctors told him that he would never walk again, he immediately delighted in imagining himself rolling around in a wheelchair through a huge library. I can still picture him, animated, engaged in discussion, as a mischievous philosophical idea is born in his eyes and mischievously springs from his lips. Despite illness and tragedy in his life, he lived life to enjoy it. [17]

Gonzalo Munévar

References

1. Feyerabend Paul K., **Against Method**, Verso, London — New York 1975.
2. Feyerabend Paul K., "Explanation, Reduction and Empiricism", *Minnesota Studies in the Philosophy of Science* 1962, Vol. 3, H. Feigl and G. Maxwell, Minnesota 1962, https://tiny.pl/c5jlk [04.09.2023].

[17] See his autobiography Paul K. Feyerabend, **Killing Time: The Autobiography of Paul Feyerabend**, Chicago University Press, Chicago 1995.

3. Feyerabend Paul K., **Killing Time: The Autobiography of Paul Feyerabend**, Chicago University Press, Chicago 1995.

4. Feyerabend Paul K., **Science in a Free Society**, NLB, London 1978.

5. Feyerabend Paul K., **The Conquest of Abundance**, Chicago University Press, Chicago 2000.

6. Galileo Galilei, **Dialogue Concerning the Two Chief World Systems: Ptolemaic and Copernican**, Modern Library of Science, New York 2001.

7. Kuhn Thomas, **The Structure of Scientific Revolutions**, Chicago University Press, Chicago — London 1962.

8. Lloyd Elizabeth A., "Feyerabend, Mill, and Pluralism", in: John Preston, Gonzalo Munévar, and David Lamb (eds.), **The Worst Enemy of Science?**, Oxford University Press, New York — Oxford 2000, pp. 115–124.

9. Margherita von Brentano, "Letter to an Anti-Liberal Liberal", in: Gonzalo Munévar (ed), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, pp. 199–212.

10. McKirahan Richard D., "Xenophanes of Colophon", in: Richard D. McKirahan (ed.), **Philosophy before Socrates: An Introduction with Texts and Commentary**, Hackett Publishing Company, Indiana 1994, pp. 60–62.

11. Munévar Gonzalo, "A Rehabilitation of Paul Feyerabend", 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 — Oxford 2000.

12. Munévar Gonzalo, **A Theory of Wonder: Evolution, Brain, and the Radical Nature of Science**, *Philosophy of Science*, Vernon Press, Wilmington — Malaga 2021.

13. Munévar Gonzalo, "Conquering Feyerabend's Conquest of Abundance", *Philosophy of Science* 2002, Vol. 69, No. 3, pp. 519–535, https://doi.org/10.1086/342457.

14. Munévar Gonzalo, **Evolution and the Naked Truth**, Ashgate, Oxford — New York 1998, pp. 219–244.

15. Munévar Gonzalo,"Karl Popper's Evolutionary Epistemology", in: Andrés Rivadulla (ed.), **Hypothesis and Truth in Science: Essays on the Philosophy of Karl R. Popper**, Editorial Complutense, Madrid 2004, pp. 49–66.

16. Munévar Gonzalo, "Science in Feyerabend's Free Society", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, pp. 179–198.

17. Munévar Gonzalo, **Variaciones sobre Temas de Feyerabend**, Programa Editorial, Universidad del Valle, Cali (Columbia) 2006.

18. Popper Karl R., **Objective Knowledge: An Evolutionary Approach**, Oxford University Press, Oxford 1972.

19. Theocharis Theo, Psimopoulos Mihalis, "Where Science has Gone Wrong?", *Nature* 1987, Vol. 329, No. 6140, pp. 595–598, https://doi.org/10.1038/329595a0.

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 71–75

https://doi.org/10.53763/fag.2023.20.2.220

LIST DO REDAKCJI / LETTER TO THE EDITOR

Matteo Motterlini

Università Vita-Salute San Raffaele

The Legacy of Paulus Empiricus

Received: August 27, 2023. Accepted: September 10, 2023. Published online: September 21, 2023.

Paul K. Feyerabend was a sceptical master and iconoclast about the philosophy of science. He denounced the break between the abstract, normative, philosophical accounts of science and the actual, complex and context-dependent scientific practice. Feyerabend's first iconoclastic enterprise was directed against philosophical empiricism: roughly, the view that what is to be believed is what experiences establishes, and no more. In fact, Feyerabend's line of attack is broad and applies to any foundationalist epistemology. A naïve appeal to experience assumes that what experience delivers is evident and unequivocal, and thus scientific theories can be grounded on independently meaningful observations. To Feyerabend, this view is at variance with actual scientific practice. Empiricism in the form in which is theorised by some Logical Empiricists philosophers cannot fulfil the hope of the progress of knowledge; on the contrary it is bound to lead to "a dogmatic petrifaction" of theories and "the establishment of a rigid metaphysics". [1]

Against Method aimed at demystifying a second philosophical idol: the existence of a strictly binding system of rules for (good) scientific practice. [2] Feyerabend observes the abyss that exists between the "real thing" and the various images of science by which we are possessed. The fine dividing line between the

[1] Paul K. Feyerabend, **Knowledge, Science and Relativism,** *Philosophical Papers*, Vol. 3, John Preston (ed.), Cambridge University Press, Cambridge 1999, p. 82.

[2] See Paul K. Feyerabend, **Against Method**, 3rd revised ed., Verso, London 1993.

practice of science and the epistemological "castles in the air" is in fact very similar to the line we draw between "normal" and "insane" people: a trait which recurs among the latter is the tendency to detach themselves further and further from reality. Normative philosophy of science that aims at general norms of the sciences are therefore a "hitherto unexamined form of mental illness". Feyerabend's therapy for philosophers' schizophrenic detached from scientific reality is methodological anarchism.

Anything goes (perhaps paradoxically) is the only general principle to which the coherent rationalist can commit himself, if he is looking for a valid rule in any given historical situations. But at the same time, it is not a principle — at least in Feyerabend's intention — since it is not introduced "to replace one set of general rules by another set"; rather "to convince the reader that all methodologies, even the most obvious one, have their own limits". [3] Scientific reality is always richer in content, more varied, more many-sided, more lively and subtle to be captured by the simple-mind rules of even the best philosopher or historian. Rules of "good science" taken as descriptions will not help the philosopher to understand science; and taken as prescriptions they will not help the scientist to do any better science either. Scientists are not rule-followers but opportunists. Not only Galileo developed a research program in striking contrast with the Aristotelian standards and the accepted observation of the time, he was also prepared to defend it. Feyerabend's Galileo had to substitute a "natural" interpretation about motion (motion can only be expressed in terms of observable changes) with an "unnatural" and highly theoretical concept of motion which introduced into the phenomenon some components (such as the circular inertia, i.e. the motion that objects share with the Earth) that cannot be observed. In this way Galileo was able to "defuse a mine" placed under the Copernican system by "explaining away" the objection regarding the motion of the Earth. This move was possible, again, because of the theoretical nature of experience. That is, experience does not travel from the external worlds directly into our brains through the medium of our senses. On the contrary, our beliefs and our observations are closely connected. People see a phenomenon and interpret it in what they regard as a natural way according with their beliefs. So it is the interpretation of the phenomenon and not the phenomenon itself which is in contradiction with a given belief. Galileo then resolved the

[3] Feyerabend, **Against Method...**, p. 23.

contradiction between the observation and the Copernican view by providing a new and highly abstract observational language and thus a newly constructed empirical basis. This, in turn, was a new theory of interpretation (containing the idea of the relativity of motion and the law of circular inertia) fitting to the Copernican system. [4]

The third idol demystified by Feyerabend was the unity of science. Years after the publication of **Against Method**, Feyerabend acknowledged that contemporary science exhibits disunity at the methodological and theoretical level, but also and especially at the experimental and laboratory level: "terms such as »experiment« and »observation« cover complex processes containing many strands".[5] Although unity of science is a regulative ideal favoured by philosophers, the so called "scientific world view" is just the result of deception or wishful thinking. Feyerabend explicitly refers to the "path breaking" works of the new constructivist and empiricist philosophers — such as Ian Hacking, Nancy Cartwritght, John Dupré, Andrew Pickering, Peter Galison — as a further articulation of his criticism to methodological monism. These works build on one basic consequence of **Against Method**, that is that "there can be many different kind of sciences". [6] The apparent achievements of science, Feyerabend argues, it is not a consequence of its allegedly systematic, unified and coherent nature. On the contrary, to be successful are the particular models and procedures within the specific disciplines, with their somewhat arbitrary compartments and their casual overlaps.

At the time of his death, Feyerabend was at work on the **Conquest of Abundance**, the subtitle (**A Tale of Abstraction Versus the Richness of the Being**) [7] hints once again at the poverty of the "reality" produced by the effect of the abstraction brought by the scientific enterprise compared to the abundance, richness and boundless variety of the world around us. This unfinished book together Feyerabend's fascinating autobiography, **Killing Time** show that Feyerabend was neither the worst enemy of science depicted by some of his commentators, nor

[4] See Feyerabend, **Against Method...**, pp. 55–85.

[5] Feyerabend, **Against Method...**, p. xi.

[6] Feyerabend, **Against Method...**, p. 2.

[7] See Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago 1999.

the irrationalist philosopher criticized by most of the profession. [8] He was primarily a sceptic about the foundation of knowledge and a cunning rhetorician who knew how to use effectively all the ancient sceptical tropes. **Against Method** refers to Greek sceptics and many Pyrronian texts of Sextus Empiricus including "Against the Physicists. Against the Ethicists", "Against the Logicians" and so on. [9] It is thus not surprising to find out that Feyerabend used to entertaining Lakatos by signing some of his letters and postcards to him as "Paulus Empiricus — hinting of course at his Pyrronian predecessor. [10] Scepticism to him was not only a powerful rhetorical devise but also well regarded in its normative implication for the practice of science and for the role of science in a "free society". Feyerabend's iconoclastic enterprise was neither against reason nor science. It was against the idea that there is some unique set of rules (whatever they are) to follow in order to produce good science (whatever it is). If anything goes, reason sometimes goes too. Feyerabend's arguments are often to be intended as a *reductio ad absurdum.* In a *reductio* one assumes for the sake of argument the opponent's position and then derives a conclusion unacceptable to that opponent. Far from a self-defeating scepticism, Feyerabend presented an impressive challenge to the received view in the philosophy of science. He argued that the elegant but useless epistemological accounts should be substitute by a detailed study of the scientific practices and of the primary sources in the history of science. In this respect, the legacy of Paulus Empiricus can be hardly overestimated.

Matteo Motterlini

References

1. Feyerabend Paul K., **Against Method**, 3rd revised ed., Verso, London 1993.

[8] See Paul K. Feyerabend, **Killing Time: The Autobiography of Paul Feyerabend**, Chicago University Press, Chicago 1996.

[9] Sextus Empiricus, "Against the Logicians", Richard Bett (ed. and trans.), Cambridge University Press, Cambridge — New York 2005; Sextus Empiricus, „Against the Physicists. Against the Ethicists", *Loeb Classical Library*, No. 311, Harvard University Press, Cambridge 1936.

[10] Matteo Motterlini (edited by), **For and Against Method. Including Lakatos's Lectures on Scientific Method, and the Lakatos-Feyerabend Correspondence**, University of Chicago Press, Chicago 1999.

2. Feyerabend Paul K., **Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago 1999.
3. Feyerabend Paul K., **Killing Time: The Autobiography of Paul Feyerabend**, Chicago University Press, Chicago 1996.
4. Feyerabend Paul K., **Knowledge, Science and Relativism,** *Philosophical Papers,* Vol. 3, John Preston (ed.), Cambridge University Press, Cambridge 1999.
5. Feyerabend Paul K., **Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago 1999.
6. Motterlini Matteo (edited by), **For and Against Method. Including Lakatos's Lectures on Scientific Method, and the Lakatos-Feyerabend Correspondence**, University of Chicago Press, Chicago 1999.
7. Sextus Empiricus, "Against the Logicians", Richard Bett (ed. and trans.), Cambridge University Press, Cambridge — New York 2005;
8. Sextus Empiricus, „Against the Physicists. Against the Ethicists", *Loeb Classical Library*, No. 311, Harvard University Press, Cambridge 1936.

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 77–121

https://doi.org/10.53763/fag.2023.20.2.229

ARTYKUŁ ORYGINALNY / ORIGINAL ARTICLE

Yuanlin Guo
Tianjin University

Chubi Yan
Tianjin University

Paul Feyerabend and Marxism

Received: October 2, 2023. Accepted: November 1, 2023. Published online: April 18, 2024.

Abstract: This article discusses the relationship between Paul Feyerabend and Marxism. Feyerabend mentioned, referenced, quoted, discussed or commented on the following Marxists, communists or leftists in his writings: Marx, Engels, Lenin, Stalin, MAO, Fidel Castro, Karl Kautsky, Eduard Bernstein, Rosa Luxemburg, Leon Trotsky, Bertolt Brecht, Hanns Eisler, Walter Hollitscher, Georg Lukacs, Ernst Bloch, Herbert Marcuse, Theodor Adorno, Jürgen Habermas, Joseph Needham, Jean-Paul Sartre, Walter Benjamin, Louis Althusser, Daniel Cohn-Benit and Robin Blackburn. On numerous occasions he discussed and commented on Dadaism, Marxism, communism, anarchism, liberalism, dialectical materialism, reductive materialism and, especially, eliminative materialism. He originated a Dadaistic philosophy, and in particular a Dadaistic epistemology. He did not convert to dialectical materialism; nevertheless, Dadaism seems highly relevant to Marxism and communism. As a Dadaist in philosophy he could well have been a Marxist, a non-Marxist, or an anti-Marxist.

Keywords:

dadaism;
marxists;
materialism;
Paul Feyerabend

1. Introduction

This article discusses the relationship between Paul Feyerabend (1924–1994) and Marxism. Feyerabend read many Marxist works. He mentioned, referenced, quoted, discussed or commented on the following Marxists, communists or leftists in his writings: Karl Marx (1818–1873), Friedrich Engels (1820–1895), Vladimir Lenin (1870–1924), Joseph Stalin (1879–1953), Zedong Mao (1893–1976), Karl Kautsky (1854–1938), Eduard Bernstein (1850–1932), Rosa Luxemburg (1871–1919), Leon Trotsky (1879–1940), Fidel Castro (1926–2016), Bertolt Brecht (1878–1956), Hanns Eisler (1898–1962), Walter Hollitscher (1911–1986), Georg Lukacs (1885–1971), Ernst Bloch (1885–1977), Herbert Marcuse (1898–1979), Theodor Adorno (1903–1969), Jürgen Habermas (1929), Joseph Needham (1900–1995), Jean-Paul Sartre (1905–1980), Walter Benjamin (1892–1940), Louis Althusser (1918–1990), Daniel Cohn-Benit (1945) and Robin Blackburn (1940). Of course, in class he read some of their writings, such as Lenin's "What Is to Be Done?" and "Left-wing Communism, An Infantile Disorder", and Mao's "Oppose Stereotyped Party Writing". [1] He also invited some people from the SDS (Students for a Democratic Society, the main leftist white student group in the years of protesting against the Vietnam War) and representatives of the Gay Liberation Front to his class. [2] Moreover, he had three assistants, all of whom were leaders of the leftist student revolt in the years of the Vietnam War protests. [3] Accordingly, he sometimes called himself "the Red Paul" [4] and wrote "Long Live Marx" in a letter to one of his friends. [5] In short, Feyerabend believed that he belonged to the left. [6]

[1] See Wilhelm Baum (ed.), **Paul Feyerabend, Hans Albert: Briefwechsel Band 1 (1958–1971)**, Kitab, Vienna 2008, p. 268; Lakatos and Feyerabend 1999, p. 210).

[2] See Imre Lakatos and Paul Feyerabend, **For and Against Method**, edited and with an Introduction by Matteo Motterlini, The University of Chicago Press, Chicago 1999, p. 191; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 212.

[3] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 139.

[4] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 160; Baum Wilhelm and Mühlmann Michael (eds.), **Paul Feyerabend, Hans Albert: Briefwechsel, Band 2, (1972–1986)**, Kitab, Vienna 2009, p. 8.

[5] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 273.

[6] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 139.

It seems that his most important work, **Against Method** is closely connected to the New Left and to Marxism. In a letter to Feyerabend, his closest friend Imre Lakatos (1922–1974) wrote that "the whole thing (**Against Method**) [was] set against the dramatic background of the student revolt and the New Left uprising". [7] Feyerabend himself expressed much the same idea, writing that "Anyway, I now see my **Against Method** as a weak and stumbling prologue to what others have done much better: Cohn-Bendit, for example. [...] It is interesting to see, by the way, how many people to whom I have sent my **Against Method** shrink back from it because it contains such names as Lenin etc.". [8] Cohn-Bendit was the leader of the French student revolt which led to the Paris events of May 1968. Joseph Agassi (1927) claimed that Cohn-Bendit ("Danny the Red") was Feyerabend's predecessor in politics, [9] while Lakatos called the students of the left "the Cohn-Bendit/Feyerabend mob". [10] Furthermore, Feyerabend admired Lenin and Mao so blindly that his friend Hans Albert (1921) commented as follows: "I have the impression that you have now become very insensitive because of immersion in your favorite left authors". [11] Feyerabend said that if he could work in New Zealand, he would be closer to "Chairman Mao" than in America. [12] As a result, Agassi made the following comment: "As we shall see, Feyerabend's ideal is totalitarian China [...]". [13] Besides, Lakatos considered him "the hero of the New Left" and called him a "darling of the New Left". [14]

On the other hand, Feyerabend criticized the New Left, contemporary Marx-

[7] Lakatos and Feyerabend, **For and Against Method**..., p. 220.

[8] Lakatos and Feyerabend, **For and Against Method**..., p. 153. Moreover, Feyerabend believed that publishing his book **Against Method** with New Left Books had helped the left. He wrote in a letter to his friend Hans Peter Duerr (1942) that "I have also helped the left, that is, the New Left in London, for their whole series would have found no US distributor without my book" (Paul K. Feyerabend, **Briefe an einen Freund**, Hans Peter Duerr (ed.), Suhrkamp, Frankfurt am Main 1995, p. 80).

[9] See Joseph Agassi, "Review Essay of **Against Method** by Paul Feyerabend", *Philosophia* 1976, Vol. 6, No. 1, p. 166 [165–191].

[10] Lakatos and Feyerabend, **For and Against Method...**, p. 157.

[11] Wilhelm Baum (ed.), **Paul Feyerabend — Hans Albert Briefwecihsel**, Fischer Taschenbuch Verlag GmbH, Frankfurt am Main 1997; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 164.

[12] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 256.

[13] Agassi, "Review Essay of **Against Method...**", p. 167 [165–191].

[14] Lakatos and Feyerabend, **For and Against Method...**, pp. 220 and 229.

ism, the "revolutionaries" and the students of the left, even while praising Marx, Lenin and Mao. As he put it:

> It is now quite clear to me that the "New Left" are a bunch of constipated academics who have hardly anything in common with either Marx, or Lenin, or Mao. I was always surprised by the arid character of most articles in the "New Left Review". Now I know that this is not an accident. It is intended. [15]

According to Feyerabend, "[i]deologies can deteriorate and become dogmatic religions (example: Marxism)". [16] Thus, contemporary Marxism had degenerated because contemporary Marxists were no longer learning from their tradition. [17] In his opinion, contemporary Marxism had degenerated into a form of intellectual gossip: "Nowadays, Marxism itself is always only a form of intellectual gossip, in which the followers of Althusser quarrel with the pure Marxists, with the followers of Bakunin, with the followers of Kautsky, and so on and so forth [...] one has mostly forgotten the human function of Marxism ...". [18]

Indeed, in his book **Science in a Free Society** he attacked contemporary Marxism in the following terms:

> It is true that Marxism once went a different way and had different aims. But the vision of the founders has now become a doctrine, their insights have been buried in footnotes and the small group of humanitarians has turned into a swarm of intellectuals who criticize other intellectuals and are taken to task by still further intellectuals, a tearful line here and there replacing the humanitarianism that is absent from the whole enterprise. [19]

To a degree, both neo-Marxism and certain of the communists bored Feyerabend. [20] Thus, he strongly criticized the "revolutionaries" (the radical left):

---

[15] Lakatos and Feyerabend, **For and Against Method...**, p. 294.

[16] Paul K. Feyerabend, **Science in a Free Society**, Verso, London 1982, p. 75.

[17] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, pp. 268 and 274.

[18] Paul K. Feyerabend, **Thesen zum Anarchismus: Artikel aus der Reihe "Unter den Pflaster liegt der Strand"**, Thorsten Hinz (ed.), Karin Kramer Verlag, Berlin 1996, p. 188. For his similar criticism of contemporary Marxism, see also: Christian Augustin (ed.), **Aber ein Paul hilft doch dem anderen: Paul Feyerabend — Paul Hoyningen-Huene Briefwechsel 1983–1994**, Passagen Verlag, Vienna 2010, pp. 165–166.

[19] Feyerabend, **Science in a Free...**, p. 176.

> I criticize their ideas, their behaviour, their morality. Their tactics. What I get back is the same aria, again and again, no progress, phrases and more phrases, and a primitive mentality where one talks about slaughter as if it were a picnic. Well, if ever they take over, I shall leave this country at once, for I do not like to be surrounded by barbarians who shout so loudly that one hears them everywhere. [21]

Of course, Feyerabend did not entirely oppose the radical left. As he put it, "I completely accept the aim of the radical student, but I consider their tactics obsolete and uninformed, their general philosophy primitive, and their attitude antihumanitarian". [22] That is to say, he was in favor of their aim, but against their tactics, general philosophy and attitude. As a matter of fact, his criticism of the New Left was levelled at the extremists on the left. It was directed "against the »new«, but actually age old, antediluvian, primitive Puritanism of the »new« left which is always based on anger, on frustration, on the urge for revenge, but never on imagination. Restrictions, demands, moral arias, generalized violence everywhere". [23] Feyerabend criticized the radical left on the grounds that they preferred anger, restrictions, revenge and violence to imagination and humor.

Nonetheless, Marxists and leftists seemed fond of Feyerabend, even though he criticized the New Left, the "revolutionaries" and the students of the left. As his friend Roy Edgley (1925–1999, one of the leading figures of the New Left) put it, "[i]t's little wonder that the Left took Feyerabend to their hearts". [24] Students on the left invited Feyerabend to attend their activities. [25] A journal used Feyerabend's article to produce "propaganda for dialectical materialism". [26] Maoists in London published the collected works of Feyerabend, which were then translated

---

[20] See Baum Wilhelm and Mühlmann Michael (eds.), **Paul Feyerabend, Hans Albert...,** pp. 53 and 69.

[21] Lakatos and Feyerabend, **For and Against Method...**, p. 185.

[22] Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 166.

[23] Feyerabend Paul K., **Problems of Empiricism, Philosophical Papers, Volume 2**, Cambridge University Press, Cambridge 1981, p. 70; Feyerabend, **Science in a Free Society**, p. 133.

[24] 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, p. 28 [27–56], https://doi.org/10.1016/j.shpsa.2015.08.004.

[25] See Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert...**, pp. 156–157.

[26] Baum (ed.), **Paul Feyerabend...**, p. 44; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 112.

into Italian and published by Leninists in Italy. [27] According to Lakatos, Feyerabend had a great influence on "all the Marxist students" at the London School of Economics. Lakatos wrote in a letter to Feyerabend that "[t]he only good news from the departments is that everybody is learning German to read your recent stuff. I also understand that all the Marxist students in the School now learn German to read Marx and Feyerabend". [28]

It seems obvious that Feyerabend and his writings were preferred and extolled by some of his Marxist and leftist contemporaries. For example, two Australian Marxist philosophers Jean Curthoys (1947) and Walter Suchting (1931–1997) reviewed Feyerabend's principal work **Against Method** from a Marxist point of view. They observed that "[n]ot only does **Against Method** appear under the imprint of one of the leading publishers of English-language books of a »left« (mostly Marxist) orientation, but his views have found some reception among Marxists and radicals generally". [29] However, they criticized Feyerabend on the grounds that he represented "an extreme subjectivism and scepticism" (epistemologically), and "an extreme individualism" (ethico-politically), which were deeply hostile to Marxism. [30] Accordingly, the relation of Feyerabend to Marxism appears complex and vague. Indeed, Feyerabend was, generally speaking, a figure who was complicated and difficult to pin down in precise terms. He and his relation to Marxism need to be investigated.

More importantly, the relation of Feyerabend to Marxism surely calls for study, as no scholar has so far conducted systematic and detailed research in this area. It is worth noting, for example, that John Watkins and Matteo Collodel have discussed the relationship between Feyerabend and Popperians, [31] while Gonzalo

[27] See Baum (ed.), **Paul Feyerabend...**„ p. 99; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 159.

[28] Lakatos and Feyerabend, **For and Against Method...**, pp. 310–311.

[29] Jean Curthoys and Walter Suchting, "Feyerabend's Discourse against Method: A Marxist Critique", *Inquiry* 1977, Vol. 20, No. 1–4, pp. pp. 249–250 [243–379], https://doi.org/10.1080/00201747708601836.

[30] See Curthoys and Suchting, "Feyerabend's Discourse against Method...", p. 338 [243–379].

[31] See John Watkins, "Feyerabend Among Popperians, 1948–1978", in: John Preston, Gonzalo Munevar, and David Lamb (eds.), **The Worst Enemy of Science? Essays in Memory of Paul Feyerabend**, Oxford University Press, New York 2000, pp. 47–57; Collodel, "Was Feyerabend a Popperian?...", p. 28.

Munevar regards "the work of Plato, Aristotle, Galileo, Machiavelli and Mill" as furnishing "valuable historical antecedents to Feyerabend's philosophy" [32] and Paul Hoyningen-Huene has discussed and analyzed the relation between Thomas Kuhn and Paul Feyerabend in his writings. [33] Eric Oberheim's research, meanwhile, shows that the development of Feyerabend's ideas occurred under the influence of Einstein, Wittgenstein, Popper and Ehrenhaft, [34] and Yuanlin GUO and Xin ZHENG have analyzed the influence of Wittgenstein on Feyerabend's philosophical development. [35] Moreover, Ian Kidd has commented on the influence of Søren Kierkegaard and Pseudo-Dionysius on Feyerabend in two articles. [36] Nevertheless, none of the above-mentioned authors have investigated the relationship between Feyerabend and Marxism, even though they have carried out a considerable amount of research into the relation of Feyerabend to other thinkers. To be sure, Rory Kent has engaged briefly, and at a simple level, with "Feyerabend's Engagements with Marxism" and "Feyerabend's »Dialectical Materialism and the Quantum Theory«" — though the subject of his article is actually "philosophical Dadaism". [37] It is for this reason that the present article aims to deal with the relationship of Feyerabend to Marxism in detail.

---

[32] Gonzalo Munévar, "Historical Antecedents to the Philosophy of Paul Feyerabend", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 9–16, https://doi.org/10.1016/j.shpsa.2015.11.002.

[33] See Paul Hoyningen-Huene (ed.), "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, pp. 353–387, https://doi.org/10.1016/0039-3681(95)00005-8; Paul Hoyningen-Huene, "Paul Feyerabend and Thomas Kuhn", in: Preston John, Munevar Gonzalo, and Lamb David (eds.),**The Worst Enemy of Science? Essays in Memory of Paul Feyerabend**, Oxford University Press, New York — Oxford 2000, pp. 102–114; Paul Hoyningen-Huene, "More Letters by Paul Feyerabend to Thomas S. Kuhn on Proto-Structure", *Studies in History and Philosophy of Science* 2006, Vol. 37, No. 4, pp. 610–632, https://doi.org/10.1016/j.shpsa.2006.09.007.

[34] See Eric Oberheim, **Feyerabend's Philosophy**, Walter der Gruyter, Berlin 2006; Eric Oberheim, "Rediscovering Einstein's Legacy: How Einstein Anticipated Kuhn and Feyerabend on the Nature of Science", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 17–26, https://doi.org/10.1016/j.shpsa.2015.11.005.

[35] See Yuanlin Guo and Xin Zheng, "Wittgenstein's Influence on Feyerabend", *Jiangxi Shehuikexue (Jiangxi Social Sciences)* 2016, Vol. 36, No. 10, pp. 25–32.

[36] See Ian Kidd, "Objectivity, Abstraction and the Individual: The Influence of Søren Kierkegaard on Paul Feyerabend", *Studies in History and Philosophy of Science* 2011, Vol. 42, No. 1, pp. 125–134; Ian Kidd, "Feyerabend, Pseudo-Dionysius, and the Ineffability of Reality", *Philosophia* 2012, Vol. 40, No. 2, pp. 365–377, https://doi.org/10.1007/s11406-011-9322-9.

The article consists of four sections: Section One ("Introduction"); Section Two ("Marxists in Feyerabend's Writings and Pertinent Comments"), centered on how (or why) Feyerabend referred to or construed Marxists and leftists as he did; Section Three ("Feyerabend on Dadaism and Dialectical Materialism"), centered around his discussions of Dadaism, communism, anarchism, liberalism, dialectical materialism, eliminative materialism and reductive materialism; and, finally, Section Four, ("Conclusion"), in which it is concluded that Feyerabend was a Dadaist in philosophy, and that as a Dadaistic philosopher he might be considered a Marxist, a non-Marxist, or an anti-Marxist.

2. Marxists in Feyerabend's Writings and Pertinent Comments

This section deals with how Marxists figure in Feyerabend's writings. According to the Communist Party of China (CPC), they can be divided into two different categories: the first one is classical or orthodox Marxists, to which Marx, Engels, Lenin, Stalin, Mao and Fidel Castro belong; the second one is other (non-classical) Marxists and leftists, among which are Karl Kautsky, Eduard Bernstein, Rosa Luxemburg, Leon Trotsky, Bertolt Brecht, Hanns Eisler, Walter Hollitscher, Georg Lukacs, Ernst Bloch, Herbert Marcuse, Theodor Adorno, Jürgen Habermas, Joseph Needham, Jean-Paul Sartre, Walter Benjamin, Louis Althusser, Daniel Cohn-Benit and Robin Blackburn. These two categories of Marxists will therefore be discussed in turn in what follows.

2.1 Classical Marxists

In a letter to Feyerabend, Hans Albert, one of his friends, after having read many Marxist works, praised Marx's **Capital** as "one of the best economic and historical books" and "the crown of classical economics". [38] Correspondingly, in his

[37] Rory Kent, "Paul Feyerabend and the Dialectical Character of Quantum Mechanics: A Lesson in Philosophical Dadaism", *International Studies in the Philosophy of Science* 2022, Vol. 35, No. 1, pp. 51–67, https://doi.org/10.1080/02698595.2022.2075687.

[38] Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 269.

letter to Albert, Feyerabend replied that he had great respect for Marx's early writings, such as the **Contribution to the Critique of Political Economy**, **The Holy Family**, **The German Ideology**, and **Critique of Hegel's Philosophy of Law**, although in 1971 he himself had not read **Capital**. Nonetheless, he thought that he ought to read **Capital** afterwards, and planned to consult Albert about it. [39] Furthermore, Feyerabend pointed out that "Marx's comments on Hegel's **Philosophy of Law**" was an "ancestor" of his own view. [40] When it came to the problem of "alienation", he referred to Marx's "National Economy and Philosophy", his **Critique of Hegel's Philosophy of Law**, and the **German-French Yearbooks**. [41] As regards the "unevenness" of historical development, he cited both **The Poverty of Philosophy** and **Introduction to the Critique of Political Economy** by Marx, writing that "[a]ccording to Marx, »secondary« parts of the social process, such as demand, artistic production or legal relations, may get ahead of material production and drag it along". [42] In particular, he quotes the following passage from the **Introduction to the Critique of Political Economy**:

> The unequal relation between the development of material production and art, for instance. In general, the conception of progress is not to be taken in the sense of the usual abstraction. In the case of art, etc., it is not so important and difficult to understand this disproportion as in that of practical social relations, e.g. the relation between education in the U.S. and Europe. The really difficult point, however, that is to be discussed here is that of the unequal development of relations of production as legal relations. [43]

In general, Feyerabend referenced, quoted and discussed Marx in affirmative terms. That is to say, he was intellectually sympathetic to the latter. It was, according to Lakatos, precisely because Feyerabend admired Marx so enormously that Lakatos himself wrote in his letter to him: "Just imagine a statue of me to the right

[39] See Baum (ed.), **Paul Feyerabend...**, p. 227; Wilhelm Baum (ed.), Paul Feyerabend, Hans Albert: Briefwechsel Band 1 (1958–1971), Kitab, Vienna 2008, p. 271.

[40] See Feyerabend, **Science in a Free...**, p. 163.

[41] See Feyerabend, **Problems of Empiricism...**, p. 160.

[42] Paul K. Feyerabend, **Against Method: Outline of an Anarchistic Theory of Knowledge**, Verso, London 1979, p. 146. For a similar citation, see also: Paul K. Feyerabend, **Knowledge, Science and Relativism, Philosophical Papers, Volume 3,** John Preston (ed.), Cambridge University Press, Cambridge 1999, p. 168.

[43] Feyerabend, **Against Method**, pp. 146–147.

of Marx and a statue of you to his left in Highgate Cemetery". [44] However, while praising Marx, Feyerabend criticized the followers of Marx of his own time in the following terms: "His followers of today are uneducated barbarians […] As far as style is concerned, although somewhat unrefined, Papa Marx almost surpasses all of them. Marx's style is substantial, rich in content, interesting, not an insipid sauce…". [45] In a word, Feyerabend frequently extolled Marx while criticizing the Marxists and leftists that were his own actual contemporaries.

Feyerabend offered high praise to Engels. In a letter to Lakatos, he professed to be "the Engels of the twentieth century". "In 2300", he wrote, "when Popper will be known as the Kant, you as the Hegel and I as the Engels of the twentieth century, one will have to go to the darkest corners of libraries to find out about Cohen, Hesse, etc. etc.". [46] Arguing in support of dialectics, Feyerabend quoted four passages from the **Anti-Düring** by Engels. With these quotations, Feyerabend sought to expressed the following key ideas: Negation, accordingly, "does not mean simply saying No, or declaring a thing to be non-existent, or destroying it in any way one may choose". The "negation of the negation" is "extremely universal and just on that account extremely far-reaching and important law of development in nature, history and thought. […] Dialectics, however, is nothing else than the science of the general laws of motion and development in nature, human society and thought". [47]

Feyerabend clearly attached great importance to dialectical materialism and dialectics — the philosophy of Marx and Engels, including the notions of contradiction, "negation" and "the negation of the negation". Marx and Engels and their followers, as friends of science, believed in science, were for science, and based their theory and practice on science. As Feyerabend put it, "Marx and Engels were convinced that science would aid the workers in their quest for mental and social liberation". [48] Nonetheless, Feyerabend, as "the worst enemy of science," was against modern science. Thus, contemporary Marxists and leftists seemed dissat-

[44] Lakatos and Feyerabend, **For and Against Method…**, p. 259.

[45] Baum (ed.), **Paul Feyerabend…**, p. 227; Baum (ed.), **Paul Feyerabend, Hans Albert…**, p. 271.

[46] Lakatos and Feyerabend, **For and Against Method…**, p. 231.

[47] Feyerabend, **Problems of Empiricism…**, pp. 77–78.

[48] Feyerabend, **Science in a Free…**, p. 75; Feyerabend, **Thesen zum Anarchismus…**, p. 115; Feyerabend, **Knowledge, Science and Relativism**, p. 181.

isfied with Feyerabend. In this connection, Feyerabend wrote, "Marxists have been especially incensed at my mocking disregard for their two favorite playthings, Western science and humanitarianism". [49] It is obvious that Feyerabend's attitude to modern science ran contrary to that of Marx and Engels, even though he thought highly of them.

Feyerabend read a great deal of Lenin's writings, such as **Imperialism, Materialism and Empirico-criticism**, and **Notes on Philosophy**, and bought his **Collected Works** (45 volumes in all). [50] What is more, he read Lenin's letter (in his **Collected Works**) to his friend Inessa Armand (1874–1920), a female revolutionary, in class. [51] He praised Lenin as "a clever man" in his letter to Albert [52] and repeatedly quoted the following sentence from Lenin's "»Left-Wing« Communism, An Infantile Disorder": "History as a whole, and the history of revolutions in particular, is always richer in content, more varied, more multiform, more lively and ingenious than is imagined by even the best parties, the most conscious vanguards of the most advanced class". [53] Quoting this sentence, he aimed to transform Lenin's viewpoint on parties and revolutionary vanguards into that of himself on scientists and methodologists. Feyerabend also cited **"Left-Wing" Communism: An Infantile Disorder** and "Backward Europe and Advanced Asia", in order to address issues of "uneven historical development" and "liberty". [54]

In his article "Two Models of Epistemic Change" Feyerabend referenced and quoted Lenin, Mao and Hegel many times while discussing dialectical epistemology. The theme of this epistemology is presented in the following passage, this being a clear reference to Lenin's **Notes on Philosophy**:

> Knowledge is the eternal infinite approach of thought and object. The mirroring of nature in human thought is not "dead", it is not "abstract", it is not without motion, not

[49] Paul K. Feyerabend., **Farewell to Reason**, Verso, London 1988, p. 305.

[50] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 242.

[51] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 268.

[52] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 172.

[53] Feyerabend, **Against Method...**, p. 17; Feyerabend, **Knowledge, Science and Relativism**, p. 179; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 199.

[54] Feyerabend, **Against Method...**, p. 147; Feyerabend, **Knowledge, Science and Relativism**, p. 68; Feyerabend, **Philosophical Papers...**, p. 168.

> without its contradictions, but is to be conceived as an eternally moving process that gives rise to contradictions and removes them. [55]

The above-quoted passage is at the core of dialectical epistemology. Feyerabend glorified Lenin, especially in the fields of science and philosophy, asserting as he did that "[t]here are not many writers in the field today who are as well acquainted with contemporary science as was Lenin with the science of his time, and no one can match the philosophical intuition of that astounding author". [56] Thus, when it came to classifying various philosophers of science, Feyerabend thought that Lenin, Lakatos and he himself belonged to the same class. [57] Lenin had strongly attacked Ernst Mach (1838–1916) in **Materialism and Empirico-criticism**. In this connection, it is a nice irony that Feyerabend praised Mach highly, despite citing that book. [58]

Feyerabend dreamed about Stalin, though they never met. In his autobiography **Killing Time**, he wrote: "Years later I dreamt that I met Bohr again, that he recognized me and consulted me on important matters [...] On the other hand, I also dreamt that I advised Stalin, but I never met him". [59] Clearly, he was an admirer of Stalin. He read Stalin's little pamphlet on dialectical and historical materialism [60] and wrote: "Today Stalin's rules seem to me preferable by far to the complicated and epicycle-ridden standards of our modern friends of reason". [61] In other words, he much preferred Stalin's rules to the standards of critical rationalism — wondering, as he put it, "if (I), when back, still shall turn Marxist, and chop

---

[55] Feyerabend, **Problems of Empiricism...**, p. 79. Quoting the sentence from **Philosophical Notebooks** where Lenin writes that "[i]t is the crude, metaphysical, simplistic materialist who regards philosophical idealism as being merely nonsense," Feyerabend emphasized that the latter recognized "that idealism can have a positive function at certain periods of the history of our thought" (Paul K. Feyerabend, **Physics and Philosophy, Philosophical Papers, Volume 4**, Stefano Gattei and Joseph Agassi (eds.), Cambridge University Press, New York 2016, pp. 222–223.

[56] Feyerabend, **Physics and Philosophy...**, p. 219.

[57] See Lakatos and Feyerabend, **For and Against Method...**, p. 216.

[58] See Feyerabend, **Physics and Philosophy...**, p. 11.

[59] Paul K. Feyerabend, **Killing Time: The Autobiography of Paul Feyerabend**, The University of Chicago Press, Chicago 1995, p. 78.

[60] See Feyerabend, **Science in a Free...**, p. 112.

[61] Feyerabend, **Science in a Free...**, p. 113. For a similar idea, also see Augustin (ed.), **Aber ein Paul hilft doch dem anderen...**, p. 92.

all Popperian heads off from a Stalinist point of view". [62] Moreover, he outlined "epistemological Stalinism" in rough terms as follows:

> Another school, called epistemological Stalinism by Lakatos (in some of his talks, not in any one of his publications) assumes that the evaluation of theories depends on the judgment of some Great Man or of some Great Group: good theories are those theories which great scientists, or groups of great scientists say are good. [63]

According to Feyerabend, "epistemological Stalinism" is the same as or similar to political Stalinism: the evaluation of everything depends on the judgment of a great leader or group of great leaders. This means that "epistemological Stalinism" can be regarded as "epistemological totalitarianism", and shows that Feyerabend praised Stalin and Stalinism even though he did not accept dialectics and historical materialism.

Feyerabend adored Mao, and took a keen interest in Maoism and communist China. He had read a number of Mao's writings. He also read Edgar Snow's (1905–1972) **Red Star Over China**, and praised it as "a marvelously interesting book". [64] He wrote in a letter to his friend Albert: "Thus, I am a thousand times fonder of Mao than Popper, too". [65] He cited, quoted and discussed Mao and Maoism in **Against Method**, [66] **Problems of Empiricism (Philosophical Papers, volume 2)**, [67] **Science in a Free Society**, [68] **Three Dialogues on Knowledge**, [69] **Paul Feyerabend — Hans Albert Briefweichsel**, [70] and **For and Against Method**. [71] His

[62] Baum Wilhelm and Mühlmann Michael (eds.), **Paul Feyerabend, Hans Albert...**, p. 17.

[63] Paul K. Feyerabend, "Imre Lakatos", *The British Journal for the Philosophy of Science* 1975, Vol. 26, No. 1, p. 16 [1–18].

[64] Lakatos and Feyerabend, **For and Against Method...**, p. 277.

[65] Baum (ed.), **Paul Feyerabend...**,, p. 100; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 160.

[66] See Feyerabend, **Against Method...**

[67] See Feyerabend, **Problems of Empiricism...**

[68] See Feyerabend, **Science in a Free...**

[69] See Paul K. Feyerabend, **Three Dialogues on Knowledge**, Basil Blackwell Ltd, Oxford 1991.

[70] See Baum (ed.), **Paul Feyerabend...**; Baum (ed.), **Paul Feyerabend, Hans Albert...**; Baum and Mühlmann (eds.), **Paul Feyerabend...**

[71] See Lakatos and Feyerabend, **For and Against Method**... For more details, see: Feyerabend, **Against Method...**, p. 147; Feyerabend, **Problems of Empiricism...**, pp. 67–68; Feyerabend, **Science in**

citations and quotations were from Mao's articles "Oppose Stereotyped Party Writing", "On the Correct Handling of Contradictions among the People", "On Practice", and "On Contradiction". Generally speaking, Feyerabend was enlightened by Mao and Maoism in respect of two of his viewpoints: firstly, that pertaining to "state interference with science", in which he invoked the revival of traditional medicine in Mao's China as an example, and secondly, his thought that "citizens can and must supervise the expert and science", for which he took inspiration from Mao's writings.

In particular, he quoted the following passages from "On the Correct Handling of Contradictions among the People" in order to demonstrate that Mao was similar to the liberal thinker Mill:

> "Ideological struggle" [...] "is not like other forms of struggle. The only method to be used in this struggle is that of painstaking reasoning and not crude coercion". [...] "It is therefore necessary to be careful about questions of right and wrong in the arts and sciences, to encourage free discussion and avoid hasty conclusions". [...] "People may ask, since Marxism is accepted as the guiding ideology by the majority of the people in our country, can it be criticized? Certainly it can. [...] Marxists should not be afraid of criticism from any quarter. [...] What should our policy be towards non-Marxist ideas? [...] Will it do to ban such ideas and deny them any opportunity for expression? Certainly not. [...] Therefore, it is only by employing the method of discussion, criticism and reasoning that we can really foster correct ideas and overcome wrong ones, and that we can really settle issues". [...] The similarity to Mill, whom Mao read in his youth, is remarkable. [72]

However, it is also evident that he misunderstood Mao and Maoism. Mao wrote an article entitled "Combat Liberalism" in which he strongly opposed liberalism. [73] In his text "On the People's Democratic Dictatorship: In Commemoration of the Twenty-Eighth Anniversary of the Communist Party" he announced that

---

a Free Society, pp. 128 and 161–162; Feyerabend, **Three Dialogues on Knowledge...**, p. 88; Lakatos and Feyerabend, **For and Against Method**..., pp. 210, 218–219 and 330; Baum, **Paul Feyerabend...**, pp. 100-101, 104, 108 and 120; Baum (ed.), **Paul Feyerabend, Hans Albert...**, pp. 160–161, 167, 250 and 201.

[72] Feyerabend, **Problems of Empiricism...**, pp. 67–68; Mao Zedong, **Selected Works of Mao Tsetung**, **Volume V,** Foreign Language Press, Peking 1977, pp. 384-421)

[73] See Zedong Mao, **Selected Works of Mao Tsetung**, **Volume II,** Foreign Language Press, Peking 1975, pp. 31–33.

"we are dictatorial". [74] Of course, if we take into consideration the Anti-Rightist Struggle (1957), the Great Leap (the Great Famine, 1958–1961) and the Cultural Revolution (1966–1976), then it has to be said that Mao's deeds were very different from his words. As a result, Feyerabend was mistaken about Mao and Maoism.

Finally, Feyerabend also praised Fidel Castro. For his lecture in Yale, he bought a set of clothes (a US-army-jacket and a pair of stormtrooper trousers) of the kind worn by all anarchists — like Castro — at that time. [75] In addition, he watched a detailed report about Castro on TV, [76] and never criticized or sought to blame the latter.

2.2 Other Marxists and Leftists

Feyerabend quoted the following sentence from Trotsky's **The Revolution Betrayed**: "A political struggle is in its essence a struggle of interests and of forces, *not of arguments*". [77] In this respect, he fully endorsed that thinker, endorsing the slogan "Back to Marx!" and seeking to further extend "the struggle of interests and of forces" to astronomy, while implying that there were not only arguments, but also a struggle of interests and forces, at work in science. Moreover, he addressed the "unevenness" of historical development with the following quotation from Trotsky's "The School of Revolutionary Strategy" speech: "The gist of the matter lies in this, that the different aspects of the historical process — economics, politics, the state, the growth of the working class — do not develop simultaneously along parallel lines". [78] Moreover, he adduced both Bernstein and Luxemburg ("Luxemburg's reply to Bernstein's criticism of Marx or Trotsky's ac-

[74] See Zedong Mao, **Selected Works of Mao Tsetung**, **Volume IV,** Foreign Language Press, Peking 1975, p. 417.

[75] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 188.

[76] See Feyerabend, **Briefe an einen Freund**, p.118.

[77] Lakatos and Feyerabend, **For and Against Method...**, p. 151 [italics in the original].

[78] Feyerabend, **Against Method...**, p. 147; Feyerabend, **Knowledge, Science and Relativism**, p. 168; Feyerabend, **Physics and Philosophy...**, p. 245.

count of why the Russian Revolution took place in a backward country…"), as well as Benjamin, in his written works. [79]

It is also worth noting that in the course of his writings Feyerabend referred to a rat in his house as "Kautsky": [80] "Remembering that Lenin had called Kautsky (the Austrian socialist) a rat, Robin called my rat Kautsky". [81] Given that Kautsky had opposed Lenin, the Bolshevik putsch in Russia of October 1917, and revolutionary violence more generally, it is fair to conclude that Feyerabend was employing the nickname "Kautsky" for the "rat" as a way of singing Lenin's praises and showing disrespect for Kautsky.

Feyerabend regarded Brecht, who was a Marxist and an important artist, as "his hero". The following quotation from the latter is placed on page one of **Against Method**: "Ordnung ist heutzutage meistens dort, wo nichts ist. Es is eine Mangelerscheinung". [82] According to Feyerabend himself, he came to know Brecht through Hollitscher. Concerning his relation to Brecht, he wrote in his autobiography **Killing Time** as follows:

> We met Brecht at a rehearsal of **Die Mutter** with Helene Weigel in the title role. […] Brecht, Walter told me, was prepared to take me on as an assistant (in Berlin). I said no and stayed in Vienna. I once thought (and said so in print) that this was the biggest mistake of my life. Today I am not so sure. I would have liked to learn more about the theatre, and from such an extraordinary man. I would also have liked to get some training in forms of communication different from the scientific essay. But I suspect I would have detested the collective pressure of the partly fearful, partly dedicated, and certainly pushy and closely knit group that surrounded Brecht. [83]

There can be no doubt that Brecht exercised a great influence on Feyerabend. As he put it, "I have become very interested in problems of aesthetics, theoretical and applied. At some time I would like to publish something in this fascinating

[79] See Feyerabend, **Problems of Empiricism…**, p. 207; Feyerabend, **Briefe an einen Freund**, p. 184; Baum (ed.), **Paul Feyerabend, Hans Albert…**, p. 262.

[80] See Lakatos and Feyerabend, **For and Against Method…**, p. 201; Baum (ed.), **Paul Feyerabend, Hans Albert…**, p. 246.

[81] Feyerabend, **Killing Time…**, p. 113.

[82] Feyerabend, **Against Method…**, p. 1.

[83] Feyerabend, **Killing Time…**, p. 73. For "one of the biggest mistakes of his life," see Feyerabend, **Science in a Free Society**, p. 114. For "the tensions inside the Brecht Circle," see also Paul K. Feyerabend, **Against Method**, with an introduction by Ian Hacking, Verso, London 2010, p. 273.

field. I have been very much impressed by the ideas of Bertolt Brecht. Certainly, he is a Marxist". [84] Under the influence of latter, and especially of his **Über Lyrik**, he introduced methods from Brecht's theatre into his own theory of knowledge, stating that "[i]n my lectures on the theory of knowledge I usually present and discuss the thesis that finding a new theory for given facts is like finding a new production for a well-known play". [85] He published a review of Brecht's **Life of Galileo**, "Let's Make More Movies", in which he concluded that "there are better ways of dealing with philosophical problems than verbal exchange, written discourse, and, a fortiori, scholarly research". [86] That is to say, one should employ the entire spectrum of theories, books, images, emotions, sounds, institutions, etc., to address philosophical problems. In Feyerabend's view, Brecht's theatre was an attempt to do just that, although he did not completely succeed. Consequently, Feyerabend suggested that "we try movies instead". [87] Besides, he quoted a passage from Brecht's **Schriften zur Literatuer und Kunst** in his letter to Lakatos, [88] and also mentioned Brecht in other places. [89] More particularly, he actually presented lectures on Brecht. [90]

Hollitscher was a Marxist, a member of the Central Committee of the Communist Party of Austria, a philosopher, a publicist, and a psychoanalyst. He was one of Feyerabend's closest friends. In this connection, Feyerabend wrote in his letter to an editor a few months before his death that "Walter Hollitscher was one of my best friends from 1950 till his death, and we have discussed basic philosophical problems year in year out". [91] Of course, their discussions were centered around dialectical and historical materialism. As Feyerabend put it,

---

[84] Matteo Collodel and Eric Oberheim (eds.), **Feyerabend's Formative Years (Vol. 1. Feyerabend and Popper, Correspondence and Unpublished Papers)**, Springer Nature Switzerland AG, Cham 2020, p. 368.

[85] Feyerabend, **Problems of Empiricism...**, p. 161.

[86] Feyerabend, **Knowledge, Science and Relativism**, p. 195.

[87] Feyerabend, **Knowledge, Science and Relativism**, p. 199.

[88] See Lakatos and Feyerabend, **For and Against Method...**, p. 212.

[89] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, pp. 258 and 271–272; Lakatos and Feyerabend, **For and Against Method...**, p. 233.

[90] See Baum (ed.), **Paul Feyerabend, Hans Albert...,** p. 160.

[91] Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 19.

> From the very beginning of our discussion, Hollitscher made it clear that he was a communist, and that he would try to convince me of the intellectual and social advantages of dialectical and historical materialism. […] Nor did Hollitscher use unfair emotional or intellectual pressures. Of course, he criticized my attitude and he still does, but our personal relations have not suffered from my reluctance to follow him in every respect. This is why Walter Hollitscher is a teacher while Popper whom I also came to know quite well is a mere propagandist. [92]

In the above quotation, we can see that Feyerabend valued Hollitscher very highly as a teacher, but decried Popper as a mere propagandist. Nevertheless, in fact Popper had been his teacher, not Hollitscher. This shows that Hollitscher exerted an important and positive influence on Feyerabend. For example, his conversion from positivism to realism was partly attributed to "discussions he had with Hollitscher about philosophy and scientific practice". [93] Hollitscher retained a firm belief in Marxism and communism, while committed liberals thought that he "was beyond the pale: an intellectual who had become a slave of totalitarianism". [94] For Feyerabend, however, he remained a wonderful, gentle, humane friend.

Through Hollitscher, Feyerabend came to know Hanns Eisler. He said of the latter that he "accompanied me singing Schumann and some of his marching songs…". [95] Eisler was a German-Austrian-American composer, music theoretician, and lyricist, and also a co-worker of Brecht. As a communist, he was expelled from America in 1948. Feyerabend mentions him in other places, too. [96]

Feyerabend read **Die Zerstörung der Vernunft** by Georg Lukacs during the period when he was engaged in translating Popper's **The Open Society and Its Enemies** from English into German. He talks about the book in his letter to Popper in the following terms:

> I happened to get hold of a very interesting and highly challenging book […] namely, **Die Zerstörung der Vernunft** by Georg Lukacs, the Marxist. Perhaps you have al-

[92] Feyerabend, **Science in a Free…**, p. 114.

[93] Kent, "Paul Feyerabend and the Dialectical Character of Quantum Mechanics…", p. 53 [51–57]. For Feyerabend's account, see Feyerabend, **Science in a Free Society**, pp. 113–114.

[94] Feyerabend, **Killing Time…**, p. 73.

[95] Feyerabend, **Killing Time…**, p. 73.

[96] See Feyerabend, **Science in a Free…**, p. 112; Feyerabend, **Killing Time…**, p. 60.

> ready seen it (subtitle: **Der Weg des Irrationalismus von Schelling zu Hitler**). There are some (or even many) similarities to some of your ideas in the **Open Society** [...] I think it would be a very good thing if some footnote containing your ideas about this book would be added somewhere in this translation. [97]

For Feyerabend, Lukacs' work was so interesting and challenging that he was driven to suggest that Popper add some footnotes about it to the translation of **The Open Society and Its Enemies**. It is thus obvious that Feyerabend valued **Die Zerstörung der Vernunft**, a work by someone who was one of the founders of Western Marxism. [98] In addition, Feyerabend cited Lukacs' **Der Junge Hegel**. [99]

Feyerabend also admired the Western Marxist Ernst Bloch, writing that "I admire Ernst Bloch because he speaks with the tongue of the common people and enhances the colorful accounts they and their poets have given of life". [100] Feyerabend respected Bloch because he was "a philosopher of the common people". For this reason, he experienced real grief over Bloch's death. As he put it, "Bloch's death made me very grievous. I have much liked him and his philosophy. He is a lot better than Althusser, the insipid joke". [101] Here he praises Bloch while taking a critical stand against Althusser. Nonetheless, he blamed Bloch for having "sucked the blood of the youth". [102]

Marcuse was an important Western Marxist. His theory seemed so similar to Maoism that, in a kind of fusion with Mao, he was referred to as "Mao-ku-sung or Maokuse" by Albert. [103] Feyerabend found Marcuse's **Vernunft und Revolution** to be the only good book among Marcuse's works, and an excellent introduction to Hegel's writings at a time when he was studying more and more of the latter's philosophy. [104] At one point he was preparing to write a chapter on "anti-Marcuse" (or the poverty of Marcuse, entitled "The Testament of Dr. Marcuse") in his book

---

[97] Collodel and Oberheim (eds.), **Feyerabend's Formative Years...**, p. 175.

[98] See For another citation, see Feyerabend, **Physics and Philosophy...**, p. 219.

[99] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 129.

[100] Feyerabend, **Three Dialogues on Knowledge...**, p. 122.

[101] Baum Wilhelm and Mühlmann Michael (eds.), **Paul Feyerabend, Hans Albert...**, p. 172.

[102] Baum (ed.), **Paul Feyerabend, Hans Albert...,** p. 130.

[103] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 110.

[104] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 156.

Against Method. [105] Even so, it can seem from that same book that Feyerabend valued Marcuse. His friend Agassi commented on Feyerabend that "[h]e sounds super-revolutionary, in politics as well as in methodology; he also practically equates the two and makes Lenin the greatest methodologist of them all (p. 17n and elsewhere). He means Herbert Marcuse, but he says Lenin". [106] That is to say, Agassi thought that Feyerabend attached the same importance to Marcuse and Lenin.

In fact, though, there is no such chapter in **Against Method**, where instead we only find Feyerabend quoting a sentence from Marcuse's **Reason and Revolution**. [107] Indeed, Feyerabend frequently criticized Marcuse. [108] He said: "For me, the New Left is simply too stupid, and Marcuse is an old obscurant". [109] In particular, he offered a serious criticism of the following statement from Marcuse's "Repressive Tolerance": "Marcuse's case is quite interesting. […] One wonders why he prefers to use an imaginary power which he does not yet possess but which he (or his wife) would certainly like to have, for suppressing opponents rather than for education and a more balanced discussion of views". [110] So, Feyerabend was definitely not willing to stand by Marcuse. As he put it,

> In a pamphlet with the title "*Stalinismus und Anarchismus in der Spanischen Revolution*", I am referred to as "*der Amerikanische Oberdada Paul Feyerabend*," and there is a long discussion of "*das negative Element in Feyerabend und Marcuse*". If things continue like that I shall be Marcuse's successor with the New Left of 1980 — not an attractive prospect. [111]

[105] See Lakatos and Feyerabend, **For and Against Method…**, pp. 183–185; Baum (ed.), **Paul Feyerabend, Hans Albert…**, pp. 227–228.

[106] Agassi, "Review Essay of **Against Method…**", p. 166 [165–191].

[107] See Feyerabend, **Against Method…**, p. 27.

[108] See, e.g., Baum (ed.), **Paul Feyerabend, Hans Albert…**, p. 278.

[109] Baum (ed.), **Paul Feyerabend, Hans Albert…**, p. 247.

[110] Feyerabend, **Problems of Empiricism…**, p. 68. Similarly, Mao said: "suppress them (the enemy), allow them only to behave themselves and not to be unruly in word or deed. If they speak or act in an unruly way, they will be promptly stopped and punished. Democracy is practiced within the ranks of the people" (Mao, **Selected Works of Mao Tsetung…**, p. 418). So quite to the contrary, Feyerabend praised Mao as a liberal similar to Mill.

[111] Lakatos and Feyerabend, **For and Against Method…**, p. 336 [italics and quotation marks in the original].

To a degree, Feyerabend opposed Marcuse. He was not happy to "be Marcuse's successor with the New Left of 1980". For him, the "prospect" did not seem attractive. He wanted to argue against Marcuse rather than on his behalf.

Feyerabend believed that Adorno had no sense of humor. [112] What is more, he made the following comment about the latter:

> Adorno — I have read him. In my opinion, his writings are bad. They are German? Where do the sentences begin? And where do they end? He should sing them in an opera if he has a voice. They are very melodic, but there is little content in them (as in all opera texts). The man is probably smart, and some of his comments are very good, just not so good, that it is worth any trouble, to select them from the pile of nonsense (pardon!). [113]

In general, Feyerabend offered criticisms rather than praise where Adorno was concerned. For instance, in his letter to Albert, one of his friends, he told him that "Adorno was interrupted in his lecture by a bare-breasted female student". [114] This shows that he did not respect Adorno. Similarly, Albert strongly criticized Adorno, and thought that his writings were extremely unclear, unnatural and affected. [115]

In 1982 Feyerabend was invited to a dinner with Habermas, [116] even though he was unwilling either to meet him or attend his lecture. [117] He showed disrespect for the latter, referring to him as "livestock", "that seemingly schizophrenic German philosopher", and "Habermasochismus". [118] As he put it, "I do not like Habermas. I can read nothing of his writings. His style doesn't suit me". [119] He even

[112] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 40.

[113] Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 37.

[114] Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 176.

[115] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 38.

[116] See Feyerabend, **Briefe an einen Freund...**, p. 212.

[117] See Feyerabend, **Briefe an einen Freund...**, pp. 85 and 205; Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert...**, p. 251.

[118] See Feyerabend, **Briefe an einen Freund...**, p. 205; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 136; Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert...**, p. 27.

[119] Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert...**, p. 27.

asserted that "neither Popper nor Habermas could write in German". [120] To some extent, his criticisms of Habermas were relevant to his friends Hans Albert and Hans Duerr (1942–). For instance, Duerr dubbed Habermas "an armchair-fart", [121] while Albert considered the Frankfurt School "paranoid". [122] In his letter to Albert, Feyerabend wrote the following satirical poem, mocking Habermas:

> Heintel and Gabriel
> Who feel very weak
> They shout at Habermas
> And plead "help us!"
> Habermas with his last ounce of strength
> Groans only "I am beaten
> I have Albert in my neck
> And Spinnerich in my stomach". [123]

In the above quotation, it is notable that Feyerabend is criticizing Habermas through satire. Nonetheless, he did occasionally put in a good word for him. In his letters to his friends, he emphasized that Habermas was not "stupid" [124] and, where **Against Method** was concerned, would "even invite Habermas to write the introduction to the Suhrkamp edition". [125] He also read Habermas' **Erkenntnis und Interesse** and remarked that it was "not bad". [126] Besides, in Berlin Feyerabend and Hübner held a Popper seminar, which Habermas regularly showed up at. [127]

[120] Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 247.

[121] See Feyerabend, **Briefe an einen Freund...**, p. 207.

[122] See Baum Wilhelm and Mühlmann Michael (eds.), **Paul Feyerabend, Hans Albert...**, p. 46.

[123] Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 64.

[124] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, pp. 114 and 149.

[125] Lakatos and Feyerabend, **For and Against Method...**, p. 257.

[126] Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 149.

[127] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 149.

Feyerabend held that Althusser, among "progressive" leftists, was one of the best followers of Marx of his time. He mentioned or cited him, and his work **For Marx**, in his books.[128] Generally speaking, though, he criticized rather than praised Althusser. As he put it, "Althusser, one of the best contemporary followers of Marx, whom I have read a little, produces more intellectual gossip, but represents the high point in the general literary misery today".[129] The two Australian Marxist philosophers Curthoys and Suchting, reviewing the **Against Method** from a Marxist point of view, were Althusser's students. For them, the Marxist point of view in fact stemmed from Althusser. In their words: "Our argument will be undertaken from the perspective of a Marxist theory of knowledge, one which stems in fact from the same Louis Althusser whom Feyerabend has described as intellectually medieval".[130] In the quoted passage they make it clear that they think Feyerabend had criticized Althusser as "intellectually medieval". Accordingly, they put forward a number of severe counter-criticisms of Feyerabend's critical points. On the other hand, Althusser himself was dissatisfied with their criticisms of Feyerabend's **Against Method**, telling Suchting "Was it necessary, to disgrace me so?".[131]

Needham was a British biochemist and sinologist affiliated with the left, and more particularly, a historian of science and technology as these pertained to traditional China. Feyerabend mentioned, referenced or quoted him in his writings.[132] His knowledge of Chinese science and technology was acquired mainly through reading Needham's works, such as **Science and Civilization in China, Science in Traditional China, and Celestial Lances**. As he put it, "I don't know Chinese. I haven't seen the relevant evidence. I only read a few books, some volumes of Needham's monstrous work on Chinese science included, and this is what they say".[133] It could be that he derived the following ideas from Needham:

[128] See Feyerabend, **Against Method...**, p. 147; Feyerabend, **Science in a Free Society**, pp. 166–167.

[129] Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 271.

[130] Curthoys and Suchting, "Feyerabend's Discourse against Method...", p. 266.

[131] Baum Wilhelm and Mühlmann Michael (eds.), **Paul Feyerabend, Hans Albert...**, p. 186.

[132] See Feyerabend, **Farewell to Reason...**, pp. 24, 38 and 88; Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction versus The Richness of Being,** Bert Terpstra (ed.),The University of Chicago Press, Chicago 1999, p. 131; Paul K. Feyerabend, **The Tyranny of Science**, edited, and with an introduction, by Eric Oberheim, Polity Press, Cambridge 2011, p. 76.

[133] Feyerabend, **The Tyranny of Science...**, p. 76.

> … [I]ndeed, Chinese technology, medicine included, was for a long time far ahead of the West. […] When the "scientific revolutions" of the sixteenth and seventeenth centuries started in the West, Western technology was rather primitive compared with Chinese technology. [134]

Although such notions were and still are highly debatable, Feyerabend criticized modern science and Western civilization on the basis of them, praising traditional China and its science, medicine and technology.

Sartre had a very firm belief in communism. In 1952 he remarked, crudely, that "any anti-communist is a dog!". [135] Feyerabend read his autobiography (**Le mots**) and liked it. [136]

Cohn-Bendit was the leader of the French student revolt of May 1968. It is very clear that the title of his book **Obsolete Communism: The Left-Wing Alternative**, bears a resemblance to that of Lenin's **"Left-Wing" Communism, an Infantile Disorder**. Cohn-Bendit had such a significant impact on Feyerabend that the latter studied the book in depth and began his lecture on the philosophy of science at Yale by reading aloud a page from the book. [137] Feyerabend vividly described his impact as follows:

> I have finished Cohn-Bendit, and am *wholly* on his side. He is against theories; so am I He is against organizations; so am I. He is against "leaders", be they professors who "know", or generals who command; so am I. He is *for joy* and *against* sacrifice; so am I: "The real meaning in revolution is not a change in management, but a change in man. This change we must make in our own lifetime and not for our children's sake, for the revolution must be born of joy and not of sacrifice". [138]

Hence, as Agassi claimed, "He (Feyerabend) has predecessors […] in politics (i.e. Danny the Red)…". [139] Feyerabend used many viewpoints of Cohn-Bendit in

---

[134] Feyerabend, **The Tyranny of Science…**, p. 76.

[135] Stéphane Courtois, Nicholas Werth, Jean-Luis Panné, Andrzej Paczkowski, Karel Bartošek, and Jean-Luis Margolin, **The Black Book of Communism: Crimes, Terror, Repression**, Mark Kramer (ed.), trans. Jonathan Murphy and Mark Kramer, Harvard University Press, Cambridge — London 1999, p. 750.

[136] See Baum Wilhelm and Mühlmann Michael (eds.), **Paul Feyerabend, Hans Albert…**, p. 186.

[137] See Baum (ed.), **Paul Feyerabend, Hans Albert…**, pp. 149 and 190.

[138] Lakatos and Feyerabend, **For and Against Method…**, p. 152 [italics in the original].

[139] Agassi, "Review Essay of **Against Method…**", p. 166 [165–191].

his writings: for instance, the link between theory and politics, the emphasis on action within a libertarian framework, the demand for flexibility and a democratic basis for all institutions, the opposition to specialists and specialization, and the struggle against any kind of hierarchy and bureaucracy, including that in educational institutions, schools and universities. [140] Furthermore, he claimed to combine the ideas of Mill and Cohn-Bendit: "It seems to me that the best starting point in our attempt to remove the still existing fetters to thought and action is a combination of Mill's general ideas and of a practical anarchism such as that of Cohn-Bendit". [141] Here, he considered Cohn-Bendit to be an anarchist. Nevertheless, elsewhere he held that Cohn-Bendit was a Dadaist, not an anarchist — insofar as Dadaism was better than anarchism. [142] In short, Feyerabend was against law and order, not only in society, but also in the philosophy of science. He concluded that "I will do for the philosophy of science what Cohn-Bendit (a good example) will do for society". [143]

Blackburn, as a leftist, sided with the students during the protests of 1968 and 1969, was expelled from the London School of Economics in 1969, and visited China in 1972. Feyerabend published his **Against Method** and **Science in a Free Society** with New Left Books (now Verso) while Blackburn was editor of the New Left Review and director of New Left Books itself. Feyerabend "preferred the New Left Books, but did not like the idea of having anything published with an academic publisher". [144] In his opinion, "the New Left Books, whatever else the disadvantages may be, are a nice group", and "they (Robin and Branka) seem to be nice people". [145] However, Feyerabend was angered that the New Left Books had changed the manuscript of **Against Method**. He wrote in his letters to Lakatos that "[t]hese bastards from the New Left have changed my style", and that "I am seriously considering taking the MS (AM) away from them (New Left Books) and

[140] See Lakatos and Feyerabend, **For and Against Method…**, p. 154; Feyerabend, **Problems of Empiricism…**, p. 66.

[141] Feyerabend, **Problems of Empiricism…**, p. 66.

[142] See Baum (ed.), **Paul Feyerabend, Hans Albert…**, p. 279.

[143] Feyerabend, **Briefe an einen Freund**, p. 11.

[144] Lakatos and Feyerabend, **For and Against Method…**, pp. 222 and 225.

[145] Lakatos and Feyerabend, **For and Against Method…**, pp. 222 and 225.

giving it to CUP [Cambridge University Press]". [146] He complained in a letter to Albert that "[t]he left are as bad as the right". [147]

2.3 Concluding Comments: Feyerabend as a Dadaist in Philosophy

To sum up, Feyerabend sided with the classical Marxists, Trotsky, Hollitscher and Cohn-Bendit, but argued against most of the other Marxists and leftists. This then poses a critically important question: was he himself a Marxist? Of course it was, and still is, an extremely controversial matter. Feyerabend was variously regarded as a fascist, Marxist, anarchist and anarcho-fascist. [148] In particular, Agassi held that Feyerabend had converted to Trotskyism, a form of Marxism: "Nevertheless, somehow he got converted to Trotskyism, from which he was never freed though he managed to put it aside and, while a disciple of Popper, even expounded rather anti-Trotskyite views". [149] In striking contrast to this, the two Australian Marxists Curthoys and Suchting considered Feyerabend an anti-Marxist — one who belonged to a peripheral (in class-struggle terms) grouping of parasitic intellectuals, while being committed epistemologically to extreme empiricism, subjectivism and skepticism, and ethico-politically to extreme individualism and liberalism, of a kind profoundly hostile to socialism. [150]

We may, moreover, add to this the fact that in his letters Feyerabend made such declarations to his friends as that "I have been an atheist for a long time", [151]

[146] Lakatos and Feyerabend, **For and Against Method…**, pp. 292 and 294.

[147] Wilhelm Baum and Michael Mühlmann (eds.), **Paul Feyerabend, Hans Albert…**, p. 29.

[148] See Lakatos and Feyerabend, **For and Against Method…**, p. 229; Feyerabend, **Briefe an einen Freund**, pp. 22, 182, 229, 230 and 231. Concerning neo-fascism, Feyerabend said, in his letter to Duerr: „lange lebe der Neofaschismus der Traumzeit long live the neo-fascism of dream time" (Feyerabend, **Briefe an einen Freund**, p. 144).

[149] Joseph Agassi, "As You Like", in: Gonzalo Munévar (ed), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, p. 383 [379–387].

[150] See Curthoys and Suchting, "Feyerabend's Discourse against Method…", pp. 337–338.

[151] Feyerabend, **Briefe an einen Freund…**, p. 102.

that "I am also a Catholic", [152] and that "the Jesuits will award me an honorary doctorate". [153] Feyerabend also seemed to be a conservative, [154] and it is noteworthy that he sometimes supported the Roman Catholic Church. As he put it, "[t]oday, in my lecture, I defended the Church's stand for law and order against the anarchist Galileo, and convinced everyone that Galileo was a bastard while Bellarmino was wise, scientific, humanitarian, etc. etc.". [155] Nevertheless, in **Against Method** he supported Galileo against the Church. This suffices to show in a full and rich way that Feyerabend was a very complex and vague figure, not only in respect of his philosophy, but also in terms of his lifestyle. It is for this reason that Lakatos saw fit to write in his letter to Feyerabend that "[i]f you do so, I shall distinguish Feyerabend$_0$, Feyerabend$_1$, Feyerabend$_2$, Feyerabend$_3$...". [156] Did Feyerabend adhere to any belief in Marxism, anarchism, fascism, individualism, liberalism, empiricism, subjectivism, skepticism, atheism, conservatism or Catholicism? No, he never allied himself enduringly to any one belief, with the exception of Dadaism. Consequently, Feyerabend not only took an interest in Marxism, applying it at the level of both theory and practice, but also embraced Dadaism — at least in philosophy, and to the extent that Dadaism itself has close links with Marxism and communism. This conclusion will be explored in greater detail in the next section.

3. Feyerabend on Dadaism and Dialectical Materialism

Feyerabend preferred to use the term "Dadaism" instead of "anarchism" to characterize his approach — especially where his epistemology (theory of knowledge) was concerned. His epistemology was transformed from an anarchistic one into a Dadaistic one. Thus, he proffered and pursued many comments and discussions on issues connected with Dadaism, Marxism, communism, anarchism and liberalism. What is more, he devoted some time and energy to materialism, dialectical materialism, reductive materialism and, especially, eliminative materialism.

[152] Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert...**, p. 238.

[153] Baum (ed.), **Paul Feyerabend...**„ p. 99; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 159.

[154] See Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert...**, p. 202.

[155] Lakatos and Feyerabend, **For and Against Method...**, p. 333.

[156] Lakatos and Feyerabend, **For and Against Method...**, p. 296.

As a Dadaistic philosopher, he never converted to dialectical materialism, even though Dadaism seems highly relevant to Marxism and communism. He might, conceivably, have been either for or against dialectical materialism. In conclusion, and taking into consideration his firm belief in Dadaism, we may say that he could quite possibly have been a Marxist, a non-Marxist, or an anti-Marxist. And, of course, as a Dadaist in philosophy he would also have had to be an anti-Dadaist in that very same regard.

3.1 Anarchism, Dadaism, Communism and Liberalism

In his book **Against Method: Outline of an Anarchistic Theory of Knowledge**, Feyerabend set a high value on anarchism, as the following quotations make clear:

> Science is an essentially anarchistic enterprise: theoretical anarchism is more humanitarian and more likely to encourage progress than its law-and-order alternatives. [157]

> The following essay is written in the conviction that anarchism, while perhaps not the most attractive political philosophy, is certainly excellent medicine for epistemology, and for the philosophy of science. [158]

Equally, though, he conveyed a dislike for anarchism in that very same work. As he put it, "…[h]owever, anarchism […] has features I am not prepared to support. It cares little for human lives and human happiness […] contains precisely the kind of Puritanical dedication and seriousness which I detest. […] I now prefer to use the term *Dadaism*". [159] Feyerabend gave up anarchism because it bored him. [160] It was said of him that, for Feyerabend himself, "anarchism is just another passing stage in his life". [161] He would have liked to become a flippant Dadaist:

[157] See Feyerabend, **Against Method. Outline…**, p. 17.

[158] Feyerabend, **Against Method. Outline…**, p. 17.

[159] Feyerabend, **Against Method…**, p.21; Lakatos and Feyerabend, **For and Against Method…**, pp. 294–295.

[160] See Feyerabend, **Thesen zum Anarchismus…**, p. 20; Lakatos and Feyerabend, **For and Against Method…**, pp. 311 and 362; Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert…**, p. 53.

[161] Lakatos and Feyerabend, **For and Against Method…**, p. 323.

"I hope that having read the pamphlet the reader will remember me as a flippant Dadaist and not as a serious anarchist". [162] Accordingly, he chose the term "Dadaism" rather than "anarchism" for his enterprise, especially for his epistemology.

"In a pamphlet with the title «*Stalinismus und Anarchismus in der Spanischen Revolution*» [Stalinism and Anarchism in the Spanish Revolution], [...] [Feyerabend was] referred to as «*der Amerikanische Oberdada Paul Feyerabend*» [the American Chief of Dada]".[163] Agassi emphasized that Feyerabend had a predecessor in aesthetics in the form of John Cage (1912–1992), [164] who as an American composer belonged to the avant-garde, including Dadaism. Feyerabend studied Dadaism and was keen on it. In this respect, he wrote: "I had studied Dadaism after the Second World War. What attracted me to this movement was the style its inventors used when not engaged in Dadaistic activities. It was clear, luminous, simple without being banal, precise without being narrow; it was a style adapted to the expression of thought as well as of emotion. I connected this style with the Dadaistic exercises themselves". [165] Feyerabend was such a highly original thinker that he could apply Dadaism to his philosophy, and especially his theory of knowledge, to create his very own Dadaistic philosophy or epistemology.

Feyerabend looked to achieve in philosophy, and especially in epistemology, what Marcel Duchamp (1887–1968), a leading figure of the Dada movement, had accomplished in art. [166] He quoted the following two slogans from the excellent textbook for Dadaistic science **Dada: Art and Anti-Art**, by Hans Richter (1888–1976), a Dadaist and German artist: "Dada not only had no programme, it was

[162] Feyerabend, **Against Method. Outline...**, p.21; Lakatos and Feyerabend, **For and Against Method...**, p. 295) For Feyerabend, perhaps, "a flippant anarchist" was the same as or similar to "a flippant Dadaist." He pointed out that "[i]ncidentally — a flippant anarchist, a really flippant anarchist is of course also prepared to engage in a rational debate and to defend Spiro Agnew. It is only the mechanical anarchist who is either too shortsighted, or too cowardly to do such things" (Lakatos and Feyerabend, **For and Against Method...**, p. 210. Generally, in his writings he did not distinguish "a flippant anarchist" from "a flippant Dadaist", but rather from "a mechanical anarchist" or "a serious anarchist".

[163] See Lakatos and Feyerabend, **For and Against Method...**, p. 336 [italics and quotation marks in the original].

[164] See Agassi, "Review essay of **Against Method...**", p. 166 [165–191].

[165] Feyerabend, **Science in a Free...**, p. 120; Feyerabend, **Against Method...**, p. 279.

[166] See Feyerabend, **Thesen zum Anarchismus...**, p. 12.

against all programmes", and "To be a true Dadaist, one must also be an anti-Dadaist". [167] Feyerabend offered the following general reflections on his Dadaistic epistemology and/or epistemological Dadaism:

> There is no view, however "absurd" or "immoral", he (the epistemological Dadaist) refuses to consider or to act upon, and no method is regarded as indispensable. The one thing he opposes positively and absolutely are universal standards, universal laws, universal ideas such as "Truth", "Reason", "Justice", "Love", and the behavior they bring along, though he does not deny that it is often good policy to act as if such laws (such standards, such ideas) existed, as if he believed in them. He may approach the religious anarchist in his opposition to science and the material world, he may outdo any Nobel Prize winner in his vigorous defence of scientific purity. [168]

The above quotation shows that his Dadaistic epistemology actually signifies having no epistemology at all. [169] To be a true Dadaistic epistemology, it must also be an anti-Dadaistic epistemology: on the one hand, "No prohibition!" or "Anything goes!"; on the other hand, "No permission!" or "Nothing goes!". The aims and objectives of this epistemology are to challenge and overturn the dominant orthodoxy of Western philosophy or (especially) epistemology, in that the core of Dadaism is to repudiate, mock and overturn artistic and social conventions in the West.

Dadaism has clear links with Marxism and communism, as all of them aim to protest or struggle against or to overthrow Western capitalism. As Jack Flam (1940) put it, "the core of Dadaism was based on what might be called an absurdist spirit, which was itself based upon a wholehearted and unremitting attack on all the norms of industrial-age bourgeois culture: social, ethical, political, artistic, and philosophical – a kind of guerilla warfare against the Establishment". [170] Dadaists enjoyed some level of association with Lenin when he was in exile in Zurich, [171] and the movement was closely connected to the Soviet Union. Tristan

[167] Feyerabend, **Against Method...**, pp. 33 and 189; Feyerabend, **Thesen zum Anarchismus...**, pp. 22–23; Lakatos and Feyerabend Paul, **For and Against Method**, pp. 114–115.

[168] Feyerabend, **Against Method...**, p. 189; Feyerabend, **Thesen zum Anarchismus...**, p. 23; Lakatos and Feyerabend Paul, **For and Against Method**, p. 115.

[169] See Feyerabend, **Briefe an einen Freund**, p. 12.

[170] Robert Motherwell, **The Dada Painters and Poets: An Anthology**, Second Edition, Belknap Press, Cambridge 1981, p. xii.

[171] See Motherwell, **The Dada Painters and Poets...**, p. xxiv.

Tzara (1896–1963, a Romanian artist and founder of Dada) said: "The Russian Revolution was saluted by some among us as a window opened upon the future, a breach in the fortifications of an outmoded civilization". [172] Meanwhile, Robert Motherwell (1915–1991) stated that "Indeed, I believe that present view of Dada as a historical movement held by each of the dadas is in every case somewhat colored by his present sympathy for or antagonism to the U.S.S.R.". [173] In particular, Dadaism abandoned art completely, and was turned into a political movement in Germany. As Richard Huelsenbeck (1892–1974) put it, "Dada is German Bolshevism. The bourgeois must be deprived of the opportunity to »buy up art for his justification«". [174] In the program of German Dada drawn up by Richard Huelsenbeck and Raoul Hausmann (1886–1971) entitled "What Is Dadaism and What Does It Want in Germany", from which the following key sentences are excerpted, Dadaism comes across as practically identical to communism and Marxism:

> **What is Dadaism and what does it want in Germany?**
>
> 1. Dadaism demands:
>
> 1) The international revolutionary union of all creative and intellectual men and women on the basis of radical Communism; [...]
>
> 3) The immediate expropriation of property (socialization) and the communal feeding of all; [...]
>
> 2. The Central Council demands: [...]
>
> b) Compulsory adherence of all clergymen and teachers to the Dadaist articles of faith;
>
> c) The most brutal struggle against all directions of so-called "workers of the spirit" (Hiller, Adler), against their concealed bourgeoisism [...];
>
> d) [...] the concept of property is entirely excluded from the super-individual movement of Dadaism which liberates all mankind;
>
> e) Introduction of the simultaneist poem as a Communist state prayer; [...]
>
> h) Immediate organization of a large scale Dadaist propaganda campaign with 150 circuses for the enlightenment of the proletariat [...]. [175]

[172] Motherwell, **The Dada Painters and Poets...**1981, p. 403.

[173] Motherwell, **The Dada Painters and Poets...**, p. xviii.

[174] Motherwell, **The Dada Painters and Poets...**, p. 44.

[175] Motherwell, **The Dada Painters and Poets...**, pp. 41–42.

From the first German Dadaist manifesto it is very evident that the Dadaistic movement in that country amounted to a form of radical communism — one that was directed against the bourgeoisie, supported the proletariat, and was aimed at destroying capitalist society in order to liberate all mankind and ultimately establish communism all over the world. Nevertheless, the fact that communism and Marxism exercised this level of influence upon Dadaists has been largely overlooked. It is surprising, and noteworthy, that Feyerabend himself did not mention it. However, it is very clear that Dadaism, Marxism and communism all had a great impact on Feyerabend himself. Dadaism, and with this communism, was in favor of evil or badness, and against the good, inasmuch as it held that these were really the same. As Huelsenbeck put it:

> Consequently, the good is for the Dadaist no "better" than the bad — there is only a simultaneity, in values as in everything else. This simultaneity applied to the economy of facts is communism, a communism, to be sure, which has abandoned the principle of "making things better" and above all sees its goal in the destruction of everything that has gone bourgeois. […] "Evil" has a profound meaning, the polarity of events finds in it a limit, and though the real political thinker (such as Lenin seems to be) creates a movement, i.e., he dissolves individualities with the help of a theory, he changes nothing. And that, as paradoxical as it may seem, is the import of the Communist movement. [176]

According to the quotation, for the Dadaists there was only a simultaneity; this simultaneity was communism, which had abandoned the principle of "making things better" and would completely destroy the capitalist system. From the perspective of the Dadaists or the communists, there were neither universal standards, nor any distinction between good and evil, nor "improvement". Likewise, Feyerabend defended Lenin, Stalin, Mao and Hitler (1889–1945), but attacked Alexander Solzhenitsyn (1918–2008), the dissident writer and Nobel laureate in literature exiled from the Soviet Union, as well as Lizhi Fang (1936–2012), a leading astrophysicist and political dissident from China, and Claus von Stauffenberg (1907–1944). "Stauffenberg", he said, "who tried to kill Hitler, was a terrorist, though unfortunately an unsuccessful one". [177] By contrast, he emphasized that his

[176] Motherwell, **The Dada Painters and Poets...**, p. 42.

[177] Paul K. Feyerabend, "Concluding Unphilosophical Conversation", in: Gonzalo Munévar (ed), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, p. 520 [487–527]. For a similar idea, see also Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert...**, p. 197.

love would be capable of including Hitler. [178] Feyerabend exalted Mao, but criticized Fang for his viewpoints and argumentation in his own writings. [179] Similarly, Feyerabend praised and admired Lenin and Stalin, but was disgusted with Solzhenitsyn, even though he had only seen him on television. [180] Given the simultaneity or communism of the Dadaists, the above viewpoints of Feyerabend are perfectly understandable, although they can certainly seem absurd, immoral and astonishing.

What is more, believing in the simultaneity or communism of the Dadaists, Feyerabend tried to justify the Nazis' wickedness. He wrote that "»[o]f course« many Nazis were puny and despicable men […] But puny and despicable men are human, they have been created in the image of God and that alone requires of us to treat them with greater circumspection than on the basis of a mere »of course«". [181] On the other hand, he strongly attacked "teachers", "intellectual leaders", and "leaders of mankind": "Should not the same or an even greater punishment be extended to our »teachers« and our »intellectual leaders« than is now extended to individual and collective murders? Should not guilty teachers be found out with the same vigor one applies to the hunting of Nazi octogenarians? Are not the so-called »leaders of mankind« — men such as Christ, Buddha, St Augustine, Luther, Marx, some of our greatest criminals […]". [182] For Feyerabend, a Dadaist in philosophy, the Nazis were the same as or even better than the "teachers", "intellectual leaders", and "leaders of mankind". Generally, Feyerabend assailed Western civilization and held that this civilization was similar to the "spirit of Auschwitz": "Western civilization as a whole now values efficiency to an extent that occasionally makes ethical objections seem »naïve« and »unscientific«. There are many similarities between this civilization and the »spirit of Auschwitz«". [183] In short, his ideas, however absurd, astonishing or immoral, are wholly under-

[178] See Feyerabend, "Concluding Unphilosophical Conversation, p. 251 [487–527].

[179] See Feyerabend, **Conquest of Abundance…**, pp. 242–251; Feyerabend, **Three Dialogues on Knowledge…**, pp. 166–167.

[180] See Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert…**, p. 140; John Watkins, "Feyerabend Among Popperians…", p. 51 [47–57].

[181] Feyerabend, **Science in a Free…**, p. 139.

[182] Feyerabend, **Science in a Free…**, pp. 139–140.

[183] Feyerabend, **Farewell to Reason…**, p. 23.

standable on condition that one understands their relationship to Dadaism and its commitment to simultaneity and communism.

As a Dadaistic philosopher, and especially as a Dadaistic epistemologist or an epistemological Dadaist, Feyerabend could have been a liberal, a non-liberal, or an anti-liberal. Nonetheless, Curthoys and Suchting, the two Marxists from Australia, held that Feyerabend was an extreme liberal or individualist, profoundly hostile to socialism. [184] Obviously, they misunderstood Feyerabend — even though he did argue for liberalism or a free society. [185] His "liberalism or free society" aimed not to make an individual free, but to grant all traditions equal rights and equal access to the centres of power. Feyerabend defined his conception of this as follows:

> A free society is a society in which all traditions have equal rights and equal access to the centres of power (this differs from the customary definition where individuals have equal rights of access to positions defined by a special tradition — the tradition of Western Science and Rationalism). [186]
>
> A free society is a society in which all traditions are given equal rights, equal access to education and other positions of power. [187]

Feyerabend's free society could conceivably lead to individuals living under a dictatorship or some form of totalitarianism. For Feyerabend, these were also traditions to be given equal rights and equal access to education and other positions of power. For this reason, his "liberalism or free society" was so different from the customary one (in which individuals had equal rights of access to positions defined by a special tradition) that his notion actually referred neither to individualism nor to liberalism (let alone extreme individualism or liberalism), but rather to despotism or absolutism. In general, Feyerabend was neither an individualist nor a liberal, but instead a Dadaist in philosophy. Furthermore, he also seemed to be a materialist — one who engaged in many discussions concerning

---

[184] See Curthoys and Suchting, "Feyerabend's Discourse against Method…", p. 338 [243–379].

[185] Feyerabend detested the tensions inside the Brecht Circle, and criticized and opposed the collective pressure of the certainly pushy and closely knit group that surrounded Popper in his later life. This should be explained from the perspective of their personal relationships, not from that of Feyerabend's belief in liberalism.

[186] Feyerabend, **Science in a Free…**, p. 9.

[187] Feyerabend, **Science in a Free…**, p. 30.

dialectical materialism, eliminative materialism and reductive materialism. The subject of his materialism will be dealt with in the next subsection.

3.2 Dialectical Materialism, Eliminative Materialism and Reductive Materialism

Feyerabend claimed of himself that he was "an unrepentant materialist". [188] He was very keen on dialectical materialism, and even converted to it.[189] As he put it, "Imre Lakatos has convinced me that I am not a Popperian, but a dialectical materialist. [...] My last article has defended Bohr against Popper. The next article will defend Marx (whom I am studying) against Popper". [190] Furthermore, he said in a letter to Hans Albert that "I have converted to dialectical materialism". [191] However, Albert was not surprised at his conversion to dialectical materialism, and thought that an entirely new form of dialectical materialism would appear. [192] In addition, he convinced his closest friend Lakatos of his own dialectical materialism. He emphasized that Mill and Engels were predecessors of Lakatos in philosophy, [193] writing that "[t]he catalyst that leads from Mill to Lakatos is the philosophy of dialectical materialism". [194] To a degree, Feyerabend's dialectical materialism was accepted, and so his article was employed to propagandize for that stance. [195]

In particular, his paper "Dialectical Materialism and the Quantum Theory" (1966), which includes both a review of Gustav Wetter's book **Dialectical Materialism: A Historical and Systematic Survey of Philosophy in the Soviet Union**

[188] Feyerabend, **Physics and Philosophy...**, p. 256.

[189] See Lakatos and Feyerabend Paul, **For and Against Method...**, p. 151; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 147.

[190] Baum (ed.), **Paul Feyerabend...**„ p. 83; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 145.

[191] Baum (ed.), **Paul Feyerabend...**„ p. 85; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 147.

[192] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, pp. 147–148.

[193] See Baum and Mühlmann (eds.), **Paul Feyerabend, Hans Albert...**, pp. 106–107.

[194] Feyerabend, **Problems of Empiricism...**, p. 197.

[195] See Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 112.

and some comments on an article entitled "Quantum Mechanics and Dialectical Materialism" by Loren Graham, was centered on dialectical principles. Amongst the latter, in Feyerabend's view, were the following:

> (1) the emphasis on the fact that in nature there are no isolated elements, but that everything is related to everything else; (2) the emphasis on the existence of discontinuities, indicating essential limitations of our knowledge; (3) connected with this, the emphasis on the approximate character of knowledge; (4) the demand to unite practice with theory, so that there is neither unreflected practice nor empty theory; (5) the change (motion) of concepts in the course of the development of our knowledge. [196]

Among the above-mentioned items, the first of which belongs to dialectical ontology, items (2), (3), (4) and (5) belong to dialectical epistemology, similar to his Dadaistic or anarchistic epistemology. One scholar has interpreted the above-mentioned article ("Dialectical Materialism and the Quantum Theory") as an attempt by Feyerabend to deploy his Dadaistic philosophy:

> The SR [*Slavic Review*] article is an attempt by Feyerabend to deploy his emergent Dadaist philosophical methodology. Against a perceived background of Western intellectuals' failure to take seriously dialectical materialist ideology, [...] Feyerabend offers a charitable interpretation of how dialectical materialism could be expected to motivate and guide research [...] the 1960s saw Feyerabend's early attempt to exercise his Dadaist approach to philosophy. [197]

From the perspective of that commentator, Feyerabend's Dadaistic philosophy emerged out of dialectical materialism. This shows that his Dadaistic or anarchist epistemology was closely connected with dialectical materialism. As a Dadaist in philosophy, and especially in epistemology, Feyerabend attached a great importance to dialectical materialism. He held that "most of the dialectical principles enunciated above have been also accepted by Niels Bohr (1885–1962)". [198] He praised Bohr, and regarded his way of presenting what claims to be the truth as "a dialectical presentation which enlarges faults and lets different and incommensurable jargons run side by side". [199] According to Feyerabend, dialectical materi-

[196] Feyerabend, **Physics and Philosophy...**, pp. 220–221.

[197] Kent, "Paul Feyerabend and the Dialectical Character of Quantum Mechanics...", p. 46 [51–57].

[198] Feyerabend, **Physics and Philosophy...**, p. 221.

[199] Feyerabend, **Three Dialogues on Knowledge...**, p. 95.

alism has two marked characteristics: its "need for tenacity" and its "synthesis": "The need for tenacity was emphasized by those dialectical materialists who objected to extreme »idealistic« flights of fancy. And the synthesis, finally, is the very essence of dialectical materialism in the form in which it appears in the writings of Engels, Lenin and Trotsky". [200] For Feyerabend, it was clear that Aristotle, Descartes, Newton, Kant, Russell, Popper and Lakatos were all examples of naïve rationalism. By contrast, he considered dialectical materialism a sophisticated form of rationalism: "Sophisticated rationalism is quite rare. It may be found [...] in dialectical materialism". [201] More importantly, he optimistically believed that "philosophy can advance our knowledge". [202] Of course, it follows from his belief that "dialectical materialism, as an important part of philosophy, can also advance our state of knowledge". It is very clear that Feyerabend greatly valued dialectical materialism. However, he wrote, "I did not accept dialectics and historical materialism". [203] In other words, Feyerabend did not accept dialectical materialism — all the while valuing it and even converting to it. Given his Dadaistic philosophy, "to be a dialectical materialist, Feyerabend must also be a non-dialectical materialist or an anti-dialectical materialist".

Feyerabend also addressed eliminative materialism or reductive materialism, in addition to dialectical materialism. [204] In "Materialism and the Mind-Body Problem" he defined materialism as follows: "Materialism, as it will be discussed here, assumes that the only entities existing in the world are atoms, aggregates of atoms, and that the only properties and relations are the properties of, and the relations between, such aggregates". [205] In our view, the materialism defined by Feyerabend was an eliminative materialism, for it eliminated "mental events" so that there were only "atoms" in the world. In his comment "Mental Events and the Brain", Feyerabend also explicitly denied mental events. He came to the following

[200] Feyerabend, **Problems of Empiricism...**, p. 144.

[201] Feyerabend, **Philosophical Papers...**, p. 202.

[202] Feyerabend, **Physics and Philosophy...**, pp. 223–224.

[203] Feyerabend, **Science in a Free...**, p. 113.

[204] See As regards Feyerabend on anti-reductionism and eliminative materialism, see also Yuanlin Guo, "Feyerabend's Confusion: Anti-Reductionism and Eliminative Materialism", *Shijie Zhexue (World Philosophy)* 2014, Vol. 5, pp. 83–93.

[205] Paul K. Feyerabend, **Realism, Rationalism and Scientific Method, Philosophical Papers, Volume 1**, Cambridge University Press, Cambridge 1981, p. 161.

conclusion: "There is no reason why physiology should not by itself be capable of forming such a background. We have to conclude, then, that the reasonableness — and the success — of a purely physiological approach to human beings is not at all dependent on the outcome of an analysis of H". [206] For this reason, some philosophers thought that Feyerabend had sought to justify eliminative materialism. [207]

However, some other philosophers have claimed that Feyerabend was not an eliminative materialist. [208] On the basis of the following quotation from Feyerabend's "Explanation, Reduction and Empiricism", John Preston sees him as a reductive materialist:

> All these difficulties disappear if we are prepared to admit that, in the course of the progress of knowledge, we may have to abandon a certain point of view and the meanings connected with it — for example if we are prepare to admit that the mental connotation of mental terms may be spurious and in need of replacement by a physical connotation according to which mental events, such as pains, states of awareness, and thoughts are complex physical states of either the brain or the central nervous system, or perhaps the whole organism. [209]

In the quotation, Feyerabend points out that the mental connotation of mental terms might need to be "replaced by" rather than "reduced to" a physical connotation, and that mental events "were" rather than "were reduced to" complex physical states of the brain, the nervous system or the whole organism. Therefore, this vague quotation is not evidence that Feyerabend defended reductive materialism. In "Materialism and the Mind-body Problem" he did not develop in detail eliminative materialism (as a position in which there is only matter and no mind), all the while refuting three arguments against materialism. More importantly, his follow-

[206] Paul K. Feyerabend, "Mental Events and the Brain", *The Journal of Philosophy* 1963, Vol. 60, No. 11, p. 296 [295–296], https://doi.org/10.2307/2023030.

[207] See Richard Rorty, **Philosophy and the Mirror of Nature**, Princeton University Press, Princeton 1979, p. 117; Grover Maxwell, "Feyerabend's Materialism", in: Gonzalo Munévar (ed), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, pp. 453–463.

[208] See John Preston, **Feyerabend: Philosophy, Science and Society**, Polity Press, Cambridge 1997, pp. 151, 155 and 162; Jamie Shaw, "Feyerabend Never Was an Eliminative Materialist: Feyerabend's Meta-Philosophy and the Mind-Body Problem", in: Karim Bschir and Jamie Shaw (eds.), **Interpreting Feyerabend: Critical Essays**, Cambridge University Press, New York 2021, pp. 114–131.

[209] Feyerabend, **Realism, Rationalism and Scientific Method...**, p. 90; Preston, **Feyerabend...**, p. 151.

ers so far have failed to do that, too. In such a situation, Preston's view seems reasonable. If he was engaged in seeking to justify reductive materialism rather than eliminative materialism, then it seems reasonable to conclude that Feyerabend was inclined to become a Marxist or a dialectical materialist, as reductive materialism is more similar to dialectical materialism than eliminative materialism is. Both reductive materialism and dialectical materialism subscribe to dualism, in claiming that matter and mind exist, whereas eliminative materialism subscribes to monism, claiming as it does that only matter exists.

3.3 Concluding Comments: Was Feyerabend a Marxist?

It astonished Lakatos that Feyerabend had converted to Marxism. Lakatos wrote in a letter to Feyerabend: "I was stunned by your conversion to Marxism". [210] But did Feyerabend really convert to Marxism, communism or dialectical materialism? He chose the term "Dadaism" for his epistemology, his philosophy, and his overall intellectual enterprise. Dadaism had clear links with Marxism and communism, and was turned into Bolshevism in Germany. The Dadaist movement in that country was a radical communist movement that was against the bourgeoisie and in favor of the proletariat, while being aimed at destroying capitalist society, liberating all of mankind, and ultimately establishing communism all over the world. Did Feyerabend use "Dadaism" to signify Marxism and communism? No answer can be found, because he himself never provided any. Nevertheless, Marxism, communism and dialectical materialism all indisputably had a great influence on him.

In a letter to a friend, Feyerabend declared that "I have never become a communist, and I have not joined any party". [211] Indeed, he was neither a communist nor a member of any party. As a Dadaistic philosopher he did not adhere to Marxism, communism or dialectical materialism, but rather made "»opportunistic« use of the classics of Marxism (quotations, references)". [212] In this connection, one scholar has written that "[u]pon scanning Feyerabend's references to Marxism

[210] Lakatos and Feyerabend, **For and Against Method...**, p. 150.

[211] Baum (ed.), **Paul Feyerabend...**„ p. 161; Baum (ed.), **Paul Feyerabend, Hans Albert...**, p. 213.

[212] Curthoys and Suchting, "Feyerabend's Discourse against Method…", p. 338.

across his writings, one finds that he is prepared to both criticize and praise the tradition and its practitioners". [213] That is to say, Feyerabend was both for and against Marxism. Hence, as a Dadaist in philosophy he was someone to whom the question of whether or not one is a Marxist was not really applicable.

As a Dadaistic philosopher, and especially as a Dadaistic epistemologist or epistemological Dadaist, Feyerabend may have been a Marxist and a communist, or a non-Marxist and a non-communist, or an anti-Marxist and an anti-communist. Given that he researched and praised dialectical materialism, it seems fair to assert that he was a dialectical materialist. However, taking his Dadaist philosophy as a basis, it appears he might well have been either a non-dialectical materialist or an anti-dialectical one. On the one hand, then, Feyerabend was either an eliminative or a reductive materialist, in that he was either arguing for eliminative materialism or advocating reductive materialism. On the other hand, meanwhile, as an epistemological Dadaist, it is possible that he was either a non-eliminative materialist (or a non-reductive one) or an anti-eliminative materialist (or an anti-reductive one).

4. Conclusion

Marxism thus had an important influence on Feyerabend, even though he said he had never accepted either dialectics or historical materialism. Feyerabend mentioned, referenced, quoted, commented or discussed the following Marxists, communists, or leftists in his writings: Marx, Engels, Lenin, Stalin, Mao, Fidel Castro, Karl Kautsky, Eduard Bernstein, Rosa Luxemburg, Leon Trotsky, Bertolt Brecht, Hanns Eisler, Walter Hollitscher, Georg Lukacs, Ernst Bloch, Herbert Marcuse, Theodor Adorno, Jürgen Habermas, Joseph Needham, Jean-Paul Sartre, Walter Benjamin, Louis Althusser, Daniel Cohn-Benit and Robin Blackburn. On numerous occasions he discussed and commented on Dadaism, Marxism, communism, anarchism, liberalism, dialectical materialism, reductive materialism and, especially, eliminative materialism. Equally, though, Feyerabend himself influenced the Marxism of his own time. Feyerabend and his writings were certainly favored and extolled by some of his Marxist and leftist contemporaries, even though he

[213] Kent, "Paul Feyerabend and the Dialectical Character of Quantum Mechanics...", p. 53 [51–57].

criticized contemporary Marxism, neo-Marxists, the New Left, "revolutionaries", and students of the left.

Feyerabend produced an original Dadaist philosophy and, especially, a Dadaistic epistemology, in circumstances where this was conditioned by the fact that Dadaism seemed highly relevant to Marxism and communism. The Dadaist movement in Germany was a radical communist movement opposed to the bourgeoisie, on the side of the proletariat, and seeking to upend capitalist society, emancipate all of mankind and, ultimately, establish communism across the world. As a Dadaist in philosophy, he could well have been a Marxist, non-Marxist, or anti-Marxist, in that he also had to be an anti-Dadaist in philosophy. The relationship of Feyerabend to Marxism should be understood and explained from the perspective of his Dadaistic philosophy, and especially his Dadaistic epistemology or epistemological Dadaism. In short, his ideas, however absurd, astonishing, or immoral, are wholly understandable if he is regarded as a Dadaist philosopher, or a Dadaist in philosophy. Consequently, Feyerabend's philosophy really ought to be studied and researched from the viewpoint of Dadaism in philosophy.

Feyerabend made opportunistic use of Marxism to criticize and oppose modern Western civilization, including capitalism, science and rationality. Marxists and leftists, of course, attack capitalism forcefully with a view to establishing communism, but favour science and rationality. Basing their theories on modern natural science and rationality, and especially on the theory of evolution of Charles Darwin (1809–1882), Marx and Engels imagined certain general laws of motion and development operative in human society and thought, and tried to change mankind and society according to those laws. By contrast, Feyerabend assailed Western capitalism, and especially science and rationality, making him an enemy of science, rationality and Western civilization. In this connection, as a Dadaist philosopher, he could well have been either a Marxist or an anti-Marxist.

Acknowledgments

The authors wish to thank Gonzalo Munevar, Hans Radder, and Mike Stuart for their helpful comments. In particular, they are grateful to Junyu Guo for all his help.

References

1. Agassi Joseph, "Review Essay of **Against Method** by Paul Feyerabend", *Philosophia* 1976, Vol. 6, No. 1, pp. 165–191.
2. Agassi Joseph, "As You Like", in: Gonzalo Munévar (ed), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, pp. 379–387.
3. Augustin Christian (ed.), **Aber ein Paul hilft doch dem anderen: Paul Feyerabend — Paul Hoyningen-Huene Briefwechsel 1983–1994**, Passagen Verlag, Vienna 2010.
4. Baum Wilhelm (ed.), **Paul Feyerabend — Hans Albert Briefwecihsel**, Fischer Taschenbuch Verlag GmbH, Frankfurt am Main 1997.
5. Baum Wilhelm (ed.), **Paul Feyerabend, Hans Albert: Briefwechsel Band 1 (1958–1971)**, Kitab, Vienna 2008.
6. Baum Wilhelm and Mühlmann Michael (eds.), **Paul Feyerabend, Hans Albert: Briefwechsel, Band 2, (1972–1986)**, Kitab, Vienna 2009.
7. Collodel Matteo, "Was Feyerabend a Popperian? Methodological Issues in the History of the Philosophy of Science", *Studies in History and Philosophy of Science* 2016, Vol. 57, 27–56, https://doi.org/10.1016/j.shpsa.2015.08.004.
8. Collodel Matteo and Oberheim Eric (eds.), **Feyerabend's Formative Years (Vol. 1. Feyerabend and Popper, Correspondence and Unpublished Papers)**, Springer Nature Switzerland AG, Cham 2020.
9. Courtois Stéphane, Werth Nicholas, Panné Jean-Luis, Paczkowski Andrzej, Bartošek Karel, and Margolin Jean-Luis, **The Black Book of Communism: Crimes, Terror, Repression**, Mark Kramer (ed.), trans. Jonathan Murphy and Mark Kramer, Harvard University Press, Cambridge — London 1999.
10. Curthoys Jean and Suchting Walter, "Feyerabend's Discourse against Method: A Marxist Critique", *Inquiry* 1977, Vol. 20, No. 1–4, pp. 243–379, https://doi.org/10.1080/00201747708601836.
11. Feyerabend Paul K., **Against Method. New Edition**, London, Verso 2010.
12. Feyerabend Paul K., **Against Method: Outline of an Anarchistic Theory of Knowledge**, Verso, London 1979.
13. Feyerabend Paul K., **Briefe an einen Freund**, Hans Peter Duerr (ed.), Suhrkamp, Frankfurt am Main 1995.

14. Feyerabend Paul K., "Concluding Unphilosophical Conversation", in: Gonzalo Munévar (ed), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, pp. 487–527.
15. Feyerabend Paul K., **Conquest of Abundance: A Tale of Abstraction versus The Richness of Being,** Bert Terpstra (ed.),The University of Chicago Press, Chicago 1999.
16. Feyerabend Paul K., **Farewell to Reason**, Verso, London 1988.
17. Feyerabend Paul K., **Killing Time: The Autobiography of Paul Feyerabend**, The University of Chicago Press, Chicago 1995.
18. Feyerabend Paul K., **Knowledge, Science and Relativism, Philosophical Papers, Volume 3**, John Preston (ed.), Cambridge University Press, Cambridge 1999.
19. Feyerabend Paul K., "Mental Events and the Brain", *The Journal of Philosophy* 1963, Vol. 60, No. 11, pp. 295–296, https://doi.org/10.2307/2023030.
20. Feyerabend Paul K., **Physics and Philosophy, Philosophical Papers, Volume 4**, Stefano Gattei and Joseph Agassi (eds.), Cambridge University Press, New York 2016.
21. Feyerabend Paul K., **Problems of Empiricism, Philosophical Papers, volume 2**, Cambridge University Press, Cambridge 1981.
22. Feyerabend Paul K., **Realism, Rationalism and Scientific Method, Philosophical Papers, Volume 1**, Cambridge University Press, Cambridge 1981.
23. Feyerabend Paul K., **Science in a Free Society**, Verso, London 1982.
24. Feyerabend Paul K., **Thesen zum Anarchismus: Artikel aus der Reihe "Unter den Pflaster liegt der Strand"**, Thorsten Hinz (ed.), Karin Kramer Verlag, Berlin 1996.
25. Feyerabend Paul K., **The Tyranny of Science**, edited, and with an introduction, by Eric Oberheim, Polity Press, Cambridge 2011.
26. Feyerabend Paul K., **Three Dialogues on Knowledge**, Basil Blackwell Ltd, Oxford 1991.
27. Guo Yuanlin, "Feyerabend's Confusion: Anti-Reductionism and Eliminative Materialism", *Shijie Zhexue (World Philosophy)* 2014, Vol. 5, pp. 83–93.
28. Guo Yuanlin and Zheng Xin, "Wittgenstein's Influence on Feyerabend", *Jiangxi Shehuikexue (Jiangxi Social Sciences)* 2016, Vol. 36, No. 10, pp. 25–32.
29. Hoyningen-Huene Paul (ed.), "Two Letters of Paul Feyerabend to Thomas S. Kuhn on a Draft of **The Structure of Scientific Revolutions**", *Studies in History and Philos-*

ophy of Science 1995, Vol. 26, No. 3, pp. 353–387, https://doi.org/10.1016/0039-3681(95)00005-8.

30. Hoyningen-Huene Paul, "Paul Feyerabend and Thomas Kuhn", in: John Preston, Gonzalo Munevar, and David Lamb (eds.),**The Worst Enemy of Science? Essays in Memory of Paul Feyerabend**, Oxford University Press, New York — Oxford 2000, pp. 102–114.
31. Hoyningen-Huene Paul, "More Letters by Paul Feyerabend to Thomas S. Kuhn on Proto-Structure", *Studies in History and Philosophy of Science* 2006, Vol. 37, No. 4, pp. 610–632, https://doi.org/10.1016/j.shpsa.2006.09.007.
32. Kent Rory, "Paul Feyerabend and the Dialectical Character of Quantum Mechanics: A Lesson in Philosophical Dadaism", *International Studies in the Philosophy of Science* 2022, Vo. 35, No. 1, pp. 51–67, https://doi.org/10.1080/02698595.2022.2075687.
33. Kidd Ian, "Objectivity, Abstraction and the Individual: The Influence of Søren Kierkegaard on Paul Feyerabend", *Studies in History and Philosophy of Science* 2011, Vol. 42, No. 1, pp. 125–134.
34. Kidd Ian, "Feyerabend, Pseudo-Dionysius, and the Ineffability of Reality", *Philosophia* 2012, Vol. 40, No. 2, pp. 365–377, https://doi.org/10.1007/s11406-011-9322-9.
35. Lakatos Imre and Feyerabend Paul, **For and Against Method**, edited and with an Introduction by Matteo Motterlini, The University of Chicago Press, Chicago 1999.
36. Mao Zedong, **Selected Works of Mao Tsetung**, **Volume II,** Foreign Language Press, Peking 1975.
37. Mao Zedong, **Selected Works of Mao Tsetung**, **Volume IV,** Foreign Language Press, Peking 1975.
38. Mao Zedong, **Selected Works of Mao Tsetung**, **Volume V,** Foreign Language Press, Peking 1977.
39. Maxwell Grover, "Feyerabend's Materialism", in: Gonzalo Munévar (ed), **Beyond Reason: Essays on the Philosophy of Paul Feyerabend**, Kluwer Academic Publishers, Dordrecht 1991, pp. 453–463.
40. Motherwell Robert, **The Dada Painters and Poets: An Anthology**, Second Edition, Belknap Press, Cambridge 1981.
41. Munévar Gonzalo, "Historical Antecedents to the Philosophy of Paul Feyerabend", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 9–16, https://doi.org/10.1016/j.shpsa.2015.11.002.
42. Oberheim Eric, **Feyerabend's Philosophy**, Walter der Gruyter, Berlin 2006.

43. Oberheim Eric, "Rediscovering Einstein's Legacy: How Einstein Anticipated Kuhn and Feyerabend on the Nature of Science", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 17–26, https://doi.org/10.1016/j.shpsa.2015.11.005.

44. Preston John, **Feyerabend: Philosophy, Science and Society**, Polity Press, Cambridge 1997.

45. Rorty Richard, **Philosophy and the Mirror of Nature**, Princeton University Press, Princeton 1979.

46. Shaw Jamie, "Feyerabend Never Was an Eliminative Materialist: Feyerabend's Meta-Philosophy and the Mind-Body Problem", in: Karim Bschir and Jamie Shaw (eds.), **Interpreting Feyerabend: Critical Essays**, Cambridge University Press, New York 2021, pp. 114–131.

47. Watkins John, "Feyerabend Among Popperians, 1948–1978", in: John Preston, Gonzalo Munevar, and David Lamb (eds.), **The Worst Enemy of Science? Essays in Memory of Paul Feyerabend**, Oxford University Press, New York 2000, pp. 47–57.

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 123–156

https://doi.org/10.53763/fag.2023.20.2.224

ISSN 2299-0356

ARTYKUŁ ORYGINALNY / ORIGINAL ARTICLE

Donald Gillies

University College London

Feyerabend's Criticisms of Kuhn [1]

Received: October 30, 2023. Accepted: November 13, 2023. Published online: February 9, 2024.

Abstract: This paper gives an account of Feyerabend's criticisms of Kuhn. The main exposition of these criticisms is in Feyerabend's paper in the 1970 collection **Criticism and the Growth of Knowledge**, edited by Imre Lakatos and Alan Musgrave. However, another source consists of two letters from Feyerabend to Kuhn written in the period 1960–1961, which were published by Hoyningen-Huene in 1995. The paper contains a comparison of Feyerabend's 1970 criticisms with the earlier ones in his letters to Kuhn. Kuhn replied to Feyerabend's criticisms in his contribution to the 1970 collection. However, I claim that Feyerabend's criticisms have considerable force, and Kuhn succeeds in answering some, but not all of them. In Section 5 of the paper, I try to answer Feyerabend's criticisms of Kuhn by reviving the old empiricist idea of the inductive justification of scientific theories by the results of observations and experiments (observation statements). This leads to a position which is called *empirical rationalism*, and which is perhaps Kuhnian in character without being exactly the same as Kuhn's own views.

Keywords:

confirmation;
critical rationalism;
incommensurability;
inductive justification

1. Introduction. Outline of Kuhn's Early Position

The aim of this paper is to state and discuss Feyerabend's criticisms of Kuhn, and Kuhn's reply to these criticisms. My claim will be that Kuhn's reply is not ade-

[1] I am grateful for helpful comments on an earlier draft of this paper by Karim Bschir, Paul Hoyningen-Huene, John Preston and an anonymous referee. These led to several improvements.

quate in many respects, but that better replies to Feyerabend's criticisms can be developed. To achieve this, I suggest that Kuhn's approach can be strengthened by adding some ideas from the empiricist tradition. This leads to a position which I call empirical rationalism (Section 5).

Feyerabend's criticisms are directed against what could be called Kuhn's early position. This is defined by his writings published between 1957 (**The Copernican Revolution**) and 1962 (**The Structure of Scientific Revolutions**). This position is a familiar one, but it is probably worth giving a brief summary of it in this section, before going on to Feyerabend's criticisms in the next two sections. [2]

Kuhn's basic idea is that science develops through periods of *normal science* which are characterised by the dominance of a *paradigm*, but which are interrupted by occasional revolutions during which the old paradigm is replaced by a new one. I will illustrate this theory by considering in turn three favourite examples of Kuhn's. These are (i) the Copernican Revolution, (ii) the Einsteinian Revolution, and (iii) the Development of Theories of Light.

(i) *The Copernican Revolution*. Kuhn's first book, published in 1957, was entitled **The Copernican Revolution**, and it was probably this example more than any other which led him to his general model of scientific revolutions. From late Greek times until Copernicus, astronomy was dominated by the Aristotelian-Ptolemaic paradigm. The Earth was considered to be stationary at the centre of the universe. The different movements of sublunary and heavenly bodies were described by Aristotelian mechanics. The astronomer had to describe and predict the movements of the Sun, Moon and planets as accurately as possible using the Ptolemaic scheme of epicycles, equants etc. This was the normal science of the time. [3]

Copernicus, however, challenged the dominant paradigm by suggesting that the Earth spun on its axis and moved round the Sun. The publication of his book **De Revolutionibus Orbium Caelestium** (1543) inaugurated a revolutionary period during which the old Aristotelian-Ptolemaic paradigm was replaced by a new paradigm based on Newtonian mechanics. Newton published his new mechanics

[2] See Thomas S. Kuhn, **The Copernican Revolution: Planetary Astronomy in the Development of Western Thought**, first edition in 1957, Vintage Books, Cambridge 1959; Thomas S. Kuhn, **The Structure of Scientific Revolutions**, The University of Chicago Press, Chicago — London 1962.

[3] See Kuhn, **The Copernican Revolution...**

in **Philosophiae Naturalis Principia Mathematica** (1687), but the new paradigm was not based directly on this text because the majority of the scientists of the time preferred to use Leibniz's version of the calculus rather than Newton's geometrical approach to mathematics.

(ii) *The Einsteinian Revolution*. The triumph of the Newtonian paradigm initiated a new period of normal science for astronomy (c. 1700 – c. 1900). The dominant paradigm consisted in Newtonian mechanics, including the law of gravity, and the normal scientist had to use this tool to explain the motions of the heavenly bodies in detail comets, perturbations of the planets and the Moon, etc. In the Einsteinian revolution (c. 1900 – c. 1920), however, the Newtonian paradigm was replaced by the special and general theories of relativity.

(iii) *The Development of Theories of Light*. From about 1700 to the present, the development of theories of light are claimed by Kuhn to exemplify his model of periods of normal science dominated by a paradigm, interrupted by occasional revolutions. At the beginning of the eighteenth century, Newton's theory of light came to be generally accepted. This postulated that light consists of a stream of particles. At the beginning of the nineteenth century, Young in England and Fresnel in France overthrew this Newtonian paradigm and replaced it by a new one, according to which light was a transverse wave motion in a luminiferous ether. This was in turn replaced early in the twentyth century by the new model of Planck, Einstein and others, according to which light consists of photons, i.e., quantum-mechanical entities that exhibit some characteristics of waves and some of particles.

Before 1700, however, Kuhn sees the situation as regards theories of light as essentially different. As he says:

> No period between remote antiquity and the end of the seventeenth century exhibited a single generally accepted view about the nature of light. Instead there were a number of competing schools and sub-schools, most of them espousing one variant or another of Epicurean, Aristotelian, or Platonic theory. One group took light to be particles emanating from material bodies; for another it was a modification of the medium that intervened between the body and the eye; still another explained light in terms of an interaction of the medium with an emanation from the eye; and there were other combinations and modifications besides. Each of the corresponding schools derived strength from its relation to some particular metaphysics[…]. [4]

[4] Kuhn, **The Structure…**, p. 12.

This is a description of what Kuhn calls "pre-paradigmatic science". This is characterised by a number of competing schools, and controversies over fundamentals. Disciplines in the pre-paradigmatic phase are, according to Kuhn, immature and not fully scientific. The victory of one single paradigm, which is accepted by nearly everyone in the scientific community, inaugurates the first period of normal science.

Kuhn describes normal science as follows: "When examining normal science [...] we shall want finally to describe that research as a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education". [5] The "conceptual boxes" are those given by the dominant paradigm. Sometimes observations and experiments appear to contradict the paradigm, but normal scientists do not react by questioning the validity of the paradigm. They see the situation as a puzzle which has to be resolved while maintaining the paradigm. Hence, their activity is described by Kuhn as "puzzle-solving". He gives the following further description of normal science: "Normal science, the activity in which most scientists inevitably spend almost all their time, is predicated on the assumption that the scientific community knows what the world is like. Much of the success of the enterprise derives from the community's willingness to defend that assumption, if necessary at considerable cost". [6]

It is clear from this passage that Kuhn regards normal science as a successful enterprise, and he explicitly defends this assumption in a number of passages, such as the following: "[H]istory strongly suggests that, though one can practice science — as one does philosophy or art or political science — without a firm consensus, this more flexible practice will not produce the pattern of rapid consequential scientific advance to which recent centuries have accustomed us". [7] Kuhn stresses that commitment to a paradigm and the practice of normal science may force scientists to investigate the natural world in a detail and depth which would not otherwise be achieved. This is one of the secrets of the success of normal science:

[5] Kuhn, **The Structure...**, p. 5.

[6] Kuhn, **The Structure...**, p. 5.

[7] Thomas S. Kuhn, "The Essential Tension: Tradition and Innovation in Scientific Research", in: Thomas S. Kuhn (ed.), **The Essential Tension: Selected Studies in Scientific Tradition and Change**, University of Chicago Press, Chicago 1977, p. 232 [225–239].

> By focusing attention upon a small range of relatively esoteric problems, t he paradigm forces scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable. [...] during the period when the paradigm is successful, the profession will have solved problems that its members could scarcely have imagined and would never have undertaken without commitment to the paradigm. And at least part of that achievement always proves to be permanent. [8]

Kuhn's elaboration and defence of the concept of normal science is the principal target for Feyerabend's criticisms, as we shall see in the next section.

2. Feyerabend's Criticisms of Kuhn in 1970

Feyerabend and Kuhn were both in Berkeley in the late 1950s and early 1960s. Feyerabend begins his 1970 paper with some reminiscences of that period:

> In the years 1960 and 1961, when Kuhn was a member of the philosophy department at the University of California in Berkeley, I had the good fortune of being able to discuss with him various aspects of science. I have profited enormously from these discussions and I have looked at science in a new way ever since. [9]

Feyerabend adds a footnote on the next page, which says of his debates with Kuhn: "Some of which were carried out in the now defunct *Café Old Europe* on Telegraph Avenue and greatly amused the other customers by their friendly vehemence".[10] However, Feyerabend and Kuhn did not always disagree. The concept of incommensurability seems to have emerged from their discussions. As goes on to say: "I do not know who of us was the first to use the term »incommensurable« in the sense that is at issue here. It occurs in Kuhn's **Structure of Scientific Revolutions** and in my essay »Explanation, Reduction, and Empiricism«, both of which appeared in 1962". [11]

---

[8] Kuhn, **The Structure...**, pp. 24–25.

[9] Paul K. Feyerabend, "Consolations for the Specialist", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Vol. 4, Cambridge University Press 1970, London — New York, p. 197 [197–230].

[10] Feyerabend, "Consolations for ...", p. 198.

[11] Feyerabend, "Consolations for ...", p. 219.

All this gives a picture of friendly co-operation, and so it comes as something of a surprise that Feyerabend's criticisms of Kuhn are often very harsh in tone. Feyerabend begins by accusing Kuhn of being ambiguous about whether what he is doing is prescription or description:

> Whenever I read Kuhn, I am troubled by the following question: are we here presented with *methodological prescriptions* which tell the scientist how to proceed; or are we given a *description*, void of any evaluative element, of those activities which are generally called "scientific"? Kuhn's writings, it seems to me, do not lead to a straightforward answer. They are *ambiguous* in the sense that they are compatible with, and lend support to, both interpretations. [12]

Moreover, Feyerabend goes on to suggest on the next page that this ambiguity is intentional and is used by Kuhn for propagandistic purposes. More specifically, it is used to promote a general ideology which Feyerabend thinks forms the background of Kuhn's thinking. Feyerabend strongly disapproves of this ideology, of which he gives the following account:

> This ideology, so it seemed to me, could only give comfort to the most narrowminded and the most conceited kind of specialism. It would tend to inhibit the advancement of knowledge. And is bound to increase the anti-humanitarian tendencies which are such a disquieting feature of much of post-Newtonian science. [13]

It is clear that Feyerabend is objecting to the ideology of normal science, for which he seems to entertain a visceral hatred. Typically, he speaks of "the humourless dedication and the constipated style of a »normal« science". [14]

To combat normal science, Feyerabend suggests that if a paradigm has become dominant, instead of just accepting it, scientists should adopt a *principle of proliferation*, according to which they should try to invent and develop theories alternative to the paradigm. This of course is a prescription, but, unfortunately for Feyerabend, it seems that scientists have not adopted it for quite long periods during which science has developed well. These are the periods of normal science which, as we have seen, Kuhn describes in his historical accounts. However, Feyerabend goes on to challenge Kuhn's description by raising "the suspicion that

[12] Feyerabend, "Consolations for …", p. 198 [emphasis in the original].

[13] Feyerabend, "Consolations for …", pp. 197–198.

[14] Feyerabend, "Consolations for…", p. 199, fn. 4 [begining of the footnote on p. 198].

normal or »mature« science, as described by Kuhn, *is not even a historical fact"*. [15]

Feyerabend continues:

> [W]hy should we not start proliferating *at once* and *never* allow a purely normal science to come into existence? And is it too much to be hoped that scientists thought likewise, and that normal periods, if they ever existed, cannot have lasted very long and cannot have extended over large fields either? [16]

To support this point of view, Feyerabend gives an example taken from science in the second third of the nineteenth century. Instead of there being a single paradigm, as an advocate of normal science would consider desirable, there were, according to Feyerabend, three different and mutually incompatible paradigms, which he lists as follows:

> They were: (1) the *mechanical point of view* which found expression in astronomy, in the kinetic theory [...]; (2) the point of view connected with the invention of an independent and phenomenological *theory of heat* which finally turned out to be inconsistent with mechanics; (3) the point of view implicit in Faraday's and Maxwell's *electrodynamics* which was developed, and freed from its mechanical concomitants, by Hertz. [17]

Feyerabend uses this example from the history of science to develop an interesting argument in favour of his principle of proliferation. He thinks that sometimes the anomalies in one theory remain hidden and are only discovered when the situation is examined with a competing theory. So, anomalies in a paradigm may only come to light if theories alternative to the paradigm are developed. This view has been named the *anomaly importation thesis* (or AIT) by Hoyningen-Huene. [18] To illustrate this thesis, Feyerabend repeatedly uses the same example (Brownian motion). Preston lists 11 occasions when Feyerabend uses this example. [19] Despite its frequent recurrence, Preston points out that Feyerabend never

[15] Paul K. Feyerabend, "Consolations for...", p. 207 [emphasis in the original].

[16] Paul K. Feyerabend, "Consolations for...", p. 207 [emphasis in the original].

[17] Paul K. Feyerabend, "Consolations for...", p. 207 [emphasis in the original].

[18] See 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 — Oxford 2000, p. 112 [102–114].

gives a detailed account of the history behind the example. [20] Fortunately, Preston himself supplies just such an account, and I have made use of it in the following shorter sketch. [21]

The second of the paradigms mentioned by Feyerabend in the preceding quotation is the phenomenological theory of heat which involved the second law of thermodynamics. Brownian motion or the constant but irregular motion of tiny particles within water drops was discussed by the Scottish botanist Robert Brown in his publications of 1828–1829. From a modern point of view, Brownian motion can be considered as a *perpetuum mobile* of the second kind and so refutes the second law, though this law can still be regarded as "statistically valid". However, this refutation was not, and according to Feyerabend could not have been, discovered until a theory alternative to the phenomenological theory of heat had been developed — namely, the kinetic theory of heat. As Feyerabend says: "Nor was it possible to use the phenomenon of Brownian motion for a direct refutation of the second law of the phenomenological theory. The kinetic theory had to be introduced from the very start. Here again Einstein, following Boltzmann, led the way". [22]

I next turn to what I regard as Feyerabend's strongest and most interesting argument against Kuhn. It runs as follows:

> More than one social scientist has pointed out to me that now at last he had learned how to turn his field into a "science"0. […] The recipe, according to these people, is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal science that has this one theory as its paradigm. Students must be prevented from speculating along different lines and the more restless colleagues must be made to conform and "to do serious work". *Is this what Kuhn wants to achieve?* [23]

Note that this passage refers to the social sciences, but, as we shall see, Kuhn does not discuss the social sciences in detail in his reply. However, Feyerabend's main point in this argument does, in my view, also apply to the natural sciences,

[19] See Jonh Preston, **Feyerabend: Philosophy, Science and Society**, Polity Press, Cambridge 1997, p. 218, fn. 2.

[20] See Preston, **Feyerabend...**, p. 218, fn. 5.

[21] See Preston, **Feyerabend...**, pp. 126–130.

[22] Feyerabend, "Consolations for…", p. 208.

[23] Feyerabend, "Consolations for…", 198 [emphasis in the original].

and might be put like this. A normal science tradition could be established purely by political means which have little to do with science. Indeed, there are some examples of this in the history of the natural sciences. First of all, the Ptolemaic theory was the basic paradigm for astronomy among the Jesuits in the seventeenth century. Secondly Lysenkoism was the basic paradigm for research in biology in the Soviet Union under Stalin. [24] These are examples of a normal science tradition established by political forces external to the scientific community. However, it might be possible for a normal science tradition to be established by politics within the scientific community, by academic politics. Suppose, in a particular area of science, there are three main theories *T*, *T'* and *T''*, which contradict each other but seem to be about equally confirmed by the existing evidence. Let us further suppose that the supporters of *T* occupy much more powerful positions within this research community than those of *T'* or *T''*. These supporters might use this power to ensure that only those who accept *T* get jobs, promotions, publications in prestigious research journals and research grants. After a period of time, scientists in that area of research would realise that only by accepting *T* could they pursue a good career in that field, and most of them would do so. The few recalcitrant supporters of *T'* and *T''* who were unwilling to change their views would be eliminated, and a normal science tradition based on *T* would be established. Again, we could ask Feyerabend's rhetorical question: *Is this what Kuhn wants to achieve?* Clearly this is *not* what Kuhn wants to achieve, but how can he distinguish the normal science which he commends from a normal science established by the kind of political means just described? I will call this "Feyerabend's political argument".

An obvious move for a Kuhnian in the face of this argument would be to say that the admirable normal science, the normal science to be found in the historical examples which Kuhn describes, is based on a paradigm which is accepted by the community for good scientific reasons rather than for political reasons. Feyerabend, however, points out that such an approach is problematic because of a concept which Kuhn himself accepts: namely, *incommensurability*. As Feyerabend says: "Revolutions bring about a *change* of paradigm. But following Kuhn's account of this change, [...] it is impossible to say that they have led to something *better*. It is impossible to say this because pre- and post-revolutionary paradigms

[24] A good account of Lysenkoism is to be found in Helena Sheehan, **Marxism and the Philosophy of Science: A Critical History**, Humanities Press, Atlantic Highlands 1985.

are frequently incommensurable". [25] In fact, Feyerabend claims that Kuhn would agree with the following: "succeeding paradigms can be evaluated only with difficulty and [...] may be altogether incomparable, at least as far as more familiar standards of comparison are concerned". [26] If a new paradigm cannot be accepted because it is better than the old one according to some scientific standards of comparison, then it looks as if it can only get accepted for political reasons. This conclusion was indeed drawn by some of Kuhn's followers, but it definitely was not what Kuhn wanted to achieve. Indeed, Kuhn got very upset at this development.

3. Earlier Criticisms of Kuhn by Feyerabend, and those of the Critical Rationalists (Popper and Watkins)

So far, I have given an account of Feyerabend's criticisms of Kuhn which were published in 1970. However, Kuhn finished a mimeographed draft **of The Structure of Scientific Revolutions** in the fall or early winter of 1960, and, as both he and Feyerabend were at Berkeley at the time, Kuhn gave Feyerabend a copy to read. Feyerabend wrote two letters of comments, which he probably sent to Kuhn in the period from the fall of 1960 to the fall of 1961. These letters have survived and were published by Hoyningen-Huene in 1995. They thus constitute a first draft of Feyerabend's criticisms of Kuhn, which was written almost a decade before Feyerabend's paper of 1970. [27]

On the whole, Feyerabend's criticisms in his letters to Kuhn are the same as those he published in 1970, but there is one striking difference. Feyerabend's own

[25] Feyerabend, "Consolations for…", p. 202 [emphasis in the original].

[26] Feyerabend, "Consolations for…", p. 219.

[27] Feyerabend seems to have regretted to some extent this early criticism of Kuhn, because he writes in his autobiography: "my contrariness extended even to ideas that resembled my own. For example, I criticized the manuscript of Kuhn's **Structure of Scientific Revolutions**, which I read around 1960, in a rather old-fashioned way"; Paul K. Feyerabend, **Killing Time: The Autobiography of Paul Feyerabend**, Chicago University Press, Chicago 1995, p. 141. I owe this reference to 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(spring), pp. 24–55, https://doi.org/10.1086/680368.

philosophical position seem to have changed radically between 1961 and 1970. In 1961 he is a Popperian or critical rationalist, whereas by 1970 he seems to have adopted his later "anything goes" position. For those familiar with Feyerabend's position in his 1975 **Against Method**, [28] it is surprising to learn that he was for many years an advocate of critical rationalism, but this is undoubtedly the case and the evidence for it is given in Preston's 1997 study of Feyerabend.

Feyerabend finished his doctorate at the University of Vienna in 1951, and then obtained a scholarship with which, as Preston says, "he studied the philosophy of quantum mechanics under Popper at the London School of Economics between 1952 and 1953. Having been convinced by Popper's and Pierre Duhem's critiques of inductivism [...] Feyerabend came to consider Popper's view, falsificationism, a real option and, he later said, »fell for it«". [29] Feyerabend seems to have remained a Popperian for at least another full decade, because, as Preston says, "That Feyerabend was still very much under the influence of Popper in the mid-1960s is suggested by his gushing and wholly uncritical review of **Conjectures and Refutations**, a book he calls »a major contribution to philosophy [...] and a major event in the history of the subject«". [30] This review was published in *Isis* in 1965. [31]

Thus, Feyerabend's intellectual development had some points in common with that of Lakatos. In his "Proofs and Refutations" was a strong Popperian, but in the late 1960s he moved away from Popper and adopted a different position. [32] The rift with Popper was, for both Feyerabend and Lakatos, a violent one, accom-

[28] See Paul K. Feyerabend, **Against Method: Outline of an Anarchist Theory of Knowledge**, New Left Books, London 1975.

[29] Preston, **Feyerabend...**, p. 3.

[30] Preston, **Feyerabend...**,p. 212, fn. 4.

[31] John Preston sent me the following email communication regarding his current (2023) views on Feyerabend's intellectual development: "In the light of further discoveries, and of work by recent Feyerabend scholars, I would now only claim that Feyerabend was a (leftfield) critical rationalist up until 1965. (A meeting with Carl Friedrich von Weizsäcker in that year is supposedly what changed his mind and led him to his epistemological »anarchism«). So his review of **Conjectures and Refutations** published in 1965 is, I think, the very last gasp of his critical rationalism". Further interesting accounts of Feyerabend's complex intellectual development are to be found in Eric Oberheim, **Feyerabend's Philosophy**, *Quellen und Studien zur Philosophie,* Vol. 73, Walter de Gruyter, Berlin 2006, https://doi.org/10.1515/9783110891768 and Matteo Collodel, "Was Feyerabend a Popperian? Methodological Issues in the History of the Philosophy of Science", *Studies in History and Philosophy of Science Part A* 2016, Vol. 57, pp. 27–56.

panied by quarrels and ill-feeling. In his later period, Feyerabend denied he was ever a Popperian and even went as far as to remove favourable references to Popper in his early papers when they were reprinted in the collection of his **Philosophical Papers**. Preston gives an example of this. [33] Still, the evidence of an earlier Popperian Feyerabend is incontrovertible.

We can illustrate Feyerabend's Popperian outlook in his letters to Kuhn by a passage which criticizes Kuhn's account of the pre-paradigmatic period of a discipline. In Section 1, I illustrated this part of Kuhn's theory by Kuhn's example of theories of light up to 1700. There was no single paradigm and discussions of light were carried out by different schools with different views. There was much debate about fundamentals. These features make the study of light up to 1700 for Kuhn immature and not fully scientific. Real scientific progress begins with the emergence of the first generally accepted paradigm about 1700. Feyerabend comments on this as follows:

> [T]he trouble of these earlier schools does not seem to me to lie in the fact that there were *many* of them and that people did not concentrate upon the elaboration of a *single* paradigm. The trouble of these earlier schools seems to me to lie in the fact that their assertions were *incapable of test*, that crucial experiments could therefore not be staged. [...] Not the absence of a *paradigm* makes these earlier researches seem too chaotic, but the absence of clear methods of test and *elimination*. [34]

So, according to Feyerabend, the earlier schools were unscientific because their theories were untestable. This, of course, is a completely Popperian position, and it is interesting that this was one of Feyerabend's earlier criticisms which was *not* repeated in his 1970 paper. Most of his earlier criticisms could, however, be carried over to the 1970 paper, but, as Hoyningen-Huene points out, this makes

[32] Imre Lakatos, "Proofs and Refutations (I)", *The British Journal for the Philosophy of Science* 1963, Vol. 14, No. 53, pp. 1–25; Imre Lakatos, "Proofs and Refutations (II)", *The British Journal for the Philosophy of Science* 1963, Vol. 14, No. 54, pp. 120–139; Imre Lakatos, "Proofs and Refutations (III)", *The British Journal for the Philosophy of Science* 1963, Vol. 14, No. 55, pp. 120–139; Imre Lakatos, "Proofs and Refutations (IV)", *The British Journal for the Philosophy of Science* 1964, Vol. 14, No. 56, pp. 296–342.

[33] See Preston, **Feyerabend...**, p. 213, fn. 9.

[34] 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 Part A* 1995, Vol. 26, No. 3, p. 359 [353–387], https://doi.org/10.1016/0039-3681(95)00005-8 [emphasis in the original].

them quite similar to the criticisms in 1970 of those who were still critical rationalists — namely, Popper and Watkins. As Hoyningen-Huene says:

> For Feyerabend, normal science is, to put it simply, a horror, just as it is for the other critical rationalists of the 1960s — especially Popper and Watkins. […] If Kuhn evaluates the dogmatic element of normal science positively, he shows, in the eyes of the critical rationalist, a fundamental violation of the scientific ethos, namely to be critical and undogmatic. [35]

What Hoyningen-Huene says here is completely borne out by the papers of Watkins and Popper in the 1970 collection. Watkins says that "Normal Science seems to me to be rather boring and unheroic", [36] and he goes on to argue that "**The Structure of Scientific Revolutions** contain many suggestions […] of a significant parallelism between […] Normal Science and theology". [37] Popper comments on normal science as follows:

> In my view the "normal" scientist, as Kuhn describes him, is a person one ought to be sorry for. […] I believe, and so do many others, that all teaching on the University level (and if possible below) should be training and encouragement in critical thinking. The "normal" scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. [38]

So "normal" science in Kuhn's sense is, according to Popper, the product of bad teaching and constitutes a danger to science.

The close links between Feyerabend's criticisms of Kuhn and the critical rationalist tradition are also stressed by Bschir. [39] Here he argues that Feyerabend's Anomaly Import Thesis has its origins in Popper 1957 paper "The Aim of Science". [40] In this paper, Popper argues that Newton's theory both explains Kepler's and Galileo's laws and corrects them. It shows why these laws hold approxi-

[35] Paul Hoyningen-Huene, "Paul Feyerabend…", pp. 108–109.

[36] John W. N. Watkins, "Against »Normal Science«", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Vol. 4, Cambridge University Press 1970, London — New York, p. 31 [25–37].

[37] Watkins, "Against »Normal Science«...", p. 33.

[38] Karl R. Popper, "Normal Science and its Dangers", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Vol. 4, Cambridge University Press 1970, London — New York, p. 52 [51–58].

[39] See Bschir, "Feyerabend and Popper…", pp. 24–55.

mately, but also shows that there will be some deviations from these laws in particular circumstances owing to gravitational attraction. Bschir comments:

> Popper [...] also shared the view that new and innovative alternatives are, at least in certain cases, necessary to unveil trouble spots in older theories. He uses the examples of Kepler and Galileo to point out that the failure of these theories, or rather the specific ways in which they failed, could only be understood once the Newtonian theory was available. Therefore, the idea of anomaly import is by no means incompatible with the critical rationalist view of science; it should rather be seen as a full articulation of the latter. [41]

Despite all these connections, it would be wrong to think that Feyerabend was still a critical rationalist when he wrote his 1970 paper. On the contrary, he gives strong indications in that paper that he has already moved to his later more radical position. Thus, he writes: "I want to argue that science both is, and should be, more irrational that Lakatos and Feyerabend$_1$ [...] are prepared to admit". [42] Feyerabend explains that "Feyerabend$_1$" is meant as an ironic reference to Lakatos' 1968 paper, where Lakatos speaks of Popper$_0$, Popper$_1$ and Popper$_2$. [43] Yet, though Feyerabend is speaking ironically, the use of subscripts seems quite appropriate in his case. Feyerabend$_1$ would be Feyerabend the critical rationalist up to about the mid-1960s, whereas Feyerabend$_2$ would be the more familiar and more radical later Feyerabend. Feyerabend$_2$ seems responsible for the following remark: "scientific method, as softened up by Lakatos, is but an ornament which makes us forget that a position of »anything goes« has in fact been adopted". [44]

---

[40] See Karl R. Popper, "The Aim of Science", in: Karl R. Popper, **Objective Knowledge: An Evolutionary Approach**, Oxford University Press, Oxford 1972, pp. 191–205.

[41] Bschir, "Feyerabend and Popper...", p. 51.

[42] Feyerabend, "Consolations for...", pp. 214–215.

[43] See Imre Lakatos, "Criticism and the Methodology of Scientific Research Programmes", *Proceedings of the Aristotelian Society* 1968, Vol. 69, pp. 315–417.

[44] Feyerabend, "Consolations for...", p. 229 [197–230]; Feyerabend and Lakatos exerted a strong influence on each other in the period 1968–1974, as is shown by their correspondence during those years, which was published in Imre Lakatos and Paul Feyerabend, **For and Against Method. Including Lakatos's Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence**, edited and with an Introduction by Matteo Motterlini, University of Chicago Press, Chicago 1999.

4. Kuhn's Reply

Let us now see how Kuhn replies to his critics in 1970. He responds to Feyerabend's first criticism as follows: "[A]n answer to what Feyerabend calls the ambiguity of my presentation. Are Kuhn's remarks about scientific development, he asks, to be read as descriptions or prescriptions? The answer, of course, is that they should be read in both ways at once". [45] Indeed, Kuhn describes many historical examples of normal science, but he also makes clear that he thinks normal science is helpful for the development of science. [46] Regarding the attacks on normal science by Feyerabend and the critical rationalists, he writes, rather sarcastically: "normal science […] calls forth some of the oddest rhetoric: normal science does not exist *and* is uninteresting". [47] This is a fair comment, since Feyerabend certainly considers normal science to be uninteresting, and writes: "And is it too much to be hoped that scientists thought likewise, and that normal periods, if they ever existed, cannot have lasted very long and cannot have extended over large fields either?". [48] Feyerabend seems to have had such an intense dislike of normal science, in Kuhn's sense, that he hoped that it hardly ever existed. Kuhn in his reply does not produce evidence for the existence of normal science, perhaps because he thinks that his earlier historical studies have shown beyond doubt that normal science does exist. It seems, however, worth considering in this context one of Kuhn's examples.

Perhaps the most convincing example of normal science given by Kuhn is astronomy in the period from about 1700 to about 1900. During these two hundred years nearly all astronomers accepted the paradigm of Newtonian mechanics and carried out their researches within its framework. This is undoubtedly normal science in Kuhn's sense, and yet this period gave rise to very interesting developments and discoveries in astronomy. In fact, Kuhn's analysis of why normal science can succeed applies particularly well to what is perhaps the most famous advance of this period — the discovery of Neptune. Kuhn emphasizes that normal

[45] Thomas S. Kuhn, "Reflections on my Critics", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Cambridge University Press, London 1970, p. 237 [231–278], https://doi.org/10.1017/CBO9781139171434.011.

[46] See Kuhn, **The Structure...**

[47] Kuhn, "Reflections on…", p. 233.

[48] Feyerabend, "Consolations for…", p. 207.

science focuses "attention upon a small range of relatively esoteric problems". [49] The esoteric problem which led to the discovery of Neptune arose because of small perturbations in the orbit of Uranus. Without the detailed development of the Newtonian mathematical apparatus, these perturbations would never have been detected. Nor would it have been possible to calculate that they could be caused by a hitherto unknown planet located in a specified position. The preceding developments of normal science were a precondition for the discovery of Neptune, and yet that discovery was a startling and dramatic one. So, it would seem that normal science not only exists but can be very interesting!

This conclusion needs a slight qualification in the light of Lakatos's paper "Newton's Effect on Scientific Standards", which was written in the years 1963-1964 but not published until 1978, after Lakatos's death. [50] This somewhat neglected but highly interesting paper was written in the years immediately following the publication of **The Structure of Scientific Revolutions** and contains a significant criticism of Kuhn's notion of normal science. This criticism is concerned with developments in astronomy in the eighteenth century. Lakatos begins by saying that in 1746, "Clairaut found that the progress of the Moon's apogee is in reality twice what would follow from Newton's theory, and he proposed an additional term to Newton's formula involving the inverse fourth power of the distance". [51] In other words, in the face of an anomaly, Clairaut, one of the leading scientists of the time, suggested a modification of Newton's law of gravity. Now, Newton's law of gravity was part of the dominant paradigm of the time, and so Clairaut was not acting as a normal scientist should have done. His suggestion did not prove successful, however, for, as Lakatos goes on to say:

> But as it turned out, Clairaut's mathematics was wrong, and in fact later a correct calculation was found among Newton's unpublished manuscripts. But even so, a small discrepancy remained: a "secular acceleration". In 1770 the Paris Academy put up a prize for the solution of this problem. Euler won this prize with an essay in which he first concluded that "it appears to be established, by indisputable evidence, that the secular inequality of the moon's motion cannot be produced by the [Newtonian]

[49] Kuhn, **The Structure of...**, p. 24.

[50] See Imre Lakatos, **The Methodology of Scientific Research Programmes. Philosophical Papers Vol 1**, edited by John Worrall and Gregory Currie, Cambridge University Press, Cambridge, New York, Port Chester, Melbourne, Sydney 1978, pp. 193–222.

[51] Lakatos, "Newton's Effect...", p. 219.

> forces of gravitation", and he proposed a rival formula again involving an additional term, which, in a sequel published a year later, he tried to explain from the resistance of Cartesian ether. However, Laplace in 1787 showed that the problem can be solved *better* within the Newtonian research programme. [52]

This historical example does have some features which Kuhn attributes to normal science, since it shows scientists focusing their attention upon a small range of relatively esoteric problems. However, it does not exhibit the respect which scientists are supposed to show to the dominant paradigm during a period of normal science. Once again, a leading scientist (Euler) was prepared to modify Newton's theory of gravity in order to explain a small observational anomaly, although, once again, the suggestion proved to be unsuccessful. Lakatos comments as follows: "Did Clairaut and Euler make a methodological blunder — as Kuhn would surely say — when they tried alternative research programmes to solve Newtonian puzzles and only wasted time, energy and talent?". [53] Of course, the answer to Lakatos's rhetorical question is obvious. Clairaut and Euler acted very reasonably. As a matter of fact, their suggested modifications of Newtonian theory were not successful, but this could not have been known in advance.

Such, then, is Lakatos' historical counterexample to Kuhn's normal science. How serious a problem does it pose for Kuhn's views? In my view the problem is not a very serious one. After all, the paradigm of Newtonian mechanics was accepted in astronomy for about two hundred years, and during that time the paradigm was challenged on only two occasions. So Kuhnian normal science holds to a high degree of approximation. Moreover, if Kuhnian normal science had been more rigidly enforced, this would not have held up the progress of science, since the problems on which Clairaut and Euler were working were eventually solved within the Newtonian paradigm.

Lakatos' historical example does not, in my view, lend support to Feyerabend's strategy of trying always to proliferate alternative theories. During the long period (c. 1700 to c. 1900) of Newtonian normal science, it would not have helped scientific progress if scientists had devoted a great deal of time and energy to proliferating alternative theories of mechanics and then debating the value of these alternatives as compared to Newtonian mechanics. In fact, it was only a long

[52] Lakatos, "Newton's Effect...", p. 219 [emphasis in the original].

[53] Lakatos, "Newton's Effect...", p. 219.

series of mathematical and empirical developments based upon Newtonian mechanics which created the possibility of creating radically new systems of mechanics (relativity and quantum mechanics) in the twentieth century. The kind of modification of Newtonian mechanics considered by Clairaut involved changing the inverse square law of gravitational attraction by adding a term in the inverse fourth power of the distance. Such a change is a very small one compared with the replacement of Newtonian mechanics by General Relativity, [54] and Clairaut clearly lacked the concepts needed for the Einsteinian change.

Although Lakatos' historical example does not support Feyerabend's position, it does suggest that the dogmatism of normal science should not be too rigid. Scientists should consider the possibility of now and again introducing hypotheses which contradict some features of the dominant paradigm. Such hypotheses may often prove unsuccessful, but occasionally they may be the beginning of some new and exciting revolutionary development. Moreover, by the same token, the scientific community should allow some dissidents who do not accept the general consensus. Some discipline may be required, but too much discipline can be counterproductive.

Let us next consider Feyerabend's alleged counterexample to Kuhn's normal science. Feyerabend argues that in the second third of the 19th century there were three different and mutually incompatible paradigms, associated with (i) mechanics, (ii) thermodynamics, and (iii) electrodynamics. Kuhn replies as follows: "until this century theories of matter have been a tool for scientists rather than a subject matter. That different specialities have chosen different tools and sometimes criticized each others' choices does not mean that they have not each been practising normal science". [55] This is rather cryptic, but the main point seems to me to be this. Paradigms, for Kuhn, are associated with different subject matters, and two different subject matters can have two different paradigms, while both practising normal science. For example, in the early nineteenth century the paradigm for astronomy was Newtonian mechanics, while that for light was the wave theory. The scientists in each area practised normal science, though with different paradigms.

[54] This point was made to me by Ladislav Kvasz, who has studied the magnitude of the changes introduced by scientific revolutions. See Ladislav Kvasz, "On Classification of Scientific Revolutions", *Journal for General Philosophy of Science* 1999, Vol. 30, No. 2, pp. 201–232, https://tiny.pl/c8pn5 [15.09.2023].

[55] Kuhn, "Reflections on…", p. 255.

Similarly, in the second third of the nineteenth century, the three different areas of mechanics, thermodynamics and electrodynamics had different paradigms, but the scientists in each area were still practising normal science in the framework of the paradigm appropriate to that area. So, Feyerabend's example is not a counterexample to Kuhn's normal science. To find such a counterexample, he would have to find a specific area where the dominant paradigm was challenged by an alternative paradigm, but this he has not done.

For this reason, Feyerabend's example and the associated anomaly import thesis (AIT) do not support his principle of proliferation. Still the example and the AIT are of considerable interest and do illustrate important principles of scientific method. One important such principle is the *domain interaction principle*. If two domains have developed separately but are brought into conjunction, this may well result in fruitful developments. This applies, as Feyerabend points out, to interaction between electrodynamics and mechanics which was part of the background to the emergence of special relativity. [56] The case of kinetic theory and thermodynamics is somewhat different and more similar to Popper's example of Newtonian theory in relation to Kepler's and Galileo's laws. Newton did not intend to change Kepler's laws. Indeed, he hoped to derive his theory of gravity from them. When his theory of gravity was introduced, however, it became clear that it necessitated corrections in both Kepler's and Galileo's laws. Similarly, the kinetic theory was introduced not with the intention of overthrowing phenomenological thermodynamics, but rather with the aim of providing it with a deeper explanation. However, this deeper explanation, while showing the laws of thermodynamics held approximately, also showed that a correction was needed to the second law of thermodynamics. Thus, Feyerabend's example of Brownian motion does not show that it is a good strategy to proliferate theories which contradict the dominant paradigm in normal science. However, it does show that it is a good strategy to look for deeper explanations of phenomenological theories.

We now come to what I regard as Feyerabend's strongest argument, which I have called his political argument. Kuhn responds to it as follows:

> If, as Feyerabend suggests, some social scientists take from me the view that they can improve the status of their field by first legislating agreement on fundamentals and then turning to puzzle solving, they are badly misconstruing my point. [...] Fortu-

[56] See Feyerabend, "Consolations for...", p. 208.

> nately, though no prescription will force it, the transition to maturity does come to many fields, and it is well worth waiting and struggling to attain. [57]

Unfortunately, this response seems a bit incoherent. Kuhn thinks that many fields do emerge from the immature pre-paradigmatic phase to the mature phase in which a single paradigm dominates. However, he does not think this can be achieved by forcing the researchers by political means to adopt a single paradigm. As he says, "no prescription will force it", but he adds "it is well worth [...] struggling to attain". But if it is worth struggling to attain, why not force it by prescription? The key question here is what methods are legitimate for attaining the transition to maturity? As I have already suggested, it seems obvious that consensus on accepting a paradigm is achieved legitimately if it is reached for good scientific reasons rather than being imposed by political means. Kuhn seems implicitly to accept this, because he considers what good scientific reasons might look like. As he says:

> There are [...] many good reasons for choosing one theory rather than another. [...] These are, furthermore, reasons of exactly the kind standard in philosophy of science: accuracy, scope, simplicity, fruitfulness, and the like. It is vitally important that scientists be taught to value these characteristics and that they be provided with examples that illustrate them in practice. If they did not hold values like these, their disciplines would develop very differently. [58]

However, Kuhn's list of good reasons seems rather arbitrary, and he does not elaborate his account of it. To make matters worse, he adds "Simplicity, scope, fruitfulness, and even accuracy can be judged quite differently [...] by different people''. [59] Moreover, Kuhn does not answer Feyerabend's point that the two theories may be incommensurable, making it difficult to compare them according to the kind of criteria standard in philosophy of science.

I conclude that Kuhn did not provide a very satisfactory answer to Feyerabend's political argument. This partly explains why some of Kuhn's followers reached the conclusion that paradigms are in fact accepted primarily for political reasons of various kinds. This is a conclusion with which Kuhn himself strongly

[57] Kuhn, "Reflections on...", p. 245.

[58] Kuhn, "Reflections on...", pp. 261–262.

[59] Kuhn, "Reflections on...", p. 262.

disagreed, and so do I. So, in the next section, I will attempt to sketch a more convincing answer to Feyerabend's political argument.

The idea behind my approach is to revive the old empiricist idea of the inductive justification of scientific theories by the results of observations and experiments (observation statements). Inductive justification was not a popular conception among the participants in the 1970 collection. However, it has recently acquired more currency because of the successes of AI. Contemporary AI is largely based on machine learning, which is just computer induction from data. If computers can get such good results by induction, then surely the concepts of induction and inductive justification must be of some value. Adopting them leads to a position which could be described as *empirical rationalism* (as opposed to *critical rationalism*). I will consider this position in the next section.

5. Empirical Rationalism

It was one of the main ideas of many of the empiricists of the Vienna Circle, notably Carnap, that scientific theories are justified inductively by their agreement with the results of observations and experiments. This inductive justification was connected with the concepts of empirical confirmation, and Carnap set out to explicate these concepts in his well-known book **Logical Foundations of Probability**. He writes: "One of the chief tasks of this book will be the explication of certain concepts which are connected with the scientific procedure of confirming or disconfirming hypotheses with the help of observations and which we therefore will briefly call *concepts of confirmation*". [60] Carnap is right to say that scientists do assess their theories as either confirmed or disconfirmed by observations, including the results of experiments. Sometimes alternative terms such as "support/undermine" or "corroborate/discorroborate" are used, but I will stick to the term "confirmation", except for a brief discussion of "corroboration" later on. Scientists use expressions such as confirm/disconfirm in an intuitive way, and the task of the philosopher of science is to explicate this practice by formulating a more explicit confirmation theory. In the confirmation theories produced by philosophers, the central concept is that of *degree of confirmation* of *h*, given *e,*

[60] Rudolf Carnap, **Logical Foundations of Probability**, 2nd edition, University of Chicago Press, Chicago 1950, p. 19.

which is written $C(h,e)$. Here, h is a scientific hypothesis, and, since we are dealing with empirical confirmation, e is a conjunction of the relevant observation statements. It is usually thought that in addition to the observational evidence e, some background knowledge k needs to be assumed, so that we should really write $C(h, e\&k)$. The background knowledge will, however, often be omitted for ease of writing, but it should not be forgotten.

Although it is usual to speak of the degree of confirmation of h given e, $C(h, e)$, it should not be assumed that this degree is exactly measurable. Normally only qualitative estimates can be given, such as that h is very well confirmed by the available evidence e, or that h is hardly confirmed at all by the available evidence, and so on. As I will argue later, however, there are some cases where a more precise measure of degree of confirmation can be introduced. We can now formulate the principle of what I will call *empirical rationalism*. This states that a rational human should believe in a scientific hypothesis to the extent that it is confirmed empirically. It could be formulated as follows: the degree to which it is reasonable to believe in h for someone who has evidence e and background knowledge k is $C(h,e\&k)$.

Belief I take to be connected to action, and so we can illustrate the principle of empirical rationalism by considering an example from scientific medicine. Suppose a pharmaceutical firm has developed a new drug x to treat some illness. Before x is put on the market, it is important to make sure that it does not have any harmful side effects. Let us therefore formulate the following hypothesis:

h_x: *x*, when taken in the appropriate dosage, does not have any harmful side effects.

Now before x can be put on the market h_x must, by law, be subjected to a series of severe tests — first with animals, and then in the form of clinical tests on humans. Only if h_x passes all these tests can x be marketed. To put it another way, x can only be put on the market if h_x has a sufficiently high degree of confirmation. This leads to the following principle, which is an instance of empirical rationalism:

Use, as the basis for action, theories which have a sufficiently high degree of confirmation.

What is meant by "sufficiently high degree of confirmation" is specified in the

case of drugs by the government regulations on what tests a new drug must pass before it can be put on the market. In general, it would be understood contextually as part of the practice of the area in question.

If empirical rationalism is accepted, then Feyerabend's political objection can easily be answered. In a revolution in the natural sciences, a new paradigm P_2 is accepted, not for political reasons, but because it is much better confirmed empirically than the old paradigm P_1. Empirical rationalism also shows that normal science is perfectly reasonable, and not the result of a dogmatic and uncritical attitude. If a paradigm has been very well-confirmed empirically, this of course does not mean that it is certain. Very well-confirmed theories have sometimes broken down in quite unexpected ways. However, if a theory is very well-confirmed, it is difficult to replace it by a new theory which is even better confirmed. This does occasionally happen. So, it may, in particular circumstances, be worthwhile for a scientist to try to develop such a theory. Yet because of the difficulties inherent in such a strategy, it is usually worth sticking to a very well-confirmed paradigm: that is to say, it is reasonable to continue with normal science.

Such, then, is the response, based on empirical rationalism, to Feyerabend's political argument. Needless to say, it would not be acceptable to many philosophers of science. There are two main objections. (1) The response is based on the notion of empirical confirmation, but it could be objected that this notion is a very confused and incoherent one. The various confirmation theories developed by philosophers of science disagree with each other, and this suggests that there might, after all, be no satisfactory concept of confirmation. (2) Even if there is a workable notion of confirmation, can it overcome the difficulties of incommensurability? In a revolution in the natural sciences, the old paradigm P_1 is incommensurable with the new paradigm P_2. Does this not make it impossible to compare the empirical confirmation of P_1 with that of P_2? I will now discuss these two objections in turn.

One interesting thing is that Kuhn appears to accept objection 1 to confirmation theory in **The Structure of Scientific Revolutions**. Kuhn never actually uses the term "confirmation", but he speaks of "probabilistic verification theories", which, as we shall see, are similar to Carnap's version of confirmation theory. Kuhn has this to say about such theories:

> Few philosophers of science still seek absolute criteria for the verification of scientific

> theories. Noting that no theory can ever be exposed to all possible relevant tests, they ask not whether a theory has been verified but rather about its probability in the light of the evidence that actually exists. [...] In their most usual forms, however, probabilistic verification theories all have recourse to one or another of the pure or neutral observation-languages [...]. If, as I have already urged, there can be no scientifically or empirically neutral system of language or concepts, then the proposed construction of alternate tests and theories must proceed from within one or another paradigm-based tradition. Thus restricted it would have no access to all possible experiences or to all possible theories. As a result, probabilistic theories disguise the verification situation as much as they illuminate it. [61]

Note that Kuhn thinks that probabilistic verification theories all depend on the existence of a pure or neutral observation-language, but he denies that such a language can exist because observations are always made within a particular paradigm. This is a good criticism, and I will try to answer it later on.

Kuhn then goes on to consider Popper's views. He first makes the point that what he calls "anomalies" have some points in common with what Popper calls "falsifications". However, Kuhn then continues:

> If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times. On the other hand, if only severe failure to fit justifies theory rejection, then the Popperians will require some criterion of "improbability" or of "degree of falsification". In developing one they will almost certainly encounter the same network of difficulties that has haunted the advocates of the various probabilistic verification theories. [62]

These passages show that Kuhn, in 1962, was very doubtful about the possibility of a confirmation theory either of the Carnapian or the Popperian kind. In fact, the main investigations of confirmation theory in the 1950s were carried out in Carnap and Popper. [63] A careful inspection of these works makes Kuhn's scepticism about confirmation theory highly comprehensible.

Carnap's 1950 book is 613 pages long and filled from beginning to end with complicated formulas taken from mathematical logic and probability theory. Despite this complexity, the formal system presented is inadequate to express many

[61] Kuhn, **The Structure of...**, pp. 144–145.

[62] Kuhn, **The Structure of...**, p. 145–146.

[63] See Carnap, **Logical Foundations...**; Karl R. Poper, **The Logic of Scientific Discovery**, 6th (revised) impression of the 1959 english translation, first edition 1934, Hutchinson, London 1972.

standard scientific generalisations — namely, those which involve continuous parameters. In addition to this, Popper, in his "New Appendices" of 1959, launched very harsh attacks on Carnap's confirmation theory. [64] Popper claimed that this theory was completely wrong, and he adopted the new term "corroboration" to distinguish his version of confirmation theory from that of Carnap. Popper's treatment is no less formal than Carnap's and, in particular, Popper gives a very complicated formula for degree of corroboration. [65] Confirmation theory cannot have seemed to Kuhn in 1962 a very flourishing enterprise. Yet I will now argue that the notion of empirical confirmation is much more defensible than Kuhn perhaps imagined.

I will begin by discussing Popper's objections to Carnap. There are two key ones. The first is that Carnap assumes that his confirmation function $C(h,e)$ satisfies the usual axioms of probability, or, in symbols $C(h,e) = P(h|e)$. This is the characteristic assumption of the Bayesian school. So, Carnap advocates a Bayesian confirmation theory. Popper, on the other hand, has a series of arguments against Bayesianism. So, he holds that $C(h,e)$ is not a probability function, $C(h,e)$ is not equal to $P(h|e)$. Rather than using a different term ("corroboration") for Popper's approach, it seems to me better to use "confirmation" and "corroboration" as synonyms represented by the C-function $C(h,e)$. Popper's confirmation theory is then distinguished from Carnap's by saying that Carnap advocates a Bayesian confirmation theory, while Popper advocates a non-Bayesian confirmation theory.

The existence of these two approaches to confirmation theory is perhaps less damaging than it might at first seem, because it is only in special circumstances that degree of confirmation can be measured and the qualitative considerations underlying the two approaches may well have many points in common. Moreover, it is possible that a Bayesian approach is appropriate in some circumstances and a non-Bayesian approach in others. Before exploring these matters further, I will mention Popper's second objection to Carnap, because this is, in some ways, the most relevant to the present paper.

Popper's second objection is connected with the question of whether confirmation has an inductive significance. Most of those working on confirmation the-

[64] See Karl R. Popper, **The Logic...**

[65] See Popper, **The Logic of...**, p. 400.

ory assume that this is the case. Suppose a theory has a high degree of confirmation. This means that it has explained correctly the results of past observations, and perhaps also given the correct predictions in a number of tests. Let us say, in these circumstances, that the theory has so far performed well. However, if we adopt the theory as the basis for actions, are we not assuming that it will continue to perform well in the future? An empirical rationalist is definitely assuming that the best guides to future action are well-confirmed theories. So, for an empirical rationalist, confirmation does indeed have inductive significance and confirmation provides an inductive justification for a theory. Popper, however, was always an enemy of induction in all its forms. So, he is very reluctant to accept this conclusion. It is true that, at one point, he seems to come close to giving an inductive significance to his measure of corroboration. [66] However, his considered opinion is surely that expressed as follows: "Corroboration (or degree of corroboration) is thus an evaluating *report of past performance*. [...] Being a report of past performance only, [...] *it says nothing whatever about future performance*". [67] This point is very important for distinguishing Popper's *critical rationalism* from *empirical rationalism*. Since Popper rejected any form of inductive justification, he considered rationality to consist in the critical attitude. Thus, normal science seemed to him to be an example of dogmatism rather than criticism, and so inadmissible in science. For an empirical rationalist, a normal scientist is acting perfectly rationally in accepting provisionally a paradigm which has been very well confirmed empirically.

My own view is that confirmation does have an inductive significance, and I argue for this in detail in Gillies. [68] So, on this point I side with Carnap against Popper. On the other hand, I think that at least one of Popper's arguments against Bayesianism is valid, and that therefore a non-Bayesian confirmation theory is preferable to a Bayesian confirmation theory. This is argued in detail in Gillies, where I present a non-Bayesian measure of confirmation which is a development

[66] See Popper, **The Logic of...**, p. 418.

[67] Karl R. Popper, **Objective Knowledge: An Evolutionary Approach**, Oxford University Press, Oxford 1972, p. 18 (emphasis in the original).

[68] See Donald Gillies, "Problem-Solving and the Problem of Induction", in: Zuzana Parusniková and Robert S. Cohen (eds.), **Rethinking Popper**, *Boston Studies in the Philosophy of Science*, Vol. 272, Springer, Dordrecht 2009, pp. 103–115, https://doi.org/10.1007/978-1-4020-9338-8.

and simplification of Popper's corroboration measure. [69]

I mentioned earlier that there is one area in which an exact measure of confirmation can be used. This is, of course, artificial intelligence (AI). A large part of AI is based on machine learning or computer induction. AI programs are precise and formal in character, and so one can apply exact measures of empirical confirmation in this particular context. In Gillies, I showed that my modification of Popper's corroboration measure worked very well in the context of a leading machine learning program of the time. [70] Bayesian measures have also been used for machine learning. In this paper, however, we are concerned with problems which have arisen in connection with science carried out by humans in which precise measures of empirical confirmation have not been used. What we need here are general qualitative principles underlying judgements that one theory is better confirmed empirically than another in the light of existing evidence. Luckily, however, the search for precise measures of confirmation has led to the formulation of a number of such general qualitative principles. I will next give a brief description of some of these.

The first is *the principle of severe testing*, which is largely due to Popper. It states that if a theory has passed a number of severe tests, it becomes well confirmed. We saw an instance of this in the confirmation of the hypothesis h_x: that *x*, a new drug, when taken in the appropriate dosage, does not have any harmful side effects. This principle depends on the notion of a *severe test*, but this seemingly vague notion has been given a quite precise explication. Let e be the result of a test of a hypothesis h, given background knowledge k. If e is very improbable given k, i.e. $P(e|k)$ is low, but e is very probable given h, i.e. $P(e|h\&k)$ is high, then the test is severe. A nice historical example is provided by the famous test of Fresnel's wave theory of light. Poisson deduced from this theory that if a ball bearing cast a circular shadow, then, under some circumstances, a bright spot of light should appear at the exact centre of this shadow. This result was regarded as highly improbable on background knowledge, yet when the experiment was carried out the bright spot did indeed appear at the centre of the shadow. This notion

[69] See Donald Gillies, "Confirmation Theory", in: Dov M. Gabbay and Philippe Smets (eds.), **Handbook of Defeasible Reasoning and Uncertainty Management Systems,** *Quantified Representation of Uncertainty and Imprecision*, Vol. 7, Kluwer, Dordrecht — London 1998, pp. 135–167.

[70] See Gillies, "Confirmation Theory…", pp. 135–167.

of a severe test was introduced by Popper, but it is implicitly endorsed also by Bayesianism. [71] This is an instance where the two approaches agree qualitatively.

A second principle is concerned with the successful explanation of already established facts, and has been called *the principle of explanatory surplus*. Suppose that a number of facts $f_1, f_2, \ldots f_n$ have been established in the sense that they have been well confirmed by observation and/or experiment, and so can be assumed to be true (at least when interpreted as approximations) while the attempt at theoretical explanation is being made. Then a theory is confirmed if it explains those facts using fewer assumptions that the number of facts explained, or, in other words, if the theory generates an explanatory surplus. This can be illustrated by a simple example. Suppose our theory is a linear model of the form $y = ax + b$, and we are considering whether it is confirmed by explaining n facts taking the form of observed values of y for different values of x. If we have only two such facts, then clearly our theory is not confirmed, because any two points can be connected by a line simply by adjusting the parameters a and b. On the other hand, if we have 10 facts, then two of them are sufficient to fix a and b, and if the resulting line goes through the other 8 points, we have generated an explanatory surplus of 8 facts and these confirm our hypothesis. This principle is closely connected with the criterion of simplicity mentioned by Kuhn.

A third principle could be called *the principle of precision*. It states that if a theory succeeds in making a very precise prediction or explanation then it is more strongly confirmed than it would be by less precise predictions or explanations. A "precise explanation" can be characterised as follows. Suppose physicists are studying a particular phenomenon, and connected with this phenomenon there is a parameter — θ, say — which can be measured very precisely. If there is a mathematical theory — T, say — of the phenomenon in question from which a theoretical value for θ can be derived, and if this theoretical value agrees with the observed value within the limits of experimental error, then T gives a precise explanation of θ.

A famous example of a precise explanation concerned the motion of the perihelion of the planet Mercury. The perihelion of a planet is the point at which it is closest to the Sun. The motion of the perihelion of Mercury was calculated using

[71] See Gillies, "Confirmation Theory…", p. 158.

Newtonian theory in the 19th century, but the theoretical value differed from the observed value by a small amount. Newcomb, in 1898, gave the value of this discrepancy as 41.24´´ ± 2.09´´ per century; that is, less than an eightieth part of a degree per century. This is a tiny anomaly, and yet even this anomaly was successfully explained by the general theory of relativity which Einstein introduced in 1915. Einstein's calculations using his new mathematics gave a value for the anomalous advance of the perihelion of Mercury as 42.89´´ per century — a figure well within the bounds set by Newcomb. The principle of precision is closely connected with Kuhn's criterion of accuracy.

Even someone like Kuhn, who is sceptical about precise measures of degree of empirical confirmation, will surely admit that the principles just stated are implicitly assumed by scientists and used by them to assess qualitatively the degree of confirmation of theories. Kuhn says, of his good reasons for choosing one theory rather than another such as "simplicity" and "accuracy", that: "It is vitally important that scientists be taught to value these characteristics and that they be provided with examples that illustrate them in practice". [72] I would say that scientists in their training are taught to value the empirical confirmation of theories and are provided with examples that illustrate how empirical confirmation is assessed in practice. In effect, they are taught and adopt empirical rationality.

But now we come to the last hurdle: incommensurability. Kuhn argues that to compare the confirmation of two theories, a neutral observation language is needed, but there is no such language. Given two different paradigms P_1 and P_2, Kuhn argues that the observations made by adherents of P_1 are made within P_1, while the observations made by adherents of P_2 are made within P_2. If P_1 and P_2 are incommensurable, there is no way that the empirical confirmation of P_1 can be compared with that of P_2. Feyerabend gives the example of classical celestial mechanics (CM), i.e. Newtonian mechanics, and the special theory of relativity (SR). He regards these two theories as incommensurable, and writes:

> The concept of length as used in SR and the concept of length as presupposed in CM are different concepts. Different magnitudes based on different concepts may give identical values on their respective scales without ceasing to be different magnitudes (the same remark applies to the attempt to identify classical mass with relative *rest* mass). [73]

[72] Kuhn, "Reflections on…", p. 261.

Given this situation, how can it be claimed that SR is better confirmed by observations than CM?

To try to answer this difficulty, let us suppose, then, that we have two incommensurable scientific theories *T* and *T'*, which could be Feyerabend's CM and SR. Since the theories are scientific, they will each contain a set of observation statements, {O} and {O'}. An observation statement is one whose truth-value, whether true or false, can in practice be decided by the scientific community on the basis of observation and experiment. I will assume, following Feyerabend and Kuhn, that the observation statements of *T* are made in the language of *T*, and those of *T'* in the language of *T'*. Thus, in Feyerabend's example, if a particular observation statement is "The mass of this body is 2.5 grams", we will assume that, within *T*, mass will be understood in the sense of CM, yielding the observation statement *O*, while within *T'*, mass will be understood in the sense of SR, yielding the observation statement *O'*. Now *O* and *O'* have different meanings, but, nonetheless, if we are dealing with an ordinary medium-sized body moving with a low velocity, then the adherents of *T'* would certainly agree to give the same truth-value to *O'* as the adherents of *T* give to *O*, on the basis of making the same observations and experiments. Thus, these two observation statements would be ascribed the same truth-value by the two camps, a situation which we could describe by writing O ~ *O'*. Generalising, we could establish a sequence of observation statements of *T*, O_1, O_2, ..., O_n, ... say, and a corresponding sequence of observation statements of *T'*, O_1', O_2', ..., O_n', ... say, such that $O_n \sim O_n'$. It now becomes easy to compare *T* and *T'* empirically. We work out how well *T* is confirmed (or disconfirmed) by the sequence O_1, O_2, ..., O_n, ..., and then how well T' is confirmed (or disconfirmed) by the sequence O_1', O_2', ..., O_n', ... If one of the two theories has a very much higher degree of confirmation than the other, it becomes rational to accept it in preference to the other. This is just empirical rationality, and no appeal to political reasons is needed here.

6. Conclusions

In this paper I have argued that Feyerabend's criticisms of Kuhn are of very great force. Kuhn was able to answer some of them, but not all, and this resulted

[73] Feyerabend, "Consolations for ...", pp. 221–222 [emphasis in the original].

in a weakness in Kuhn's position which led to its being developed in ways of which Kuhn did not approve. Many of Feyerabend's criticisms were supported by Popper's critical rationalism, even though Feyerabend himself had moved away from that position by 1970, when he published his main paper criticizing Kuhn. I have argued that Feyerabend's criticisms of Kuhn can be answered by moving from critical rationalism to empirical rationalism, a position which accepts that scientific theories can be justified inductively by the results of observation and experiment, using the concept of empirical confirmation. It seems unlikely that Kuhn himself would have accepted such an answer, because it downplays the notion of incommensurability and accepts the notion of empirical confirmation which Kuhn himself criticized and rejected. Thus, I have ended up defending a Kuhnian position rather than Kuhn's own views. This Kuhnian position accepts Kuhn's basic model of the development of the natural sciences as consisting of periods of normal science punctuated by occasional revolutions. However, it claims that, in a revolution, the new paradigm is better confirmed empirically than the old paradigm, and this is the reason why it is accepted by the scientific community. So scientific revolutions are rational. They embody empirical rationality.

Donald Gillies

References

1. Bschir Karim, "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(spring), pp. 24–55, https://doi.org/10.1086/680368.
2. Carnap Rudolf, **Logical Foundations of Probability**, 2nd edition, University of Chicago Press, Chicago 1950.
3. Collodel Matteo, "Was Feyerabend a Popperian? Methodological Issues in the History of the Philosophy of Science", *Studies in History and Philosophy of Science Part A* 2016, Vol. 57, pp. 27–56.
4. Feyerabend Paul K., "Consolations for the Specialist", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Vol. 4, Cambridge University Press 1970, London — New York, pp. 197–230.
5. Feyerabend Paul K., **Against Method: Outline of an Anarchist Theory of Knowledge**, New Left Books, London 1975.

6. Feyerabend Paul K., **Killing Time: The Autobiography of Paul Feyerabend**, Chicago University Press, Chicago 1995.

7. Gillies Donald, "Confirmation Theory", in: Dov M. Gabbay and Philippe Smets (eds.), **Handbook of Defeasible Reasoning and Uncertainty Management Systems,** *Quantified Representation of Uncertainty and Imprecision*, Vol. 7, Kluwer, Dordrecht — London 1998, pp. 135–167.

8. Gillies Donald, "Problem-Solving and the Problem of Induction", in: Zuzana Parusniková and Robert S. Cohen (eds.), **Rethinking Popper**, *Boston Studies in the Philosophy of Science*, Vol. 272, Springer, Dordrecht 2009, pp. 103–115, https://doi.org/10.1007/978-1-4020-9338-8.

9. Hoyningen-Huene Paul, "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 Part A* 1995, Vol. 26, No. 3, pp. 353–387, https://doi.org/10.1016/0039-3681(95)00005-8.

10. Hoyningen-Huene Paul,"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 — Oxford 2000, pp. 102–114.

11. Kuhn Thomas S., **The Copernican Revolution: Planetary Astronomy in the Development of Western Thought**, first edition in 1957, Vintage Books, Cambridge 1959.

12. Kuhn Thomas S., "The Essential Tension: Tradition and Innovation in Scientific Research", in: Thomas S. Kuhn (ed.), **The Essential Tension: Selected Studies in Scientific Tradition and Change**, University of Chicago Press, Chicago 1959, pp. 225–239.

13. Kuhn Thomas S., **The Structure of Scientific Revolutions**, The University of Chicago Press, Chicago — London 1962.

14. Kuhn Thomas S., "Reflections on my Critics", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Cambridge University Press, London 1970, pp. 231–278, https://doi.org/10.1017/CBO9781139171434.011.

15. Kvasz Ladislav, "On Classification of Scientific Revolutions", *Journal for General Philosophy of Science* 1999, Vol. 30, No. 2, pp. 201–232, https://tiny.pl/c8pn5 [15.09.2023].

16. Lakatos Imre, "Proofs and Refutations (I)", *The British Journal for the Philosophy of Science* 1963, Vol. 14, No. 53, pp. 1–25.

17. Lakatos Imre, "Proofs and Refutations (II)", *The British Journal for the Philosophy of Science* 1963, Vol. 14, No. 54, pp. 120–139.

18. Lakatos Imre, “Proofs and Refutations (III)”, *The British Journal for the Philosophy of Science* 1963, Vol. 14, No. 55, pp. 120–139.

19. Lakatos Imre , “Proofs and Refutations (IV)”, *The British Journal for the Philosophy of Science* 1964, Vol. 14, No. 56, pp. 296–342.

20. Lakatos Imre, “Criticism and the Methodology of Scientific Research Programmes”, *Proceedings of the Aristotelian Society, New Series* 1968, Vol. 69, pp. 315–417.

21. Lakatos Imre, “Newton’s Effect on Scientific Standards”, in: Imre Lakatos (ed.), **The methodology of scientific research**, Cambridge University Press, New York 1978a, pp. 193–222.

22. Lakatos Imre, **The Methodology of Scientific Research Programmes. Philosophical Papers Vol 1**, edited by John Worrall and Gregory Currie, Cambridge University Press, Cambridge, New York, Port Chester, Melbourne, Sydney 1978, pp. 193–222.

23. Imre Lakatos and Paul Feyerabend, **For and Against Method. Including Lakatos’s Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence**, edited and with an Introduction by Matteo Motterlini, University of Chicago Press, Chicago 1999.

24. Oberheim Eric, **Feyerabend’s Philosophy**, *Quellen und Studien zur Philosophie,* Vol. 73, Walter de Gruyter, Berlin 2006, https://doi.org/10.1515/9783110891768.

25. Popper Karl R., **The Logic of Scientific Discovery**, 6th (revised) impression of the 1959 english translation, first edition 1934, Hutchinson, London 1972.

26. Popper Karl R., **The Aim of Science**, in: Karl R. Popper, **Objective Knowledge: An Evolutionary Approach**, Oxford University Press, Oxford 1972, pp. 191–205.

27. Popper Karl R., “New Appendices”, in: Karl R. Popper, **The Logic of Scientific Discovery**, 6th (revised) impression of the 1959 english translation, Hutchinson, London 1972, pp. 307–464.

28. Popper Karl R., “Normal Science and its Dangers”, in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Vol. 4, Cambridge University Press 1970, London — New York, pp. 51–58.

29. Popper Karl R., **Objective Knowledge: An Evolutionary Approach**, Oxford University Press, Oxford 1972.

30. Preston Jonh, **Feyerabend: Philosophy, Science and Society**, Polity Press, Cambridge 1997.

31. Sheehan Helena, **Marxism and the Philosophy of Science: A Critical History**, Humanities Press, Atlantic Highlands 1985.

32. Watkins John W. N., "Against »Normal Science«", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, Vol. 4, Cambridge University Press 1970, London — New York, pp. 25–37.

Early Philosophy

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 159–163

https://doi.org/10.53763/fag.2022.20.2.231

LIST DO REDAKCJI / LETTER TO THE EDITOR

Dawid Lamb

University of Manchester

Feyerabend Letter: Some Thoughts on the Two Context Distinction

Received: March 08, 2024. Accepted: March 15, 2024. Published online: May 2, 2024.

The thesis that scientific and artistic discovery is amenable to rational analysis has not enjoyed much popularity with philosophers of science. This is due to two factors: a romantic belief in creative genius and the influence of logical empiricism in the early twentieth century which ruled that logic of science is strictly logic of justification. Consequently, scientific discovery was held to be of interest only to historians, psychologists and sociologists, and excluded from the topics which demand logical analysis by philosophers. Against this view it can be argued that the processes of discovery and creativity are fit subjects for philosophical analysis and that there is no qualitative distinction between the contexts of discovery and justification.

A full examination of responses to the two context distinction lies outside the scope of this letter but further details can be found in Lamb. [1] We shall briefly examine here the positions taken by Popper and Feyerabend with regard to the two context distinction.

[1] David Lamb, **Discovery, Creativity and Problem Solving**, Avebury, Aldershot 1991.

Popper

According to Popper it is only the context of justification (or falsification) where completed hypotheses are presented which are of interest to the philosopher. There can be no question of analysis of the creative process. Popper's discussion of the logic of discovery is summarised here:

> [...] the work of the scientist consists in putting forward and testing theories. The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man — whether it is a musical theme, a dramatic conflict, or a scientific theory — may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. This latter is concerned not with questions of fact (Kant's *quid facti*?), but only with questions of justification or validity (Kant's *quid juris*?) [...] Accordingly, I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. As to the task of the logic of knowledge — in contradistinction to the psychology of knowledge — I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained [...] my view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every discovery contains "an irrational element", or "a creative intuition", in Bergson's sense. In a similar way Einstein speaks of the "search for those highly universal laws [...] from which a picture of the world can be obtained by pure deduction. There is no logical path", he says, "leading to these [...] laws. They can only be reached by intuition, based upon something like an intellectual love (»Einfühlung«) of the objects of experience". [2]

Feyerabend

Whilst Feyerabend appears to share Popper's argument that discovery involves an irrational element, he indicates an affinity between the history of science and the logic of science, arguing that the latter, as it is currently understood, cannot deal adequately with philosophical problems arising out of the practice of science. He is scornful of philosophers who have employed the two context distinction to exclude studies in the history of science and interest in what are tradi-

[2] Karl R. Popper, **The Logic of Discovery**, Routledge, Taylor and Francis, London — New York 2005, p. 7–9.

tionally described as extra-scientific factors. He cites the following remarks as representative of the two context distinction.

> The model of the DNA molecule worked out by Crick and Watson stands on its own merits [...] The [...] story of how the DNA model was achieved, humanly fascinating as it may be, has little relevance for the operational concept of science. [3]

Feyerabend acknowledges that "most philosophers of science would agree" that only "the context of justification explains its content and reasons for accepting it". [4] Nevertheless, he insists that science is not autonomous with respect to either the genesis or justification of its products. Knowledge, he argues, is an open ended "living discourse" which is exemplified in recent pure mathematics where workshops, conferences and seminar meetings "do not merely add information to the content of textbooks and research papers, they explain this content and make it clear that it cannot stand on its own two feet". [5] For Feyerabend there is no essential difference between discovery and justification because "anything goes" in either stage. Thus:

> Galileo prevails because of his style and his clever techniques of persuasion, because he writes in Italian rather than Latin, and because he appeals to people who are temperamentally opposed to the old ideas and the standards of learning connected with them. [6]

But despite Feyerabend's rejection of the logic of falsification he nevertheless shares Popper's irrationalist concept of discovery.

> It is clear that allegiance to the new ideas will [...] be brought about by means other than arguments. It will [...] be brought about by irrational *means* such as propaganda, emotion, and ad hoc hypotheses, and appeal to prejudices of all kinds. We need these "irrational means" to uphold what is nothing but blind faith.[7]

---

[3] Salvador S. Luria, **A Slot Machine: A Broken Test Tube**, Harper Collins, New York 1985, p. 125.

[4] Paul K. Feyerabend, **Farewell to Reason**, Verso, London 1987, p. 110.

[5] Feyerabend, **Farewell to Reason...**, p. 111.

[6] Paul K. Feyerabend, **Against Method: Outline of an Anarchistic Theory of Knowledge**, New Left Books, London 1975, p. 141.

[7] Feyerabend, **Against Method...**, p. 154.

Whereas Popper locates faith and inclinations in the context of discovery, Feyerabend proclaims their centrality in both contexts.

> What our historic examples seem to show is this: there are situations when our most liberal judgements [...] would have eliminated an idea or a point of view which we regard today as essential for science [...] The ideas survived and they can now be said to have been in agreement with reason. They survived because prejudice, passion, conceit, errors, sheer pigheadedness, all the errors which characterise the context of discovery, opposed the dictates of reason. Copernicanism and other "rational" views exist today *only because reason was overruled at some time in their past* [...] Hence it is advisable to let one's inclinations go against reason in many circumstances, for science may profit from it. [8]

According to Feyerabend scientists normally behave, and ought to behave, at all times in the manner which Popper attributes only to the context of discovery. A methodology which actually embraces requirements for the context of justification would, if seriously implemented, strike a death blow to scientific research:

> A determined application of the methods and criticism and proof, which are said to belong to the context of justification, would wipe out science as we know it — and would never have permitted it to arise. [9]

Rejecting the theory-observation distinction, which was once the cornerstone of logical empiricism, Feyerabend's espousal of the theory-loaded character of data rules out the distinction between discovery and justification. Observation is determined by a theory whose criteria of justification and proof are self-determined. To put it more explicitly: conceptual advances in science contribute to a transformation of criteria for justification, and it is these advances which determine the relevant justifying observations. Thus Galileo's belief in the observational reliability of the new telescope was co-emergent with the new theory it was intended to prove. Each new conceptual standpoint provides confirming criteria of justification and proof. Radically new theories transform both observational terms and objects simultaneously with their theoretical counterparts. Discovery and justification are simultaneous.

Yet despite his refreshing destruction of restrictive methodologies and con-

[8] Feyerabend, **Against Method...**, pp. 155–156.

[9] Feyerabend, **Against Method...**, p. 166.

cepts of rationality Feyerabend has little to say with regard to the genesis of new ideas. He did not produce a theory of how discoveries are made. His case studies and arguments show how they are not made; that is, new ideas do not develop — nor could they survive — within the requirements of creative conjecture and refutation.

David Lamb

References

1. Lamb David, **Discovery, Creativity and Problem Solving**, Avebury, Aldershot 1991.
2. Luria Salvador S., **A Slot Machine: A Broken Test Tube**, Harper Collins, New York 1985.
3. Feyerabend Paul K., **Against Method: Outline of an Anarchistic Theory of Knowledge**, New Left Books, London 1975.
4. Feyerabend Paul K., **Farewell to Reason**, Verso, London 1987.
5. Popper Karl R., **The Logic of Discovery**, Routledge, Taylor and Francis, London — New York 2005.

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 165–183

https://doi.org/10.53763/fag.2023.20.2.225

ARTYKUŁ ORYGINALNY / ORIGINAL ARTICLE

George Couvalis

Independent Researcher

Riffing on Feyerabend: Direct Observation, Paraconsistentist Logic, and a Research Immanent Account of the Rationaliy of Science

Received: August 30, 2023. Accepted: September 20, 2023. Published online: February 15, 2024.

Abstract: Feyerabend's work, particularly his early papers contain important insights into the nature of science and scientific progress. I discuss his insights into the limits of empiricist foundationalism and positivism. I explain how the work of a number of philosophers has borne out Feyerabend's claims in startling and interesting ways. Nevertheless, I criticise Feyerabend's move from his attack on universal method to relativism. I point out that Feyerabend never confronted a well-developed research immanent view of the rationality of scientific change, which shows the limitations of the arguments in **Against Method**.

Keywords:

Feyerabend; history and philosophy of science; inconsistency; paraconsistency; philosophy of science; rationality; Shapere

Introduction

A foundation is a kind of origin. Feyerabend is an important critic of various kinds of foundationalism, theories which tell us the basis from which knowledge must be derived. He focussed on empiricist foundationalism, the theory that the basis for our knowledge and the meaning of key terms in our knowledge is derived from experience. After Feyerabend wrote many of his key works, his insights were deepened by other authors. I will here discuss some of his neglected papers, neglected aspects of **Against Method**, and the work of other authors who

have deepened and strengthened the accounts Feyerabend presented. Many of these authors do not refer to Feyerabend's work and may well have been unaware of much of it. Nevertheless, Feyerabend had the important insights before them. I will also discuss an important challenge to Feyerabend's argument in **Against Method** that Feyerabend never discussed. I will not here discuss Feyerabend's relativism in any detail. I have elsewhere argued against Feyerabend's relativism. [1]

Feyerabend on Observation

What is directly observable and what do scientific theories describe? During the high tide of positivism, it was argued that what was directly observable were certain experiences. The semantics of a scientific theory were partly or wholly dependent on a direct tie to the content of experiences. The experiences are the foundation of our knowledge. Hanson, Popper and others argued for the claim that our observations are theory-laden. That is, that part of the content of experience is and must be dependent on background theories. This view was present already in Kant and in the influential nineteenth century philosopher/historian of science, William Whewell. It has become the accepted view. While Quine produced an influential critique of positivist dogmas of empiricism, he still declared that "[A]s an empiricist I continue to think of science as a tool, ultimately, for predicting future experience in the light of past experience". On the same page, he claimed that physical objects should be regarded as irreducible posits like the gods of Homer. Myths that are no doubt more useful than the gods of Homer, but still just useful myths. He further declared that posits at the atomic level or below should be treated in a similar manner. [2]

In some important, but now neglected early papers, Feyerabend went further than Quine, or those who argued for theory-ladenness. In "The Problem of Theoretical Entities", Feyerabend argued that various claims can be tested by using the

[1] See George Couvalis, **Feyerabend's Critique of Foundationalism**, Avebury, Aldershot 1989, pp. 136–143; George Couvalis, **The Philosophy of Science**, Sage, London 1997, pp. 111–139.

[2] See Willard Quine, **From a Logical Point of View**, Norton, New York 1961, p. 44.

same test, so that there is no conceptual connection between experience and what is being tested. For instance,

> "lifting a suitcase does not serve only to test the suitcase's weight. For example, after a long illness, we can lift a suitcase of a known weight as a test of our own strength and *not* as a test of the weight of the suitcase [...] Or we lift the suitcase of a person who does not have a friendly disposition toward us, and we test his patience or our own nerve [...] the object being observed depends on the *problem* present, and that this object is not given by the simple act of observation [...] Thus, we can conceive of lifting a suitcase as an observation of the intensity of the gravitational field at the location of this action [...] (a more realistic example is the direct observation of a supernova by observing the sudden increase in brightness of a point of light in the sky)". [3]

An important point being made by this argument is that there is only a causal connection between a test and what is being tested, not a connection of meaning. What counts as a direct observation of something depends, as he says, on the problem we are dealing with — the theory we are testing. This means that experience cannot be a foundation of knowledge in the way in which positivist theory describes.

In another early paper, "On the interpretation of scientific theories", Feyerabend pointed out that the positivist account made statements describing causally independent situations semantically dependent. There is a conflict between doing properly scientific observations and positivist theory. Take one of his examples. Suppose we are doing celestial mechanics and trying to work out the mass of the sun from observations with a telescope and other data. We will have to allow for the refractive index of the earth's atmosphere, perceptual illusions caused by the functioning of our eyes, and many other things. The mass of the sun is causally independent of the refractive index of the earth's atmosphere, and of the workings of our eyes, and so on. Yet, according to positivism, the meaning of our statements about the mass of the sun is tied to the conditions under which we observe it. This is a clearly absurd result. Our statement is about the mass of the sun, not about these other things. We have to take account of these other things in doing our calculations because of interference effects caused by the light from the sun entering the earth's atmosphere, and because of the workings of our eyes, and so on. But

[3] See Paul K. Feyerabend, "The Problem of Theoretical Entities" (1960), trans. from the German by Daniel Sirtes and Eric Oberheim, in: Paul Feyerabend, **Philosophical Papers, Volume 3, Knowledge, Science and Relativism**, Cambridge — New York — Melbourne, Cambridge University Press 1999, pp. 19–20 [16–49] [emphasis in the original].

they have nothing to do with the mass of the sun. Feyerabend draws two conclusions from his argument. First, "*[T]he interpretation of a scientific theory contains metaphysical elements*", that is, elements that are non-empirical. Second, "*The interpretation of scientific theory depends upon nothing but the state of affairs it describes*". [4] Let us make clearer what Feyerabend is saying. Our senses are, for the purposes of science, only measuring instruments to be treated as like other measuring instruments. They are not at all the source of the meaning of scientific claims. Talking in positivist terms, our statements in a scientific theory are not only theory-laden, they are fully theoretical. No part of their meaning comes from experience.

Feyerabend was later to go further than claiming that scientific statements are fully theoretical. He argued that a science without experience is possible. In his 1969 paper, "Science Without Experience", Feyerabend argued that testing a scientific theory could be carried out by a computer which receives data from various devices and produces a yes-no answer to the experimenter. There is no need for the experimenter to use her sensations in testing a theory. However, he did not give convincing examples to bear out this claim. As we will see, others have done so.

Feyerabend presented a much more realistic picture of actual science than the positivists and their followers, who stuck with a supposedly "scientific theory" of meaning, which was based on nothing more than empiricist prejudice. Through much of the empiricist tradition there has been a confusion between empirical tests for hypotheses and empiricist theories about the origin of concepts. Empirical tests for theories, as Feyerabend pointed out, have to do with causal relations between a measuring device and a cause. They do not have to do with meaning relations between a hypothesis and a test.

In recent times, many philosophers have pointed out that what our measuring devices measure go way beyond anything we could experience. Take the case of temperature, we now have ways of measuring temperatures that are far too hot and far too cold for anyone to experience. Hasok Chang has done a detailed study of how these instruments developed. [5] If we were foundationalists, it would be

[4] Paul Feyerabend, "On the Interpretation of Scientific Theories" (1960), in: Paul Feyerabend, **Philosophical Papers, Volume 1, Realism, Rationalism and Scientific Method**, Cambridge, Cambridge University Press 1981, p. 42 [37–43] [emphasis in the original].

mysterious how our measurement of temperature by using our bodies as very rough measuring instruments have been gradually and rationally replaced by much more accurate measuring instruments. After all, our bodies did not evolve to measure temperature as distinct from the heat conductivity of a medium, as we can see when we enter what we think is cold water from what is in fact colder air. How is it possible that we came to distinguish the conductivity of a medium from temperature when our sense of hot or cold has not evolved to distinguish them? How, indeed, did we learn to put aside the effect of our previous sensations on the detection of temperature; for, after all, if we have previously put our hands in "hot" water, water at a normal room temperature will feel cold. If we were positivists, surely the meaning of "hot" and "cold" must be closely tied to experience in a systematic way.

Jerry Fodor

Fodor started out as a critic of the sort of claim Feyerabend sometimes endorsed. In an influential paper, he argued that the processes producing experience are modular and insensitive to beliefs. For instance, while we can believe that the Müller-Lyer illusion is false, and believe that the lines in that illusion are the same length, this has no effect on our experience. The lines continue to look a different length. Thus, experience has a content independent of higher-level beliefs that can be used to test theories. [6] This undermined the claim that experience had no content independently of a high-level theory. However, Fodor soon changed his mind about the significance of his argument.

In the wittily titled paper, "The Dogma that didn't Bark", Fodor argued against the Quinean Dogma that science is primarily about predicting experiences. As he pointed out, if that were the goal of science then the obvious strategy would be to have fewer experiences. So, "if all you want is to be able to predict your experiences, the rational strategy is clear. Don't revise your theories, just *arrange to have fewer experiences*; close your eyes, put your fingers in your ears, and don't

[5] See Hasok Chang, **Inventing Temperature: Measurement and Scientific Progress**, Oxford University Press, New York 2007.

[6] See Jerry Fodor, "Observation Reconsidered", *Philosophy of Science* 1984, Vol. 51, No. 1, pp. 23–43, https://www.jstor.org/stable/187729 [15.09.2023].

move. Now, why didn't Newton think of that?". [7] But scientists behave as if they want to find out further things about the world, not predict experiences. Fodor went on to describe experiments carried out in his laboratory.

The experiments were intended to test the hypothesis that understanding passive sentences takes a longer time than understanding active sentences in someone's native language, ceteris paribus. For instance, one experiment involved hearing active and passive sentences on earphones while attending to a display on a computer screen. The subject has to pronounce aloud any word she sees on the screen. The reaction time for a word paired with an active sentence is compared to the reaction time to the same word paired with a passive sentence. The elapsed time is measured and stored in a computer that carries out the experiment. The times are so short that only a computer could compare them. A material difference of 15 or twenty milliseconds is significant. The computer pools the data and only the differences in reaction times of 50 or 60 experimental subjects would have any importance. The data matrix is enormous. So, it is necessary for the computer to analyse the raw data and produce a result. The raw data would be beyond the capacities of human analysts to analyse. What is important is the statistical p value, which is not something that can be observed in any case. As Fodor points out, the data for a theory are just "*whatever confirms its predictions* and can thus be *practically anything at all* [...] So the data for big bang cosmology include "observations" of cosmic background radiation, the data for Mendelian genetics include the "observed" ratios of traits in the offspring of heterozygotes [...]". [8] Feyerabend could not have put the point better himself. While there is a difference between problems and theories, Feyerabend was really talking about testing theories when he used the Popperian jargon of "the *problem* present".

Fodor continues with some playful remarks on Quine's famous "Two Dogmas of Empiricism" by stating that "[T]he observability of data is thus the third dogma of Empiricism". [9] He then imagines a future science in which you plug an experimenter's cortex into a computer which feeds her the data so that there's no sen-

[7] Jerry Fodor, "The Dogma that Didn't Bark", *Mind* 1991, Vol. 100, No. 2, p. 202 [201–220], https://www.jstor.org/stable/2254867 [15.09.2023][emphasis in the original].

[8] Fodor, "The Dogma...", p. 208 [emphasis in the original].

[9] Fodor, "The Dogma...", p. 208.

sory input at all. This goes even further than Feyerabend in undermining the baleful influence of positivism.

Shapere on Direct Observation

Shapere's first Feyerabendian insight was to go further than Feyerabend by arguing that whether something is directly observed or not is dependent on the source of observation and how the information is received. Further, not only could there could be a science without experience, such a science already exists. He argued that physicists were right to talk of directly observing the centre of the sun in an experiment that started in 1967, which seems to have been unknown to Feyerabend when he wrote "Science without experience".

The experiment involves capturing a neutrino in a drum of cleaning fluid 5000 feet beneath the earth. The neutrino will react with an isotope of Chlorine in the cleaning fluid to produce argon; the argon in turn be removed from the tank by bubbling Helium through it, and then the argon is separated from the Helium by a charcoal trap which registers on a proportional counter, so that the number of neutrino captures are counted. A computer keeps track of the counts. The whole procedure is carried out to capture neutrinos from the centre of the sun in order to directly observe nuclear reactions at the centre of the sun. Neutrinos interact chemically very weakly, and they are not interfered with in travelling from the centre of the sun. The siting of the tank screens out other irrelevant particles.

The experiment was very important, for it showed that the number of neutrinos was considerably less than had been anticipated. This led to further work which resulted in a Nobel prize. [10] Shapere's paper was written long before the Nobel prize was awarded, but he was clearly aware of the importance of the experiment.

Shapere stated the sense in which neutrinos are direct observations of the centre of the sun. He said physicists use "directly observable" to mean the following:

"*x is directly observed (observable) if:*

[10] See https://www.nobelprize.org/prizes/physics/2002/summary/ [15.09.2023].

(1) information is received (can be received) by an appropriate receptor; and

(2) *that information is (can be) transmitted directly, i.e., without interference, to the receptor from the entity x (which is the source of the information)". He continues "specification of what counts as directly observed (observable), and therefore of what counts as an observation, is a function of the current state of physical knowledge, and can change with changes in that knowledge*". [11]

Whereas in his early work Feyerabend had been inclined to dismiss everyday talk as trivial and irrelevant, Shapere argued that there is an important continuity between the use of "directly observed" in everyday language and in the language of Physicists. Everyday talk of observation has two aspects, perceptual and epistemic. In everyday contexts, the two coincide. We gain evidence by perceiving with our sensations. If we perceive something under optimal conditions (for instance, we are not blinded by a bright light), we can speak of directly observing something. However, the Physicist's usage is a rational extension of everyday epistemic usage. Science has shown us, for instance, that we are only sensitive to a small part of the electromagnetic spectrum, let alone our sensitivity to many other ways in which we can acquire evidence about the world. So, we build instruments to capture information about the world. These instruments can detect things we cannot detect with our ordinary sensations. The more we learn, the more we rationally modify our notion of what is directly observable.

An important point in Shapere's argument is that what we know from science indicates that photons interact with all sorts of things before they get to our eyes. This means that detecting photons, in the way in which we do with our eyes, is an unreliable method for directly observing the centre of the sun. By contrast, what we know from science indicates that neutrinos do not interact during their transit to the drums of detergent. This why physicists call getting information from neutrinos a "direct observation" of the centre of the sun.

Shapere goes on to criticise the philosophical tradition of taking generalised doubt to be a serious matter. (Consider, for instance, Descartes' taking seriously the claim that a malignant demon might be causing all of his perceptions). By contrast, he argues that such generalised doubts are not taken seriously in science, for good reasons. Science is highly predictively successful and has transformed

[11] Dudley Shapere, "The Concept of Observation in Science and Philosophy", *Philosophy of Science* 1982, Vol. 49, No. 4, p. 492 [485–525], https://www.jstor.org/stable/187163 [15.09.2023] [emphasis in the original].

our lives. It has done this by only taking seriously specific doubts that are grounded in evidence (such as evidence about the working of particular instruments).

Shapere also points out that, contrary to positivistic lore, scientists do not start from minimum observation statements of experience, such as describing what is seen on a photographic plate as a "speck" rather than an image of a star. He objects that even describing something on a photographic plate as a speck requires prior knowledge. In any case, reference to sense-data is too impoverished to function by itself as a basis for knowledge. To be of any use in knowledge gathering, the so-called speck must be thought to be something on the basis of a rich background knowledge based on previous scientifically reliable information. In science, we describe what we see according to the strongest vocabulary justified by previous work in the area and by background knowledge. This is a practice in turn justified through the success of science. [12]

Shapere's discussion of direct observation in a complex scientific experiment which relies on a great deal of background knowledge of causal interactions furthers Feyerabend's account of scientific observation and testing of theories. It also hints at something I will discuss in detail later, which is that knowledge gathering is a research immanent practice in which previous success fundamentally alters the knowledge acquiring enterprise itself.

Feyerabend on Logic

In two early papers, "An Attempt at a Realistic Interpretation of Experience" and "The Problem of Theoretical Entities", Feyerabend pointed out that there are situations of which the phenomenologically adequate description is inconsistent, even though some philosophers have claimed that this is impossible. Feyerabend relied principally on an important paper by Tranekjaer-Rasmussen. [13] As Feyerabend put it,

[12] See Dudley Shapere, "The Concept of Observation in Science and Philosophy (summary version)", in: Dudley Shapere (ed.), **Reason and the Search for Knowledge**, Dordrecht, Reidel 1984, pp. 349–350 [342–351].

[13] See Edgar Tranekjaer–Rasmussen, "On Perspectoid Distances", *Acta Psychologica* 1955, Vol. 11, pp. 297–302.

> "there are statements that are subjectively completely certain in a particular observational situation [...] and which contain a contradiction [...] Subjects were asked to compare the lengths of three lines: a, b, and c. The result of direct observation (whose absurdity, for the most part, only subsequently appears to the subjects, who are occupied with the correct description of what is observed) is that a=b; b=c; but a>c". [14]

Feyerabend discussed an objection by Ayer, in which Ayer argued that it only seems that a=b etc., which is not inconsistent. Feyerabend pointed out that:

> "[T]his solution does not work. What I observe is not that a seems equal to b. The impression is not indefinite and uncertain. I observe that a=b. The element »seems« does not appear *in* the perception, but only serves to hint that the following report concerns a perception and not a physical object. Thus, the situation can be grasped with a single glance, so »seems« belongs to the beginning of the description and is equivalent to »I perceive that«, and that is what we have claimed — the existence of a direct description of a perception which contains a contradiction". [15]

Ayer's resistance, and the resistance of many others, to describing phenomenology as inconsistent partly results from an attachment to the view that the contents of our experience are a foundation for our other knowledge. However, in this context, a more important source of resistance is an attachment to standard modern logic, which is based on the formal systems developed by Frege and Russell. A central assumption of various positivist thinkers is that there is one true logic and that that logic is Frege/Russell logic or one its variants. Built in to those logical systems is ex contradictione quodlibet, the principle that a contradiction implies every other proposition. Accepting this principle means that individual contradictions have no structure of their own — they are not different from one another because they are logically equivalent. Thus, their individual features cannot be described adequately. Yet, as is obvious from Feyerabend's discussion, particular contradictions have highly specific features. The fact that they have highly specific features has led many logicians, particularly in Australia, to abandon ex contradictione quodlibet. Abandoning this principle leads to the development of paraconsistent logics, which are logics that allow for the possibility of true contradictions. [16]

---

[14] Feyerabend, "The Problem...", p. 33.

[15] Feyerabend, "The Problem...", p. 34 [emphasis in the original].

[16] For a detailed and interesting discussion of the advantages of paraconsistent logical systems which abandon ex contradictione quodlibet, see Richard Routley, Robert Meyer, Valerie Plumwood, and Ross Brady, **Relevant Logics and Their Rivals 1**, Ridgeview Publishing, Atrascadero 1982. For

Chris Mortensen on Inconsistent Geometry

Chris Mortensen has devoted himself over some years to systematically describing the details of the inconsistent geometry of inconsistent pictures. He has developed a paraconsistent logic and topology, a variant of group theory, matrices, and other mathematical tools to describe the inconsistent geometry involved in various inconsistent pictures. He has classified at least four kinds of inconsistent geometry. Unfortunately, the details of his analysis are too intricate and formal for a paper of this kind. So, I can only note here that Feyerabend's initial brief and suggestive remarks on an inconsistent geometry were prescient, and that recent work by Mortensen has described in detail various kinds of inconsistent figures in a rich and complex formal theory that does not reduce all the various kinds of inconsistencies to equivalents to one another, as one would expect if Frege/Russell logic and its variants were the one true logic. Inconsistent figures Mortensen analyses in detail include the Schuster fork, Escher's inconsistent Necker Cube, and the Penrose triangle. [17] By carrying out this project, Mortensen has significantly extended an insight found in Feyerabend. He has shown through the analysis of a range of concrete examples that only a paraconsistent logic and mathematics will allow us to describe accurately the phenomenology of inconsistent pictures. In this way, he has shown in detail that the standard logic students are taught, as if it is the one true logic, is not an adequate foundation for the study of phenomenology.

Escher's inconsistent pictures are well known. Mortensen has produced a significant work on Escher's predecessor who studied and produced a range of impossible pictures, Oscar Reutersvärd, and discussed his relationship with Escher and others. A range of pictures await detailed formal analysis. [18]

I note here, however, that Mortensen confines himself to phenomenology. He distinguishes between weak paraconsistency, which holds that inconsistent figures can be coherently described but do not exist in the external world, but only

---

a more technical account, see Alan Anderson, Nuel Belnap, and Michael Dunn (eds.), **Entailment 1**, Princeton University Press, Princeton 1976.

[17] See Chris Mortensen, **Inconsistent Geometry,** *Studies in Logic*, Volume 27, College Publications, London 2010.

[18] See Chris Mortensen, **The Impossible Arises, Oscar Reutesvärd and his Contemporaries**, Indiana University Press, Bloomington 2022.

in phenomenology, and strong paraconsistency, which holds that contradictions exist in the external world.

Feyerabend briefly claimed that logical laws might have to be abandoned in the light of research in his later work, and he showed a clear awareness of alternative logical systems. However, he did not explicitly discuss paraconsistency or paraconsistentist logical systems. [19]

Feyerabend on Method

In early papers such as "Explanation, Reduction, and Empiricism", and in the various editions of **Against Method**, Feyerabend argued against prevailing views that were not only prominent in positivist thinking but even in various radical critics of positivism such as Popper and Lakatos. A key part of his line of argument was the methods for acquiring significant knowledge changed over time. Another part of his line of argument was to criticise the cumulativist assumptions of many historians and philosophers of science. He argued that science has not accumulated knowledge over time, because later theories are sometimes radically conceptually different from their predecessors. This was turned into a defence of relativism in the later part of **Against Method**. There have been many criticisms of many of the central claims of **Against Method**. For instance, Feyerabend's claims about Galileo, his central case study, are largely false or misleading, and he did not correct problematic claims in later editions. [20] Nevertheless, there was an important point on which Feyerabend was correct. There is no universal scientific method in the form of precise formal rules that has been used by successful scientists or the scientific community.

An important weakness in Feyerabend's critique of a universal method is that he never gave a detailed argument that that critique implied relativism. Instead, he assumed that the lack of a universal method in some way implied relativism, and proceeded to develop a relativist account. In this way, Feyerabend ac-

[19] See Paul Feyerabend, **Against Method,** Third Edition, Verso Books, London 1993, pp. 195–197.

[20] See, for instance, Alan Chalmers, **Science and its Fabrication**, Open University Press, Milton Keynes 1990. I have summarised some of the major criticisms in George Couvalis, "Feyerabend, Critique of Rationality in Science", in: Byron Kaldis (ed.), **Encyclopedia of Philosophy and the Social Sciences**, Volume 1, Sage Publications, London 2013, pp. 356–359.

cepted a crucial assumption of the foundationalism that he had done so much to criticise, namely that if there is not a universal method that is given before we even begin research, we are condemned to relativism.

An Important Challenge: Shapere on Method

Dudley Shapere agreed with criticisms of universal method and cumulativism, and expanded on them. He also agreed with the view that the conceptual scheme of science has changed radically over time. However, he did not draw the relativist conclusions that Feyerabend drew from these facts in **Against Method**. Instead, he argued that although science had changed, it had changed in rational steps without benefit of a universal method. Unfortunately, Feyerabend never confronted Shapere's line of argument in any detail despite his preference for a pluralist epistemology. Instead, his criticisms focussed on Popper's and Lakatos' attempts to come up with a universal method.

In an important paper, Shapere summed up the foundationalist view of the origin of our knowledge stating it as the view that "*there is something which is presupposed by the knowledge-acquiring enterprise, but which is itself immune from revision or rejection in the light of any new knowledge or beliefs acquired*". [21] He distinguished four variations on this theme. First, that there are ontological claims which must be accepted before inquiry is possible. Second, that there is a universal method not subject to alteration. Third, that there are rules of reasoning which can never be changed. Fourth, that there are concepts employed in or talking about science which cannot be altered in the light of new knowledge. Feyerabend had argued in detail against these claims. For instance, **Against Method** was a critique of the second claim; some of his early papers were critiques of the fourth claim.

Shapere mentioned the line of argument in favour of the foundationalist view that there cannot be good reason for change unless there are universal standards. The proponents of foundationalism argue that there are only two alternatives, relativism or accepting the timeless universal standards. Note that, as we have seen,

[21] Dudley Shapere, "The Character of Scientific Change" (1983), in: Dudley Shapere, **Reason and the Search for Knowledge**, Reidel, Dordrecht 1984, pp. 205–260 [emphasis in the original].

Feyerabend gradually fell into relativism after radically criticising the foundationalist view. Contrary to Feyerabend, Shapere argued that both that the dichotomy is a false one, and that the four variations of the traditional view are all incorrect. Even though the given criteria at any stage for an explanation do mark out a range of possible explanations, the knowledge attained can lead to a change in the criteria — a "rational feedback" mechanism is involved because what is discovered by science changes the criteria themselves, as indeed it changes much else.

Shapere's view might be described as a research immanent rationalist view of the development of science rather than a research transcendent rationalist view. On his account, radical differences between the beliefs, methods, and concepts used by researchers at two different epochs do not lead to relativism or irrationalism if we can show that at every point, a change is rational on criteria rationally accepted at the time. Shapere's arguments for his view have never been adequately addressed by Feyerabendians. I will only briefly discuss some of his line of argument to give an idea of the detail and subtlety of Shapere's approach.

We have already seen how Shapere argued that what is directly observable changed rationally in the course of what was discovered in science. Already, as I have pointed out, measurements of temperature transformed science by allowing various important distinctions to be made. To the ancient Greeks, like Aristotle, heat was a qualitative property. The very idea that various aspects of it could be measured, and measured precisely, does not seem to have occurred to Aristotle or his followers. This changed with Galileo, who constructed an early thermometer and showed that amounts of heat were measurable in many situations, at least in a rough and ready manner. Using widely available common-sense criteria of the time, this changed the view of temperature radically.

Feyerabend often talked as if various aspects of the Aristotelian view made it a kind of closed system which could not be criticised from the outside. However, the Aristotelian view was enmeshed in much of the common-sense of the time, which contained much else apart from that view. Various moves were available to undermine the plausibility of the Aristotelian view, as we can see in the case of temperature. Alan Chalmers and Stillman Drake have shown in some detail how common-sense arguments were used to undermine criticisms of the use of the

telescope. [22] For the Aristotelians to try to make Aristotelianism a closed system was merely a manoeuvre to save them from embarrassment. Feyerabend exaggerated for rhetorical purposes and created a kind of world view that was closed off from outside influences — something which did not exist at the time, or, indeed, at any time. In the real world, cosmological hypotheses are enmeshed in a variety of real-world practices that can be used to undermine them. Closed relativist world-views are a construction of the anthropologically minded rather than a reflection of actual societies. Actual societies are much more complex and engage in real world practices which can be used to contradict hypotheses. In Italy in Galileo's time there were many useful practices involving careful measurement and the use and design of instruments.

Various surprises occurred as a result of research on temperature towards the end of the eighteenth century. As a result of the surprising data provided by the use of early thermometers, it became clear that there is not one thing "heat". It was realised that putting in the same amount of a hot substance would raise the temperature of others substances to quite different levels. It was also realised that putting in heat into ice would not raise its temperature for a long time until it relatively suddenly turned into water. As a result of attempts to make sense of various measurements of temperature, Joseph Black and others distinguished specific heat, and various kinds of latent heat from temperature. [23] Heat conductivity was also later distinguished from temperature and measured. The ontology of heat was radically changed over time. This was all partly a consequence of the widespread design and use of ever better steam engines. It is not an accident that one of the principal theorists of the new science of heat was Joseph Black, who was assisted by the now well-known James Watt. However, the view that there is a fluid heat substance, caloric was prevalent for some time, for good reasons. Nowadays, it is recognised that the ontology of temperature is quite different from the onto-

[22] See Chalmers, **The Scientist's Atom and the Philosopher's Stone…** An amusing example of Galileo's use of common-sense criteria is that Galileo responded to the claim that the moons of Jupiter might be artifacts with the ironic remark that he would "pay 10, 000 scudi to anyone who made a telescope that would create satellites around one planet and not around others", Stillman Drake, **Galileo at Work**, University of Chicago Press, Chicago 1978, p. 166. In a society with an important and influential craft and instrument making tradition like the Northern Italy of Galileo's time, remarks like this had a significant impact.

[23] See Duane Roller, **Case 3: The Early Development of Temperature and Heat**, Harvard University Press, Cambridge 1950.

logy of specific heat — specific heat is a quantum phenomenon, whereas temperature is caused by the vibration of molecules. Still, at each point, changes in the ontology of heat were arrived at rationally, as careful studies of the work of Joseph Black, Lavoisier, Perrin, and others show. [24]

A similar result has occurred recently with research on pain. The idea that there is one simple thing, "pain" has been overthrown on the basis of research on reports from patients, and on the basis of anatomical research carried out according to current standards. Chronic pain is distinct from sharp pain, and the affective aspect of pain has been distinguished from the somatosensory features of pain. It is even possible for subjects to experience pain and describe it clearly but report that it does not bother them. The simple everyday concept of pain has been replaced in medical research by more sophisticated categories as a result of careful research. [25] It has been pointed out that Plato already had a rudimentary account of what has been discovered in modern research, which he arrived at through his reflections on cognitive aspects of pain. [26]

The above two case studies are useful illustrations of Shapere's point that ontology can change radically in a rational manner. There is no need to invoke relativism in explaining any change in science.

Let me now turn to logic. Shapere rightly points out, as one of his examples from the history of science, that the early versions of the calculus were inconsistent. The inconsistent version of calculus was at the very heart of early modern science because it was being used systematically in calculations. Nevertheless, scientists worked out how to use them to make precise predictions that were confirmed while avoiding the problems raised by inconsistency. So, this shows that science is not bound by consistency when abandoning consistency is fruitful. Scientists did not follow Berkeley in rejecting the use of the calculus because it was inconsistent. Of course, for mathematicians the inconsistency needed to be re-

[24] See Robert Fox, **The Caloric Theory of Gases**, Clarendon Press, Oxford 1971; Alan Chalmers, **The Scientist's Atom and the Philosopher's Stone: How Science Succeeded and Philosophy Failed to Gain Knowledge of Atoms**, *Boston Studies in the Philosophy of Science*, Vol. 279, Springer, Dordrecht 2009.

[25] See Valerie Gray Hardcastle, **The Myth of Pain**, MIT Press, Cambridge 2001; Nicola Grahek, **Feeling Pain and Being in Pain**, MIT Press, Cambridge 2011.

[26] See George Couvalis and Mathew Usher, "Plato on False Pains and Modern Cognitive Science", *Philosophical Inquiry* 2003, Vol. 25, No. 3, pp. 99–115.

solved if it could be, as indeed it was in the nineteenth and twentieth centuries. But as Shapere points out, consistency is not a fundamental requirement of the scientific enterprise. [27]

Brown and Priest have spelled out the strategy used by physicists in the seventeenth and eighteenth centuries to deal with the inconsistency of the calculus, and suggested other ways in which the strategy can be used fruitfully when confronted with an inconsistency. [28] We have already seen in a discussion of Mortensen's work that to adequately describe the phenomenology of inconsistent images or perceptions, we need to use a paraconsistentist logic and mathematics. Unfortunately, Feyerabend does not seem to have followed up his early insights into violations of the laws of Frege/Russell logic in his later work in any detail. He seems to have been unaware of significant work by paraconsistentist logicians such as Routley.

Much else can be said about Shapere's arguments. As I have emphasised, Feyerabend never engaged in a debate with those arguments. They pose a fundamental challenge to Feyerabend's move from a rejection of a universal method to relativism.

Epilogue

We have seen that Feyerabend's work, particularly his early work, contains important insights into the nature of science and logic that were developed in detail by later researchers. However, he failed to discuss a serious challenge to his move to relativism developed by Dudley Shapere. How far can we go with a research immanent account of rationality without falling into relativism? I do not know. Shapere's neglected work constitutes a well-worked out alternative to relativism. It is time Feyerabendians took it seriously.

George Couvalis

[27] See Shapere, "The Character …", p. 235ff.

[28] See Bryson Brown and Graham Priest, "Chunk and Permeate, a Paraconsistentist Inference Strategy, Part 1: The Infinitesimal Calculus", *Journal of Philosophical Logic* 2004, Vol. 33, No. 4, pp. 379–388, https://doi.org/10.1023/B:LOGI.0000036831.48866.12.

References

1. Anderson Alan, Belnap Nuel, and Dunn Michael (eds.), **Entailment 1**, Princeton University Press, Princeton 1976.
2. Brown Bryson and Priest Graham, "Chunk and Permeate, a Paraconsistentist Inference Strategy, Part 1: The Infinitesimal Calculus"*, Journal of Philosophical Logic* 2004, Vol. 33, No. 4, pp. 379–388, https://doi.org/10.1023/B:LOGI.0000036831.48866.12.
3. Chalmers Alan, **The Scientist's Atom and the Philosopher's Stone: How Science Succeeded and Philosophy Failed to Gain Knowledge of Atoms***, Boston Studies in the Philosophy of Science*, Vol. 279, Springer, Dordrecht 2009.
4. Chang Hasok, **Inventing Temperature: Measurement and Scientific Progress**, Oxford University Press, New York 2007.
5. Couvalis George, "Feyerabend, Critique of Rationality in Science", in: Byron Kaldis (ed.), **Encyclopedia of Philosophy and the Social Sciences**, Volume 1, Sage Publications, London 2013, pp. 356–359.
6. Couvalis George and Usher Mathew, "Plato on False Pains and Modern Cognitive Science", *Philosophical Inquiry* 2003, Vol. 25, No. 3, pp. 99–115.
7. Chalmers Alan, **Science and its Fabrication**, Open University Press, Milton Keynes 1990.
8. Couvalis George, **Feyerabend's Critique of Foundationalism**, Avebury, Aldershot 1989.
9. Couvalis George, **The Philosophy of Science**, Sage, London 1997.
10. Drake Stillman, **Galileo at Work**, University of Chicago Press, Chicago 1978.
11. Feyerabend Paul, **Against Method,** Third Edition, Verso Books, London 1993.
12. Feyerabend Paul, "On the Interpretation of Scientific Theories" (1960), in: Paul Feyerabend, **Philosophical Papers, Volume 1, Realism, Rationalism and Scientific Method**, Cambridge, Cambridge University Press 1981, p. 37–43.
13. Feyerabend Paul, "The Problem of Theoretical Entities" (1960), trans. from the German Daniel Sirtes and Eric Oberheim, trans. from the German by Daniel Sirtes and Eric Oberheim, in: Paul Feyerabend, **Philosophical Papers, Volume 3, Knowledge, Science and Relativism**, Cambridge — New York — Melbourne, Cambridge University Press 1999, pp. 16–49.
14. Fodor Jerry, "Observation Reconsidered", *Philosophy of Science* 1984, Vol. 51, No. 1, pp. 23–43, https://www.jstor.org/stable/187729 [15.09.2023].

15. Fodor Jerry, "The Dogma that Didn't Bark", *Mind* 1991, Vol. 100, No. 2, p. 202 [201–220], https://www.jstor.org/stable/2254867 [15.09.2023].
16. Fox Robert, **The Caloric Theory of Gases**, Clarendon Press, Oxford 1971.
17. Grahek Nicola, **Feeling Pain and Being in Pain**, MIT Press, Cambridge 2011.
18. Hardcastle Gray, **The Myth of Pain**, MIT Press, Cambridge 2001.
19. Mortensen Chris, **Inconsistent Geometry,** *Studies in Logic*, Volume 27, College Publications, London 2010.
20. Mortensen Chris, **The Impossible Arises, Oscar Reutesvärd and his Contemporaries**, Indiana University Press, Bloomington 2022.
21. Roller Duane, **Case 3: The Early Development of Temperature and Heat**, Harvard University Press, Cambridge 1950.
22. Shapere Dudley, "The Concept of Observation in Science and Philosophy", *Philosophy of Science* 1982, Vol. 49, No. 4, p. 485–525, https://www.jstor.org/stable/187163 [15.09.2023].
23. Routley Richard, Meyer Robert, Plumwood Valerie, and Brady Ross, **Relevant Logics and Their Rivals 1**, Ridgeview Publishing, Atrascadero 1982.
24. Shapere Dudley, "The Character of Scientific Change" (1983), in: Dudley Shapere, **Reason and the Search for Knowledge**, Reidel, Dordrecht 1984, pp. 205–260.
25. Shapere Dudley, "The Concept of Observation in Science and Philosophy (summary version)", in: Dudley Shapere (ed.), **Reason and the Search for Knowledge**, Dordrecht, Reidel 1984, pp. 342–351.
26. Tranekjaer–Rasmussen Edgar, "On Perspectoid Distances", *Acta Psychologica* 1955, Vol. 11, pp. 297–302.
27. Quine Willard, **From a Logical Point of View**, Norton, New York 1961.
28. https://www.nobelprize.org/prizes/physics/2002/summary/ [15.09.2023].

Mature Philosophy

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 187–201

https://doi.org/10.53763/fag.2023.20.2.206

ARTYKUŁ ORYGINALNY / ORIGINAL ARTICLE

Sergio Benvenuto

Institute of Sciences and Technology of Cognition of the National Council of Research of Rome

Paul Feyerabend's Contribution: The Anarchic Sunset of the Philosophy of Science *

Received: November12, 2023. Accepted: December 4, 2023. Published online: March 8, 2024.

Abstract: The author places Feyerabend's contribution within a line of thought on science that ideally opens with the Wienerkreis, and which in some way ends with Feyerabend. This trend has always been based on a series of demarcations, ranging from the opposition between meaningful and meaningless statements (the Vienna Circle), or between scientific and non-scientific statements (Popper), or between normal and extraordinary science (Kuhn), to the demarcation between progressive and regressive research programs (Lakatos) — up until Feyerabend, the former student of Popper, who puts an end to the perspicuity of every demarcation. The conclusion that "anything goes" when it comes to doing rigorous science marks the clearly unsuccessful conclusion of this large-scale historical trend. Philosophical attention thus shifts from descriptions of the scientific method to an analysis of the concrete historical production of scientific ideas and discoveries, in a movement that rehabilitates a Hegelian, historical approach in the empirical sciences.

The author outlines the contemporary approach that no longer sees the whole of knowledge and scientific practice as a series of methodologies aimed at faithfully mirroring nature, construing it instead as a thought-constituted organism subject to Darwinian criteria of selection and mutation. The biological metaphor of the organism that survives by adapting to external reality replaces the

Keywords:

darwinian epistemology;
Karl Popper;
Paul K. Feyerabend;
philosophical pluralism;
science as art;
scientific method;
Thomas S. Kuhn;
Vienna Circle;

* I would like to thank Stefano Gattei for his comments on this article.

claim to foundational scientific validity on the basis of a priori paradigms.

1. Introduction

Most scientists do not think highly of Feyerabend. An elite group of somewhat eccentric scientists, on the other hand, holds him in high regard. Among them is the palaeontologist S.J. Gould: he confided to Feyerabend that **Against Method** had inspired him to construct, together with Niles Eldredge, his innovative theory of 'punctuated equilibrium' in biological evolution. Feyerabend often quoted this acknowledgement proudly. [1]

The rejection of Feyerabend's thinking is due to the fact that so many scientists today have been trained to think that all that matters in science is its method. Instead, what Feyerabend liked about science, even though he was a pupil of Popper, was its discoveries. When he held a seminar at the University of Trento in 1992, which I attended, a tenured professor of theoretical physics was also present. In an informal discussion among the participants, at one point the latter said that ultimately physics amounts to very little, that its only merit is that it has a certain method... An embarrassed silence fell over the room.

The importance of Feyerabend does not lie in having opened up a new horizon in the philosophy of science, but rather in having brought to a close, perhaps definitively, the most flourishing current of modern epistemological thought — namely, the Austro-Anglo-American line that, starting with Mach and passing through the Vienna Circle, Popper, Quine, Kuhn and Lakatos, spanned an entire century. The biography of Feyerabend — an Austrian transplanted to England and then California — almost epitomizes the spatiotemporal movement of this current. He brought the latter to an end, much as Ockham nominalistically did with regard to the scholastic era, or as Hume did through his scepticism where the period of British empiricism is concerned. This glorious line of thought has always been beleaguered by the need for *demarcation*. With Feyerabend, this need is relinquished: anything goes.

[1] See Niles Eldredge and Stephen J. Gould, "Punctuated Equilibria: an Alternative to Phyletic Gradualism", in: Thomas J. M. Schopf (ed.), **Models in Paleobiology**, Freeman, San Francisco 1972, pp. 82–115; Paul K. Feyerabend, **Against Method: Outline of an Anarchistic Theory of Knowledge**, preface by Ian Hacking, 4th edition, Verso, London 2010.

2. The Demarcation Problem

The demarcation that interested the Vienna Circle was that between *signifying* and *non-signifying utterances*. For the logical positivists, an utterance signifies only and exclusively if it is verifiable: hence, the signified of a proposition is the method of its empirical verification. From this perspective, all metaphysical propositions are not false but non-signifying. All our knowledge is inductive, and as for logico-mathematical propositions, they are necessarily true insofar as they are tautologies along the lines of "a = a". The trouble, however, is that not only is the bathwater of metaphysics thrown out into this sea of non-signification, but also all those beautiful babies such as ethical, aesthetic and expressive statements...

For Karl Popper, on the other hand, the demarcation that matters is not between signifying and non-signifying utterances, but between *scientific* and *non-scientific theoretical utterances*. That is, everything that is metaphysics, aesthetics, ethics, philosophy, etc., is signifying but unscientific. Indeed, metaphysics constitutes the "breeding ground" for science: certain theories arise as metaphysical, i.e. non-falsifiable and therefore non-scientific, and become scientific with time (the most famous case being atomism, from Democritus to Planck). For Popper, a proposition is scientific insofar as it can be falsified with precision; in short, science moves forward by trial and error. The propositions of psychoanalysis — a doctrine that Popper targets in particular — are very significant, but they are not scientific because it is impossible to refute them. That is to say, the "empirical content" of psychoanalysis is very poor because it has very few basic assertions that can potentially be refuted. In short, scientific knowledge is not built by induction, as the neo-positivists think, but by *selection* thanks to falsifying experimentation. This means that a scientific proposition — what Popper calls a conjecture — can never be definitively verified, it can only be *corroborated*. The most established scientific theories are those that have been most corroborated — that is, that have withstood the most ingenious attempts to falsify them *so far*. This is now in many countries the official philosophical vulgate on science, the one that is taught in high schools.

While the question in relation to Popper is that of discriminating between theories — the scientific and the non-scientific — the unproblematic continuity

between experience and theory is broken, and what is asserted is that drawing on experience is of value as part of a *debate between theories*. Experience ceases to be the origin and guarantee of scientific theories; it always comes *after* theory and performs a filtering function in a divergence. For Popper, "scientific research begins and ends with problems".

3. Historical Approach to Knowledge

Hegel (by which we mean, in effect, an essentially historical approach to thought), thrown out of the door of rationality by both neo-positivism and Popper's critical rationalism, re-enters through the back door mainly thanks to Thomas S. Kuhn. Significantly, the latter was a follower of Alexandre Koyré, a Hegel scholar. After Kuhn, philosophers of science would increasingly abandon *a priori* arguments and refer more and more to the concrete history of the European sciences. So, falsificationism has been historically falsified. And the demarcation that therefore becomes important is the one between *normal science* and *extraordinary science*.

Kuhn notes that the critical spirit, which Popper considered essential in the "game" of science, is not in fact indispensable to the sciences, sometimes it is also dangerous. This is because even the most powerful and established theories are widely refuted by a myriad of embarrassing facts and observations. If falsificationism were adhered to seriously in scientific work, no theory, especially at its beginnings, would be accepted, because each would in fact be falsified by a more or less extensive number of facts. In the stages that Kuhn calls normal science — where this typifies the vast majority of the work of scientists — scientific work consists of trying to solve scientific puzzles: that is, attempting to reconcile seemingly deviant facts with the accepted theory, which in turn falls into a given scientific paradigm. Here we have what really is the most successful invention of modern philosophy of science: the notion of *paradigm*, which we now all, even non philosophers, use. The history of science becomes a discontinuous process of leaps from one paradigm to another, and the reassuring image of knowledge progressing continuously through a patient accumulation of knowledge is broken. *Scientia facit saltus*.

Every new theory, by virtue of being new and not having had time to normalize through extended debate, emerges in a sea of phenomena that refute it. Thus, the ant-like labour of "normal scientists" — those who uncritically accept a given scientific paradigm — reinforces that paradigm. And it is reinforced because the "normal scientists" develop a series of buttressing sub-theories, discovering or emphasising new facts to confirm the paradigm. The work of solving puzzles — i.e. irregularities that, if they persist, can refute the theory — strengthens a given scientific paradigm because its proponents *do not surrender* to the refutation that certain facts inflict on it. As Feyerabend would later say, the proponent of a theory, especially in its infancy, needs *to be tenacious,* and tenacity is the opposite of a critical spirit. Apart from those periods that Kuhn characterizes as being of "extraordinary science", of explicit conflict between scientific paradigms as such — and of questioning what is scientific and what is not among scientists themselves — most researchers accept that they will be operating within the dominant paradigm in their field. The criterion is that *it's always better to have a false theory than no theory at all*. This is the conservative face of science, but the one that makes the advancement of knowledge possible.

4. Science and Non-Science

We will not be able to delve here into the decisive contributions made by Duhem, Poincaré, Michael Polanyi, Quine and Lakatos. Instead, we will limit ourselves to summarizing the image of scientific activity that emerges from the epistemological debate in the twentieth century.

Breaking more and more with the *representational* idea of knowledge as a system of *Bilden*, of ever more faithful *images* of the world (the "mirror of nature" of which Rorty speaks), [2] an image of scientific theories as *signifying organisms* that behave in a manner analogous to animal species is establishing itself. The speculative and contemplative metaphor has been replaced by the biological metaphor — something that has also happened in other fields of culture (take biopolitics, for example). This success on the part of the biological reference — knowing is only

[2] See Richard Rorty, **Philosophy and the Mirror of Nature,** Princeton University Press, Princeton 1979.

a part of living — would produce, among other things, Dawkins's *meme* theory, an approach to cultural processes similar to a biological approach. [3] *Meme* (from *mimesis*), the spiritual equivalent of gene, is now another commonly used term. Theories and paradigms, like genotypes, evolve by mutation and selection. Mutations do not come from some new experience but from the emergence of a new idea, which in a sense falls from the sky, and first of all exerts a seductive attraction on scientific minds. New ideas, Deleuze said, are the inaugural *party of* scientific research. But theories and paradigms become established if they have the ability to overcome the various challenging trials set by the environment, which in science are the empirical data that every theory needs to take into account. Theories, just like living organisms, are therefore conservative, where this does not prevent them from being supplanted by other theories that reproduce themselves more prolifically – i.e. ones that reproduce themselves more prolifically in the minds of scientists, which in turn constitute the environment of ideas.

Max Planck said that a new theory seldom prevails by convincing scientists through arguments alone, as scientists can invariably discover counterexamples: older scientists simply die or retire and younger ones, trained in the new theory, take their place. [4] There is a demography of scientific truth. Now, all this goes hand in hand with the most influential philosophy of the twentieth century, pragmatism, which more or less descends from Vico's *verum factum est.* Science is never disembodied, it forms a body with the very human processes of rhetorical persuasion, propagation, reproduction and hegemony. In short, to know the world is to dominate it and survive in it. From an image of theory as representation of the world, we move to an instrumentalist, action-centred image of theory.

Therefore, the inverse can also be said: that life is itself a form of progressive self-knowledge of the world.

All the theories shared by the scientific community today are falsified, they are all imperfect, so it is not so much a question of choosing between true and false theories, but between theories that are more or less imperfect. After all, even animal species are all of them imperfect, [5] and yet certain variants still predominate over others as less maladaptive.

---

[3] See Richard Dawkins, **The Selfish Gene**, Oxford University Press, Oxford 1976.

[4] See Max K. Planck, **Scientific Autobiography and Other Papers**, Philosophical Library, New York 1950.

Let's also add that for any theory-paradigm to assert itself, it needs two qualities that don't always align: *seductive power* and *explanatory power*. These two correspond to the dual fitness of biological organisms: on the one hand the ability to seduce the opposite sex more effectively (where reproduction is sexual), and on the other to evade predators and capture prey. For example, biologists have been unable to find an explanation for the antlers of male deer: the larger and more intricate they are, the more they seduce females, even if they are a handicap for the animal. Antlers, apart from this erotic potency, have no other adaptive sense. Species appear to be diverted from their purely reproductive function and entrenched in a purely erotic logic.

Even the strongest theories — such as relativity or quantum mechanics — have established themselves thanks to their intellectual seductiveness. The great scientific theories are *beautiful*. And they need to be seductive — this is Feyerabend's famous analysis of Galileo's "propaganda" for his theories. Scientists confess that certain hypotheses, which may well be plausible, are immediately discarded because they say to themselves: "God can't be so vulgar!" Nature has to have an elegance of its own that theorization needs to capture. Obviously, if the intellectual seductiveness of a theory overwhelmingly prevails over its explanatory power, then the theory loses its scientific persuasiveness. This is what is said today about Marxism and psychoanalysis, for example — theories that are intellectually highly seductive ("brilliant", even), but not very explanatory and hence unscientific. In cosmology, string theory, which describes the universe as a kind of musical harmony, held great fascination for decades, until most realised that its explanatory power was very low. Conversely, a theory that is only explanatory but lacks intellectual or aesthetic appeal will tend to be ignored. For example, Galileo's reluctance to accept Kepler's orbital ellipses can only be explained by the fact that ellipses appeared less "beautiful" to him than circles. [6] Today, certain sociobiological theories about genetic differences among humans are not taken into account, because they often lead to racist conclusions, and racism is an ugly thing.

There is therefore no authentic clear-cut boundary between scientific and non-scientific theories. It is a matter of degree: the more seductive and the less ex-

[5] For a view that contrasts with the conventional image of life forms as always perfectly adapted to the environment, see Telmo Pievani, **Imperfection: A Natural History**, The MIT Press, Boston 2022.

[6] See Erwin Panofsky, **Galileo as a Critic of the Arts**, Springer, New York 1954.

planatory a theory is, the farther removed it is from scientific respectability. And yet, in return, it can acquire considerable philosophical respectability, for instance. This is the case, for example, with Freud, who is no longer a subject of study in Psychology or Psychiatry courses, but is studied in Philosophy and Comparative Literature.

5. Permanent Revolution

If the survival of a theory-paradigm, its preservation, is an integral part of the process of knowledge, then everything that appears to be the mere garbage of knowledge — the academic structure of scientific communities, the funding strategies for research projects, the political needs of governments, the environmental pressure from the circulating ideologies, the stubborn tenacity we mentioned earlier — all becomes an integral part of the formation of knowledge. And that is exactly what Feyerabend wanted to tell us. He does not say, as many believe, that science is political, but that scientific policies exist that more or less collude or collide with the social and political environment in which science develops. Hence his political agenda of separating state and science, just as church and state have been separated — a separation, however, that I think has already taken place, given that so much research funding today comes from private enterprises.

Feyerabend rejects Kuhn's concept of normal science because he has a quite aristocratic idea of scientific work. Science progresses not by perfecting a paradigm, but by leaping over and breaking the assumptions of dominant paradigms. I find it strange that Feyerabend has been described as anti-science: on the contrary, he gives us a heroic image of science, which he believes "progresses" just like the arts.[7] What interests him is scientific creativity, not the routine work to which scientific research is often reduced today. Science is now a mass profession, involving millions of workers. We are a long way from the great scientific revolutions of the last century, which were the work of a small elite of brilliant devotees.

In short, Feyerabend does believe in scientific progress, but observes that it advances by inventing new methods time after time. Methods are like the tools

[7] See Paul K. Feyerabend, **Wissenschaft als Kunst**, Suhrkamp, Berlin 1984.

a sculptor uses to produce a statue — but what matters, in the end, is the statue. The fact that Newton assumed a mysterious long-distance attractive force between the sun and the planets did not stop the Newtonians from prevailing over Cartesian physics, which apparently explained things more effectively. The fact that many phenomena in quantum physics assume that knowledge of a phenomenon modifies or determines it (as in the paradox of Schrödinger's cat) goes as far as breaking the principle of realism in science. The fact that Einstein did not fail to point out this enormous infringement has not prevented quantum theory from prevailing as the fundamental physics of our time. The important thing about a theory is, then, its ability to *predict*, not so much its ability to adequately explain — even though, in science, we always find a tension between predictive power and explanatory intelligibility. It so happens that a theory such as Darwinism has no predictive power (no one knows what new organisms will turn up) but is very powerful as a model that makes the history of life intelligible. By contrast, quantum theory, as we have said, has many explanatory gaps but an extraordinary predictive power.

I cannot help also noticing in the human sciences what Feyerabend decried as methodological bias. Attending many congresses of social psychologists or sociologists will leave you dismayed: what really matters for most of them is to show the refined method they have followed to carry out a certain investigation, but applied to absolutely irrelevant topics. What we aim to understand no longer counts, only the methodology matters… Yet this is like using the most advanced cannons to kill a fly. In the humanities, too, therefore, the method should serve the intelligibility of the object of research. The important thing is to understand the world, and different tools can be used depending on the occasion.

But in that case, why do so many scientists — including their caricatures, i.e. certain types of social scientists — believe that what really matters is the method employed? In my opinion, because true discoveries are rare, and what most scientists produce will turn out to be negligible, not everyone has enough luck or enough genius. What determines the academic prestige of most scientists is therefore not the fact that they have produced new theories or discoveries, but the fact that they have always followed *the correct method*. Putting methodology first serves to protect one's mediocrity — something which sometimes then functions as a challenge to the creativity of other "incorrect" colleagues.

So, Feyerabend does not entertain the Kuhnian image of science as consisting of legions of "normal" scientists trying to solve puzzles within a paradigm, but rather the revolutionary image of scientists not caring about "good forms": an ideal of science in permanent revolution, but also a revolution made by creative scientists.

We should, moreover, read Feyerabend with the irony typical of Feyerabend himself.

6. Radical Pluralism

All the positive statements made by Feyerabend, a person with a great sense of humour and a taste for paradox, should be taken as impertinent *negations* of rationalist assertions. In short, the sense of his statements is almost always deconstructive. For instance, his "anything goes", as he said himself, is to be taken as the conclusive exclamation of a rationalist once he has taken a closer look at the history of science. This suggests that, after all, it is not true that anything goes, even though Feyerabend never says what should be considered wrong. His basic idea is that philosophy's claim to tell us what is right (science, truth) and what is wrong (myths, religions, metaphysics) is illusory: it is history, i.e. life, that selects. In essence, his anarchism is a *reductio ad absurdum* of rationalism, a little like Zeno's paradoxes.

Something similar should be said about his idea that scientific theories are largely *incommensurable*. This idea of incommensurability brings Feyerabend very close to Foucault. (Is Feyerabend the Foucault of science?) And, indeed, there was mutual respect between the two, despite their very different cultural backgrounds.

Prejudices thrive on the subject of incommensurability, too. To say that two theories are incommensurable by no means amounts to saying that there is an incommunicability between their proponents, that it is not possible to compare them. [8] When, in geometry, we say that the diagonal of a square is incommensurable with the length of the sides of a square, we don't mean that we can't apply the

[8] See Sergio Benvenuto, "Incommensurability and Relativism. A Discussion about Paul Feyerabend's Thought", (F)luxury 2016, https://tiny.pl/cs1l7 [12.10.2023].

same units of measurement to the two quantities! We only mean that it is impossible to find a segment small enough to fit a whole number of times into both lengths. In short, it is not possible to completely translate two incommensurable quantities into each other: there will always be a remainder, a plus or a minus that makes it impossible to reduce the two quantities to multiples of certain invariant concepts. This remainder that cannot be evacuated is at the root of deep misunderstandings in discussions between scientists, too: we may use the same words, but in fact the meaning we give to these words is not superimposable, which is why an agreement will never be reached. In short, in a dispute, the meaning of the words we use is not fixed once and for all, but is negotiated and shifts constantly. As we can see, this cripples any philosophy of universal dialogue and communication. Our debates, including our philosophical ones, are always exposed to the different implications that our concepts have for each of us. Communication between humans takes place not in spite of misunderstanding, but because of it.

We should also say that scientific theories and paradigms are incommensurable from a realist point of view. If realism is abandoned, incommensurability falls.

Hence the idea of a radical *pluralism*. This is what interests Feyerabend: a polyphonic vision not only of science, but of culture in general. And hence of Being in general. That recursive non-coincidence that ensures incommensurability is the lifeblood of cultural progress.

This is the end of a *single key* to interpreting history, and that is even so in the case of the evolutionary sciences. Although Darwinism remains the main biological theory of the history of life, many tend towards a *weak Darwinism*, or even refuting Darwinism, on the basis of evidence to the effect that not everything in life is adaptive — as we saw in the case of deer antlers. [9] In other words, *there is no single principle that governs the history of life*, not even the Darwinian principle of mutation and selection. And so, in human history too, there is no single impulse that explains it: neither class struggle (Marxism), nor the craving for freedom (liberalism), nor adaptation to environments, nor the will to power (Foucault), etc. History, as it pertains both to life and to cultures, is chaotic; it does not express a

[9] See Jerry Fodor and Massimo Piattelli-Palmarini, **What Darwin Got Wrong**, Picador, London 2011.

single principle. Furthermore, the result is the fundamental unpredictability of the world of life — and hence of scientific life too.

In essence, Feyerabend decries the rationalist *fury* that tends to oversimplify the world, because for him Being is abundance, unlimited wealth. [10] Against a supposed a "single thought" — in science as in politics — he opposes his "principle of proliferation": It is better to have as many theories as possible, even if some are bizarre. Democratic pluralism must be accompanied by epistemic pluralism. [11] After all, pluralism is already inherent in the diverse variety of thinkers to whom he claims to be indebted: Aristotle, Hegel, Marx, Kierkegaard, Mill and Wittgenstein. The Whole is Feyerabend's favourite target: his world is made up of parts that do not add up to a whole. The irreducible plurality of theories and paradigms points to a plurality of Being itself, to a multiplicity of worlds.

This pluralism leads to a refutation of the idea of 'unity of knowledge', which is why many philosophers speak not of Science but of *the sciences* in the plural. In the wake of this, the so-called "Stanford School of Philosophy of Science" and Ian Hacking also begin from this plurality: to ontologically affirm a plurality of worlds and thus challenge the substantial reductionism that still permeates most scientists' view of scientific knowledge. [12]

Hence Feyerabend's criterion, also provocative, of "unscrupulous opportunism". That is, when scientists feel that something is true, they can resort to the most suitable arguments and persuasions, bordering on lying. I wonder if Feyerabend was struck by Orson Welles's film *Touch of Evil* (1958): the hellish police captain Quinlan might have served as a model for his *opportunism of truth*.

After all, today's physicists are all opportunists in the Feyerabendian sense, since they refer to two mutually incongruent theories, relativity and quantum mechanics. Some try in vain to find a synthesis between the two, but in fact both are used in physics. Indeed, anything goes, as long as it works.

---

[10] See Feyerabend Paul K., **Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago 1999.

[11] See Sergio Benvenuto, "Paul K. Feyerabend (1924–1994) — Search for Abundance", *Télos* 1995, Vol. 107, Winter 1995, pp. 107–114, https://doi.org/10.3817/1295102107.

[12] See Ian Hacking, **Why Does Language Matter to Philosophy?**, Cambridge University Press, Cambridge 1975; Ian Hacking (ed.), **Scientific Revolutions**, *Oxford Readings in Philosophy*, Oxford University Press, Oxford 1981.

His ontological image of the world is one of irreducible chaos. Where knowledge is concerned, Homo sapiens continually tends to simplify the extreme complexity of the world in order to try and survive inside it; but in this way knowledge distances us from the real. Hence the contradictory double vocation of knowledge: on the one hand to render perceptible *the excessive abundance of* entities (approaching the real), on the other hand to reduce this abundance (favouring survival). Our need to survive is certainly the spur for knowledge, but also the source of our will to ignorance.

Feyerabend writes: "»Is it not possible«, asks Kierkegaard, »that my activity as an objective [or critico-rational] observer of nature will weaken my strength as a human being?« I suspect the answer to many of these questions is affirmative". [13] For Feyerabend, this *strength* is more important than objectivity, even though the *effort* to be objective is human too.

This helps us understand the reason for certain provocative proposals Feyerabend made, which have led us to think that he was posing just to shock us (*épater*) — such as when he recommends diverting funds from research into elementary particles in order to bestow them on astrology, homeopathy, theology, etc. What appears to be a quixotic challenge to the huge scientific establishment is actually a corollary of its own pluralism: science has produced so much because research programmes have proliferated. In other words, Feyerabend would like to apply to science the same criterion of diversification that has become common in ecological policies: the great diversity of animal and plant species, as well as the great diversity of languages, cultures, beliefs and techniques is a value in itself. Difference is wealth. A standardized world kills both biological and cultural creativity. This is why western countries today are careful not to destroy archaic crops, traditional forms of life, cultural fossils, etc. — in line with the principle that the more culturally and biologically diverse a nation is, the more it can adapt to new situations and the more creative it will be. The very excellence of the USA over the last two centuries can be explained by its being a composite country, made up of many waves of migration and many religions. Thus, in a natural or nuclear catastrophe, certain archaic forms of life could prove extremely useful for humanity to survive.

[13] Paul K. Feyerabend, **Against Method. Third Editon**, Verso, London — New York 1993, p. 154.

It is strange that Feyerabend is still seen as a sort of terrorist in philosophy of science, considering that, after all, the pluralism he proposes increasingly infuses the most economically and culturally advanced societies. Our world is becoming more and more Feyerabendian, without us realising it.

Finally, I come to my personal path of reflection on science. I think that Feyerabend, by bringing to a close the long tradition of philosophies of method stretching from Descartes to Popper, has contributed to overcoming two ever-opposing visions: one that hinges on the contemplative objectivity of knowledge and the other that, from Nietzsche onwards, makes knowledge a very human instrument for power, domination and survival. I am inclined to regard both approaches as capturing something of the truth. My view is that today's scientific knowledge is not a mirror of being, but rather the result of all the *questions that* human beings have asked Nature over the centuries, and to which It has responded. Knowledge is the result of a game with Nature. This game is based on *allowing Nature to speak*, albeit through a priori prepared protocols. Science puts Nature on parole, it "coerces" it, but it gives it sufficient freedom to answer as it wishes or turn us down. (And we know full well that Nature often turns us down. For example, it has never answered the question "Does light consist of waves or particles?") The advantage of scientific knowledge, compared to all other discourses that question being, is this allowing of Nature, at some point, to speak. Many other "games" continue to seek truth, alongside science. But science, by letting Nature answer certain "referendum" questions, is the game closest to today's pluralist and liberal democracy.

Sergio Benvenuto

References

1. Benvenuto Sergio, "Paul K. Feyerabend (1924–1994) — Search for Abundance", *Télos* 1995, Vol. 107, Winter 1995, pp. 107–114, https://doi.org/10.3817/1295102107.

2. Benvenuto Sergio, "Incommensurability and Relativism. A Discussion about Paul Feyerabend's Thought", (F)luxury 2016, https://tiny.pl/cs1l7 [12.10.2023].

3. Dawkins Richard, **The Selfish Gene**, Oxford University Press, Oxford 1976.

4. Eldredge Niles and Gould Stephen J., "Punctuated Equilibria: an Alternative to Phyletic Gradualism", in: Thomas J. M. Schopf (ed.), **Models in Paleobiology**, Freeman, San Francisco 1972, pp. 82–115.

5. Feyerabend Paul K., **Against Method. Third Editon**, Verso, London and New York 1993.

6. Feyerabend Paul K., **Against Method. Fourth Edition**, preface by Ian Hacking, Verso, London 2010.

7. Feyerabend Paul K., **Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago 1999.

8. Feyerabend Paul K., **Wissenschaft als Kunst**, Suhrkamp, Berlin 1984.

9. Feyerabend Paul K., **Against Method: Outline of an Anarchistic Theory of Knowledge**, preface by Ian Hacking, 4th edition, Verso, London 2010.

10. Fodor Jerry and Piattelli-Palmarini Massimo, **What Darwin Got Wrong**, Picador, London 2011.

11. Hacking Ian, **Why Does Language Matter to Philosophy?**, Cambridge University Press, Cambridge 1975.

12. Hacking Ian (ed.), **Scientific Revolutions**, *Oxford Readings in Philosophy*, Oxford University Press, Oxford 1981.

13. Panofsky Erwin, **Galileo as a Critic of the Arts**, Springer, New York 1954.

14. Pievani Telmo, **Imperfection: A Natural History**, The MIT Press, Boston 2022.

15. Planck Max K., **Scientific Autobiography and Other Papers**, Philosophical Library, New York 1950.

16. Rorty Richard, **Philosophy and the Mirror of Nature,** Princeton University Press, Princeton 1979.

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 203–233

https://doi.org/10.53763/fag.2023.20.2.228

ARTYKUŁ ORYGINALNY / ORIGINAL ARTICLE

Krzysztof J. Kilian

Uniwersytet Zielonogórski

What is Epistemological Anarchism?

Received: January 30, 2024. Accepted: February 22, 2024. Published online: April 11, 2024.

Abstract: Epistemological anarchism is a methodological fallibilism, i.e. an approach according to which all scientific knowledge is not only provisional, but so are the methods of acquiring it. It is a belief that we are doomed to a provisional character of knowledge, yet the guide to this provisionality is a selected methodology. This conviction is contrasted with the belief that not only are we doomed to a provisional character of knowledge, but that we have no *permanent* guide to this provisional knowledge.

Keywords:

Against Method; anything goes; epistemological anarchism; methodological fallibilism

1. Preliminary remarks

In addition to the introduction, this article consists of three sections and the conclusion. In section two, the first element of Paul K. Feyerabend's epistemological anarchism is presented — his account of scientific theories as worldviews. In section three, the general presuppositions of Feyerabend's anarchism are discussed, i.e. those of his beliefs found in all editions of his *Opus magnum*. And in the fourth section, the most important in this article, a specific interpretation of his anarchist approach to knowledge, based mainly on his own statements, is presented.

2. Weltanschauungen analyses

Distinguishing between the two basic periods of Feyerabend's work: the moderate (early) and the anarchist (late) one, [1] can be found in the vast majority of studies devoted to his work. [2] The two periods are linked above all by the tremendous emphasis on history and the blurring distinction between the context of discovery and the context of justification.

The philosophy of science propagated by Feyerabend, both during the moderate (from 1962 onwards) [3] and the anarchist periods, falls within an approach

[1] The moderate period begins with a brief encounter with logical empiricism. It is followed by a critique aimed at positivist philosophy. This critique evolves into the constructive stage in which his own counter-methodology is suggested, a variant of critical rationalism (for discussion, see Eric Oberheim, **Feyerabend's Philosophy**, "Quellen und Studien zur Philosophie", Bd. 73, Walter de Gruyter, Berlin — New York 2006, pp. 4–5, 77–122) aimed at the positivist approach. The anarchist period represents a break with all (including his own) attempts to create a constructive methodology and challenges the assumption of the existence of a form of rationality characteristic of science that allows it to be clearly distinguished from other forms of human cultural activity.

[2] See e.g., John Preston, **Feyerabend: Philosophy, Science and Society**, Polity Press, Cambridge — Malden 1997, p. 7. Incidentally, Feyerabend seemed to accept this division of his philosophy: "He [Richard M. Burian] didn't like the early PKF for being too rational, and he doesn't like the late PKF for being too irrational". Feyerabend's letter to Lakatos, dated 25 July 1969, in: Imre Lakatos, Paul K. Feyerabend, **For and Against Method: Including Lakatos s Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence**, Matteo Motterlini (ed.), The University of Chicago Press, Chicago — London 1999, p. 169.) For discussion, see Oberheim, **Feyerabend's Philosophy...**, pp. 15–16, 262–283.

The use of the terms "early" and "late" Feyerabend, despite the fact that the American philosopher classified himself in this way, is misleading insofar as it focuses attention on time rather than on the views themselves. Interpreting someone's work involves, among other things, a search for turning points that allow one to see the differences between views separated by more than just some period of time. Much better suited to such a search, with significant differences in the views of the American philosopher pinpointed, is the nomenclature proposed by Kazimierz Jodkowski: the moderate and anarchist period. See Kazimierz Jodkowski, „Filozofia nauki Paula K. Feyerabenda. Stadium umiarkowane", *Studia Filozoficzne* 1979, Nr 11, s. 59 [59–75].

[3] Historical considerations appear in Feyerabend's paper "Explanation, Reduction and Emipiricism". As late as 1960, in his first letter to Kuhn, he argued that "history is irrelevant to methodology" (Paul K. Feyerabend, "Two Letters of Paul Feyerabend to Thomas Kuhn on a Draft to **The Structure of Scientific Revolutions**", Paul Hoyningen-Huene (ed.), *Studies in History and Philosophy of Science* 1995, Vol. 26, No. 3, p. 366 [353–387], see also p. 367). See also Paul K. Feyerabend, "Concluding Unphilosophical Conversation", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul K. Feyerabend**, *Boston Studies in the Philosophy of Science*, Vol. 132, Kluwer Academic Publishers, Dordrecht — Boston — London 1991, s. 506 [487–528]; Paul K. Feyerabend, **Sci-**

known as: "a »revolutionary« philosophy of science", [4] "a new philosophy of science", [5] "postmodernism", [6] "post-empiricism", [7] "new empiricism", [8] "postpositivism", [9] "historicist philosophy of science". [10] Feyerabend preferred the term "historical approach" to describe this approach to science. [11] One of the hallmarks of the historical philosophy of science is the extensive historical analyses of episodes in the history of science. The other is the conviction that analyses from the field of the context of discovery should not be ignored and that historically variable factors shaping the development of science should not be removed from the field of the study of science. Both of these elements can easily be found in Feyerabend's writings following 1962. [12]

---

ence in a Free Society, Verso, London 1983, p. 117, n. 49; Paul K. Feyerabend, **Against Method: Revised Edition**, Verso, London — New York 1988, p. 230; Paul K. Feyerabend, "More Clothes from the Emperor's Bargain Basement. A Review of Laudan's. Progress and its Problems" (1981), in: Paul K. Feyerabend, **Philosophical Papers. Vol. 1. Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester, Melbourne — Sydney 1981, p. 238, n. 19 [231–246]; Paul K. Feyerabend, **Killing Time**, University of Chicago Press, Chicago and London 1995, p. 141.

[4] See John McEvoy, "A »Revolutionary« Philosophy of Science: Feyerabend and the Degeneration of Critical Rationalism into Sceptical Fallibilism", *Philosophy of Science* 1975, Vol. 42, No. 1, pp. 50–51 [49–66].

[5] See e.g., Kazimierz Jodkowski, „Polskie wydanie rozpraw Feyerabenda", *Studia Filozoficzne* 1981, nr 2 (183), p. 161 [159–169]; Agustín Adúriz-Bravo, "Methodology and Politics. A Proposal to Teach the Structuring Ideas of the Philosophy of Science through the Pendulum", *Science & Education* 2004, Vol. 13, p. 721 [717–731].

[6] See e.g., Adúriz-Bravo, "Methodology and Politics…", p. 721.

[7] See e.g., Stephan Fuchs, "Metatheory and the Sociology of Sociology", *Sociological Perspectives* 1992, Vol. 35, No. 3, p. 533 [531–535].

[8] See e.g., George Gale and Edward Walter, "Kordig and the Theory-Ladenness of Observation", *Philosophy of Science* 1973, Vol. 40, No. 3, p. 415 [415–432].

[9] See Ernan McMullin, "Review of Science, Revolution, and Discontinuity", *Isis* 1983, Vol. 74, No. 4, p. 577 [577–579].

[10] See e.g., Thomas Nickles, "Historicist Theories of Scientific Rationality", in: Edward N. Zalta (ed.), **The Stanford Encyclopedia of Philosophy**, Spring 2021 Edition, https://tiny.pl/czp6f (27.01.2024).

[11] See Paul K. Feyerabend, **Farewell to Reason**, Verso, London, New York 1996, p. 265.

[12] As an advocate of the historical approach to the philosophy of science, Feyerabend repeatedly stressed the links between methodology and history and research practice, but in the moderate period of his work he was – and this clearly distinguishes the two periods of his work — also an advocate of "the primacy of method over history" (Paul K. Feyerabend, **Science in a Free Society**, Verso,

Still the most accurate name for this approach, which treated scientific theories and the practice of science as part of a broader whole determining certain types of questions and criteria for acceptable answers to the former, was put forward by Frederick Suppe, who dubbed it "Weltanschauungen analyses": [13]

> [S]cience is done from within a *Weltanschauung* or *Lebenswelt*, and the job of philosophy of science is to analyze what is characteristic of scientific *Weltanschauungen*, what is characteristic of the linguistic-conceptual systems from within which science works. Theories are interpreted in terms of the *Weltanschauung*; hence to understand theories it is necessary to understand the *Weltanschauung*. Such a *Weltanschauungen* approach to analyzing the epistemology of science obviously must pay considerable attention to the history of science and the sociological factors influencing the development, articulation, employment, and acceptance or rejection of *Weltanschauungen* in science. [14]

Science is seen here as a social undertaking that cannot be fathomed by analysing merely the context of justification. Proper understanding of science is possible after taking into account the metaphysical and methodological views that co-create it, the active role of language in its practice, the interactions of a social and psychological nature. According to this approach, evaluations and methodological decisions, and the content of newly developed scientific assertions do not depend solely on facts and logic, observation and careful thinking. [15] Theories do not arise in an intellectual vacuum; the growth of knowledge is shaped by factors such as: the influence of different traditions of doing science on the formation of different beliefs and prejudices of scholars participating in different traditions; motives of an aesthetic, metaphysical and volitional nature allowing a scholar to insist on their chosen — against the facts and well-validated theories — path of inquiry; acquiring by scholars of certain character traits, such as susceptibility (or its lack) to a certain type of suggestion. [16]

London 1983, p. 160, n. 17).

[13] Frederick Suppe, "Search for Philosophic Understanding of Scientific Theories", in: Frederick Suppe (ed.), **The Structure of Scientific Theories**, University of Illinois Press, Urbana — Chicago — London 1977, p. 125 [1–241].

[14] Suppe, "Search for Philosophic Understanding of Scientific Theories…", pp. 126–127.

[15] See e.g., Paul K. Feyerabend, "Problems of Empiricism", in: Robert G. Colodny (ed.), **Beyond the Edge of Certainty: Essays in Contemporary Science and Philosophy**, Prentice-Hall, Englewood Cliffs, New Jersey 1965, p. 152–153 [145–260].

According to Feyerabend, the way of seeing the world depends on the previously accepted theory. [17] General theories have their own ontologies and they are worldviews. „To understand a theory — Suppe concludes — was to understand its use and development". [18]

3. General assumptions and goals of epistemological anarchism

Four editions of Feyerabend's *Opus magnum* [19] were published during his lifetime, which in many respects differ from each other. [20] However, it is possible to

[16] See e.g., Paul K. Feyerabend, "Explanation, Reduction and Empiricism" (1962), pp. 59–60 [44–96]; Paul K. Feyerabend, "How to be a Good Empiricist: A Plea for Tolerance in Matters Epistemological" (1963), in: Paul K. Feyerabend, **Philosophical Papers**. **Vol. 3. Knowledge, Science and Relativism**, John Preston (ed.), Cambridge University Press, Cambridge — New York — Melbourne — Madrid — Cape Town — Singapore — São Paulo 2008, p. 81, n. 4 [78–103]; Paul K. Feyerabend, "Realism and Instrumentalism…", p. 196 [176–202]; Feyerabend, "Problems of Empiricism…", p. 219, n. 5, pp. 219–220, n. 8, p. 224, n. 9; Paul K. Feyerabend, "Consolations for the Specialist" (1970), in: Paul K. Feyerabend, **Philosophical Papers. Vol. 2. Problems of Empiricism**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, p. 160 [131–167]; Paul K. Feyerabend, "Against Method. Outline of an Anarchistic Theory of Knowledge", in: Michael Radner and Stephen Winokur (eds.), **Analyses of Theories and Methods of Physics and Psychology**, *Minnesota Studies in the Philosophy of Science*, Vol. 4, University of Minnesota Press, Minneapolis 1970, p. 90 [17–130]; Paul K. Feyerabend, **Against Method: Outline of an Anarchistic Theory of Knowledge**, New Left Books, London 1975, pp. 284–285; Feyerabend, **Against Method...** (1988), p. 226.

[17] See e.g., Paul K. Feyerabend, "An Attempt at a Realistic…", p. 31 [17–36]; Feyerabend, "How to be…", pp. 97–98; Feyerabend, "Problems of Empiricism…", p. 181; Feyerabend's letter to Lakatos, dated 10 March 1970, in: Lakatos and Feyerabend, **For and Against Method**…, pp. 194–195; Feyerabend, "Against Method…" (1970), p. 90; Feyerabend, **Against Method...** 1975, p. 284; Paul K. Feyerabend, "Reply to Criticism…", p. 126 [104–131].

[18] Suppe, "Search for Understanding of Scientific Theories…", p. 126.

[19] 1970 — a very comprehensive article and in 1975, 1988 and 1993 three different editions of the book. The fourth edition marked as "New Edition" was published in 2010, but it is, in fact, virtually identical to the third (1993) edition.

[20] A thorough discussion of these differences far exceeds the aims of this paper. Overall, the 1970 edition is an attempt at a dialectical explanation of the process of the development of science. In this edition, Feyerabend refers to both Hegel and the classics of Marxism. He seeks support for his theses in source material from the history of ideas, philosophy, politics (this abundance of political material reflected Feyerabend's belief in the fundamental similarity between social revolutions and revolutions in science) and science. The 1975 edition, as part of a book planned with Imre Lakatos,

distinguish a number of elements in them that are common to all editions of **Against Method**. [21]

Feyerabend permanently supported the thesis that all methodologies include cosmological presuppositions. [22] He thus based his anarchist methodology on the following assumption:

> People and nature are very whimsical entities which cannot be conquered and understood if one decides to restrict oneself in advance. [23]

This assumption was followed by the following methodological assumption:

> [Epistemological] anarchism is not only possible, it is necessary both for the internal progress of science and for the development of our culture as a whole [...] [because]

For and Against Method, is written in a much more provocative style than the 1970 essay. The material with which Feyerabend supports the defended theses also changes there: "All the political material my article contained has been omitted, and has been replaced by more material from science and the history of science", Feyerabend wrote in 1975 in a letter to Kazimierz Jodkowski. The 1988 edition is an attempt to formulate his own philosophical position, taking into account the views presented in **Science in a Free Society**, based on epistemological relativism (see **Against Method...** 1988, p. 230), while the 1993 edition is an attempt to reinterpret **Against Method** in the light of the views presented in Feyerabend's **Farewell to Reason**.

[21] In the first known surviving letter of the correspondence between Feyerabend and Lakatos, dated 17 Dec. 1967, Feyerabend explained where the idea for such a title for his work came from: "the title will be »Against Method« (this in analogy to Susan Sontag's »Against Interpretation«)", Lakatos and Feyerabend, **For and Against Method...**, p. 125.

[22] See e.g., Paul K. Feyerabend, "On the Critique of Scientific Reason", in: Robert S. Cohen, Paul K. Feyerabend, Marx W. Wartofsky, **Essays in Memory of Imre Lakatos**, *Boston Studies in the Philosophy of Science*, Vol. 39, D. Reidel Publishing Company, Dordrecht, Holland 1976, p. 138, n. 30a [109–143]; Paul K. Feyerabend, "The Methodology of Scientific Research Programmes", p. 212, n. 18 [202–230]; Feyerabend, "Against Method..." (1970), pp. 44–45; Feyerabend, **Against Method...** (1975), p. 67; Feyerabend, **Science in a Free Society...**, p. 34; Feyerabend, **Against Method...** (1988), p. 53; Paul K. Feyerabend, **Against Method: Third Edition**, Verso, London 1993, p. 52; Paul K. Feyerabend, "Professor Bohm's Philosophy of Nature", pp. 227–228 [219–235]; Feyerabend, "Problems of Empiricism...", p. 216; Feyerabend, "An Attempt at a Realistic Interpretation...", pp. 35–36; Paul K. Feyerabend, "The Problem of the Existence of Theoretical Entities", pp. 19, 46, 49 [16–49]; Feyerabend, "Explanation, Reduction...", pp. 52–53; Paul K. Feyerabend, "On the »Meaning« of Scientific Terms", pp. 98–99 [97–103]; Feyerabend, **Farewell to Reason**..., p. 8; Paul K. Feyerabend, "Rationalism, Relativism and Scientific Method", pp. 201–203 [200–211]; Paul K. Feyerabend, "Introduction to Volumes 1 and 2", pp. IX–X [IX–XIV].

[23] Paul K. Feyerabend, "Theses on Anarchism", p. 116 [113–118]. "The world, including the world of science, is a complex and scattered entity that cannot be captured by theories and simple rules" (Feyerabend, **Killing Time...**, p. 142).

> anarchism helps to achieve progress in anyone of the senses one cares to choose. [24]

Anarchism makes it possible to achieve progress understood in this way, [25] as it always allows such methods as are necessary to achieve the assumed goal in a particular situation.

Two epistemological assumptions made by Feyerabend, which support the above methodological assumption, should also be mentioned at this point. According to the first, science is not the only effective way of acquiring knowledge:

> Nor is there one way of knowing, science; there are many such ways, and before they were ruined by Western civilization, they were effective in the sense that they kept people alive and made their existence comprehensible. [26]

According to the other, what is called science is not an ordered system of non-contradictory assertions:

> Science itself has conflicting parts with different strategies, results, metaphysical embroideries. It is a collage, not a system. [27]

Feyerabend admitted that he wrote "essays which upset people", [28] as "there is more to this »anarchism« than rhetoric". [29] That epistemological anarchism is more than rhetoric is evident from the goals he set for his undertaking:

---

[24] Feyerabend, **Against Method...** (1975), p. 180 and p. 27. See also Feyerabend, **Against Method...** (1975), pp. 37, 171, 175, 180; Feyerabend, "Against Method…" (1970), pp. 17, 21, 76; Feyerabend, **Against Method...** (1988), pp. 9, 14, 19, 32, 33, 160, 164, 249; Feyerabend, **Against Method...** (1993), pp. 9, 18, 159, 231; Feyerabend, **Science in a Free...**, p. 142.

[25] During the moderate period, he conceived of progress as moving, according to very simple methodological rules (see Feyerabend, „Problems of Empiricism…", p. 217), to theories that were inconsistent with previous theories (see Feyerabend, „Problems of Empiricism…", p. 172).

[26] Feyerabend, **Killing Time...**, p. 143. See also Feyerabend, **Against Method...** (1975), pp. 180, 217, 296, 298–299; Feyerabend, **Against Method...** (1988), pp. 3, 37, 169, 170, 256, 257–258, 260–261; Feyerabend, **Against Method...** (1993), pp. 3, 160, 163, 214, 229, 238, 246.

[27] Feyerabend, **Killing Time...**, p. 143. See also Feyerabend, "Against Method…" (1970), pp. 20, 24, 42, 80–81; Feyerabend, **Against Method...** (1975), pp.19, 23, p. 24, n. 1, pp. 46, 64, 68, 146, 179–180; Feyerabend, **Against Method...** (1988), pp. 11, 15, n. 1, pp. 49, 53–54, 59, 111, 121, 153, 156, 164, 205, 249–250; Feyerabend, **Against Method...** (1993), pp. x–xi, 11, 15, n. 1, pp. 49, 148, 157–158, 160, 196, 207, 231–232, 249.

[28] See Joachim Jung, "Paul K. Feyerabend. Last Interview", in: John Preston, Gonzalo Munévar, David Lamb (eds.), **The Worst Enemy of Science? Essays in Memory of Paul Feyerabend**, Oxford University Press, New York, Oxford 2000, p. 165 [159–168].

> Anger at the wanton destruction of cultural achievements from which we all could have learned, at the conceited assurance with which some intellectuals interfere with the lives of people, and contempt for the treacly phrases they use to embellish their misdeeds was and still is the motive force behind my work. [30]

It is worth pausing on this objective for a moment, since with it another general assumption underlying Feyerabend's anarchism comes to the fore, concerning how societies develop. This development, in his view, is achieved through competing (conflicting) cultures:

> nations, kingdoms, and tribes were often at war with each other but they exchanged materials, languages, industries, styles, people with special skills such as architects, navigators, prostitutes — and even gods [...]. [31]

and cultural exchange:

> The participants [of a given tradition] get immersed into each others' ways of thinking, feeling, perceiving to such an extent that their ideas, perceptions, world views may be entirely changed — they become different people participating in a new and different tradition. [32]

Returning now to Feyerabend's aims in pursuing his objective, he wanted to show that the philosophical conviction that there is a monolith called science, which is cemented by the concept of truth, is erroneous. [33] He also sought to show that there is no such definition of science that is able to encompass all the trans-

---

[29] Feyerabend, **Killing Time...**, p. 142.

[30] Feyerabend, **Against Method...** (1988), p. 272; Feyerabend, **Against Method...** (1993), p. 252. See also Feyerabend "Against Method…" (1970), p. 111, n. 49; Feyerabend, **Against Method...** (1975), p. 188, Feyerabend, **Science in a Free Society...**, p. 136.

[31] Feyerabend, **Farewell to Reason...**, pp. 6–7.

[32] Feyerabend, **Science in a Free Society...**, p. 9; Feyerabend, **Against Method...** (1988), p. 246. I have presented here a highly simplified picture of Feyerabend's theory of social exchange, which does not take into account his change of view that occurred between **Science in a Free Society** (see e.g., p. 83, see also Feyerabend "Against Method..." (1970), p. 108, n. 46) and **Farewell to Reason** (see e.g., p. 40). He later replaced his earlier conviction that unfamiliar social views and practices should be developed regardless of the possible consequences of such actions with the thesis that they should only be introduced when existing problem-solving strategies have failed (see e.g., Feyerabend, "Concluding Unphilosophical...", p. 519). Initially (**Science in a Free Society**), Feyerabend wanted to extend Mill's liberalism by advocating, not, like Mill, allowing freedom in societies mature enough, but rather making freedom a condition of maturity. Later (**Farewell to Reason**), returning to Mill's approach, he restricted equal rights for all traditions to democratic societies.

formations that science undergoes. [34] He also argued that traditional philosophical accounts of both scientific knowledge and scientific method are wrong as scientists do not act rationally, in the sense that philosophers of science attribute to the term. [35] The latter limit rationality to some clearly articulated and supra-historical set of rules that must always be followed. [36] The conduct of scholars/researchers who ignore such rules is neither arbitrary nor unsystematic. It can only be regarded as such in the perspective of rationalist standards. [37]

It was for this reason in **Against Method** he comes out against mainly those philosophers who urged reconstructions of what scientists actually said and did, for the latter, in the opinion of philosophers of science, did not do so clearly enough. The essential aim of the philosophy of science was therefore to offer such rational reconstructions. His book, he stated, primarily attacks the philosophy of science conceived as a method of making sense of the complex issues that scholars/researchers talk about. [38]

He also wanted to demonstrate in **Against Method** that the methodological principles presented not only by philosophers of science but, more importantly, by scholars/researchers themselves, are always violated when some scientific breakthrough occurs. The absence of such immutable principles does not prove that "science is […] »irrational«; every single step can be accounted for". [39] The steps of scholars can be explained, that is to say, why they have acted in this way and not in that way. [40]

---

[33] See Renato Parascandalo and Vittorio Hösle, "Three Interviews with Paul K. Feyerabend", *Teleos. A Quarterly Journal of Critical Thought* 1995, No. 102, p. 118 [115–148].

[34] See Feyerabend, "Concluding Unphilosophical…", p. 515.

[35] See Paul K. Feyerabend, "Preface to the Second Edition", in: Feyerabend, **Farewell to Reason…**, p. V [V–VIII].

[36] See Jung, "Paul K. Feyerabend…", p. 162; Feyerabend, **Against Method…** (1993), p.1; Feyerabend, **Against Method…** (1988), p. 1.

[37] See Feyerabend, "Concluding Unphilosophical…", p. 503, see also **Against Method…** (1988), p. 46, n. 19; **Against Method…** (1993), p. 46, n. 20.

[38] See Parascandalo and Hösle, "Three Interviews with Paul…", p. 117.

[39] Feyerabend, **Killing Time…**, p. 91.

[40] Feyerabend's letter to Ben-Israel, dated 22 Jan. 1989, in: Isaac Ben-Israel, "Philosophy and Methodology of Military Intelligence. Correspondence with Paul Feyerabend", *Philosophia* 2001, Vol. 28, No. 1–4, p. 80 [71–101].

When writing **Against Method**, he did not intend to replace well-known, long and old methodological dogmas with a short and new dogma of his own making. He wanted to "let the sciences speak for themselves", [41] which no methodological system can guarantee, but he did not want "to add myths of his own to the myths of the scientists". [42] His anarchism was therefore not aimed at eliminating methodology, its aim was to reform it. Practical rules adapted to a specific situation replace here the universal standards assumed by other philosophers. [43] The critique of these standards was to be yet the beginning of a better understanding of the sciences, of a better life, of better human relationships. [44]

4. The anarchistic approach to knowledge

This reform of methodology essentially consisted of combining Weltanschauungen analyses with epistemological anarchism, resulting in a very specific approach that was described as "the last »move« in the evolution of twentieth-century philosophy of science". [45] The essence of this move is expressed in the following Feyerabend's thesis: „Science is an essentially anarchistic enterprise […]". [46] Explaining why this was the last move requires three comments of historical nature.

There is no doubt that science has been cognitively successful, and that it has been the source of many practical benefits. [47] These facts mainly account for the

[41] See Feyerabend, "Concluding Unphilosophical…", p. 503.

[42] From an interview conducted by Teresa Ordunya in Berkeley, California, March 1981, https://tiny.pl/czl27 (28.01.2024).

[43] See Feyerabend, "Concluding Unphilosophical…", s. 503; Feyerabend's letter to Ben-Israel, dated 30 May 1989, in: Ben-Israel, "Philosophy and methodology of military intelligence…", p. 90.

[44] See Feyerabend, **Killing Time**…, p. 134.

[45] Kazimierz Jodkowski, "»Wszystko ujdzie«. Anarchizm epistemologiczny Paula K. Feyerabenda", *Akcent* 1982, nr 2 (8), p. 131 [127–134]. Denise Russell expressed a similar opinion, „Anything Goes", *Social Studies of Science* 1983, Vol. 13, No. 3, p. 437 [437–464].

[46] Feyerabend, **Against Method…** (1975), p. 17; Feyerabend, **Against Method…** (1988), p. 9; Feyerabend, **Against Method…** (1993), p. 9; see also Feyerabend, "Against Method…" (1970), p. 76.

[47] Even "the worst enemy of science", Paul K. Feyerabend, did not claim that this was not the case. See e.g., Feyerabend, **Killing Time…**, pp. 91, 151; Feyerabend, **Science in a Free Society…**, p. 101.

belief that there is a characteristic rationality of conduct in science. [48] This rationality manifests itself in beliefs according to which: there are science-specific reasons for accepting, rejecting or modifying theories; the whole range of methodological decisions other than those mentioned above, which scientists usually make, are also made in a science-specific way. [49]

The proponents of such an approach to the problem of scientific rationality intended only to describe it accurately if they accepted the descriptive character of methodology (e.g. Henri Poincaré), [50] or to constitute it, if they regarded methodology as a normative discipline (e.g. Karl R. Popper). [51] However, two thick cracks have appeared on such an approach. [52]

The first serious and at the same time widely perceived crack [53] on this approach was Thomas S. Kuhn's view that periods of rational development of science are interspersed with "irrational" ones. The vast majority of the history of science consists of periods of normal science, i.e., the one practised on the basis of a specific paradigm (the disciplinary matrix) setting the specific standards of scientific rationality. In contrast, periods of scientific revolutions, during which this methodological superstructure is exchanged, are relatively rare and short phases in the development of science. These periods — characterized by a tendency to replace the existing criteria of rationality — are not, according to Kuhn, reconstructible on the basis of the previously accepted accounts of the problem of scientific rationality. For this reason, writing about interparadigmatic incommensurability, Kuhn argued against the belief that there are supra-historical criteria for evaluating theories and methodological rules. In his view, with the victorious sci-

[48] See e.g., Henri Poincaré, **Science and Method**, Thomas Nelson and Sons, London, Edinburgh, Dublin and New York 1914, pp. 22–23, 59–60, 275.

[49] See e.g., Karl R. Popper, **The Logic of Scientific Discovery**, Routledge Classics, London and New York 2002, p. 4.

[50] See Henri Poincaré, **Science and Hypothesis**, The Walter Scott Publishing Co, New York 1905, p. xxvii.

[51] See Popper, **The Logic of Scientific Discovery...**, p. 29.

[52] See e.g., Stefan Amsterdamski, **Między historią a metodą**, Państwowy Instytut Wydawniczy, Warszawa 1983, pp. 11–12.

[53] Before that, of course, there was Ludwik Fleck's conception of the thought collective (*Denkkollektiv*) expressed in **Entstehung und Entwicklung einer wissenschaftlichen Tatsache**. **Einführung in die Lehre vom Denkstil und Denkkollektiv** (Benno-Schwabe et Co. Verlag, Basel 1935).

entific revolution, not only do the theoretical beliefs change but so does the entire methodological superstructure changes.

The second significant, and at the same time the latest move in the evolution of twentieth century philosophy of science, crack on this view was Paul K. Feyerabend's epistemological anarchism. This anarchism has two essential components.

Feyerabend challenged the Kuhnian "punctuated equilibrium" thesis [54] by questioning the relevance of the division between the rational (scientific) and the irrational (non-scientific). He maintained that the elementary assumption of all the previous methodologies, including even the limited Kuhnian account, of the existence of a characteristic rationality of research procedure in science should be rejected. According to Feyerabend, every rule of doing science described or discovered by methodologists has been broken and replaced by another counter-rule. Therefore, such tactics of breaking valid methodological rules should not be reserved, as in Kuhn's view, only for the revolutionary periods, but should be applied simultaneously (revolution in permanence) [55] in revolutionary and normal periods (proliferation and tenacity in Feyerabend's terminology).

By adopting the incommensurability thesis, Feyerabend [56] did not only undermine the idea of the cumulative development of science and the belief that modern theories are better than their predecessors as they are devoid of superstition, that they are broader because they cover a wider range of phenomena, that they are deeper because they explain the studied aspect of the world by means of a smaller number of more basic principles. [57] He also rejected the thesis that in-

[54] Kuhn's approach is sometimes called "punctuated equilibrium". See Peter Godfrey-Smith, **Theory and Reality. An Introduction to the Philosophy of Science**, University of Chicago Press, Chicago and London 2003, p. 100.

[55] This slogan is coined in the moderate period of Feyerabend's work, see Feyerabend, "Problems of Empiricism…", p. 172.

[56] Kuhn acknowledged that Feyerabend began to use the term "incommensurability" independently, and this occurred during their discussions (1960–1961) on the manuscript **The Structure…**, see Thomas S. Kuhn, "Commensurability, Comparability, Comunicability", in: Thomas S. Kuhn, **The Road Since Structure. Philosophical Essays, 1970–1993, with an Autobiographical Interview**, James Conant and John Haugeland (ed.), University of Chicago Press, Chicago, 2000, p. 33, n. 1 [33–57].

[57] The fullest articulation of the incommensurability thesis can be found in the writings of Thomas S. Kuhn and Paul K. Feyerabend. However, they did not use the term "incommensurability"

commensurable theories can be compared objectively. The absence of such a standard for comparing incommensurable theories demonstrates that, at crucial periods in the development of science, the choice of theory is determined by factors that differ significantly from those considered by earlier philosophies of science.

The consequences of undermining the punctuated equilibrium thesis and accepting the incommensurability thesis [58] can be presented as follows.

What Feyerabend proposed in **Against Method** is neither a theory of the de-

perspicuously, leading to a number of misinterpretations of the thesis. The word itself has no sharply defined meaning in the philosophy of science, either. That issue lies far beyond the scope of this paper. For present purposes, I will make use of just one approach, which deals with the problem of the vagueness of this concept in such a way that it distinguishes five levels of incommensurability where scientific theories are concerned: quantitative variability of empirical consequences, observational variability, linguistic variability, variability with respect to scientific problems and evaluation criteria, and ontological variability (see Kazimierz Jodkowski, **Teza o niewspółmierności w ujęciu Thomasa S. Kuhna i Paula K. Feyerabenda**, *Realizm. Racjonalność. Relatywizm*, Vol. 1, Wydawnictwo UMCS, Lublin 1984, https://tiny.pl/tlk98 (28.01.2024); see also note 20 to Lakatos' letter to Feyerabend, dated 2 March 1968 added by Motterlini, in: Lakatos and Feyerabend, **For and Against Method...**, p. 133. In two of Feyerabend's texts one can find clues that allow for just such an interpretation of the thesis of incommensurability (see Paul K. Feyerabend, "Changing Patterns of Reconstruction", *British Journal for the Philosophy of Science* 1977, Vol. 28, No. 4, pp. 363–365 [351–369], see also p. 364 n. 3; Feyerabend, **Science in a Free Society...**, p. 66–67, see also p. 67, n. 114).

Despite the fact that the relationship of incommensurability is most often said to hold among scientific theories, not all such theories can be incommensurable. Indeed, according to Feyerabend this possibility holds only for realistically interpreted universal ones (see e.g., Feyerabend, **Against Method...** (1975), p. 114). Universal theories can be characterized in three ways. First, they are top-level theories: that is, theories that are not elements of other theories. The objects they speak of are neither defined independently from these theories, nor are we independently convinced of the existence of these objects (see Paul K. Feyerabend, "Physics and Ontology", in: Paul K. Feyerabend, **Philosophical Papers. Vol. 4. Physics and Philosophy**, Stefano Gattei and Joseph Agassi (eds.), Cambridge University Press, New York 2016, pp. 20–22 [9–24]). Second, they are theories that apply, at least in some respect, to everything that exists. They must provide the researcher with an adequate system of concepts for describing and explaining features of the world. They must also be sufficient to completely replace the previously accepted language and ontology (see Feyerabend's statement in Herbert Feigl, Paul K. Feyerabend, Norwood R. Hanson, Carl G. Hempel, Mary Hesse, Grover Maxwell and William Rozeboom, "Discussion at the Conference on Correspondence Rules", in: Radner and Winokur (eds.), **Analyses of Theories and Methods...**, p. 246 [220–259]). Third, they are theories that are distinguishable from (directly testable) empirical generalizations. Universal theories are themselves tested by deriving empirical generalizations from them and from certain boundary conditions (see Feyerabend, "Explanation, Reduction and Empiricism...", p. 44, n. 1).

Incommensurable theories are incompatible at *the level quantitative variability of empirical consequences*. This claim is valid for any pair of consecutive theories of a given field that stem from dif-

velopment of science nor idle rhetorics. [59] His anarchism is a specific approach to knowledge, a remedy to free science from the myth of a one-size-fits-all method.

> I introduce "anarchism" as a *medicine*, not as a final philosophy [...]. [60]

> I don't defend anarchism as an "eternal philosophy" but as a "medicine" (A[gainst] M[ethod], p. 17) that may have to be withdrawn when the conditions change (A[gainst] M[ethod], p. 22). [61]

Anarchism is the first step towards a new, more liberal form of rationality:

---

ferent theoretical principles. The basis of its validity, on which Feyerabend relies, is the difference in Galileo's and Newton's explanation of the free fall of bodies (see e.g., Feyerabend, "How to be a Good...", p. 84; Feyerabend, "Explanation, Reduction...", p. 58; Feyerabend, "Problems of Empiricism...", p. 168). On *the ontological level*, the incommensurability thesis states that when moving from one theory to another, fundamental beliefs about the structure of the world and the structure of each object are changed (see e.g. Feyerabend, "Problems of Empiricism...", p. 170; Feyerabend, "Introduction to Volumes 1 and...", p. xi; Feyerabend, "Explanation, Reduction...", p. 68; Feyerabend, "Theses on Anarchism...", p. 114; Feyerabend, **Against Method...** (1975), p. 188; Feyerabend, **Science in a Free...**, p. 80). So, before we start looking for causes of the phenomena in the world around us, we must first decide where we will look for these causes. On *the methodological level* (i.e. that which deals with the variability of scientific problems and criteria of evaluation), the incommensurability thesis states that when moving from one theory to another (or from one paradigm to another, or from one scientific research program to another), standards of scientificality and criteria for evaluating research results are radically altered (see e.g., Feyerabend, "Consolations for the Specialist...", pp. 163–164; Feyerabend, "The Methodology of Scientific..." p. 212, n. 18). The *level of observational variability* engenders different ways of seeing the world. According to this idea (i.e. that of observational variability), proponents of different, incommensurable theories will view the world differently (see e.g., Feyerabend, "Problems of Empiricism...", p. 214; Feyerabend, "Against Method..." (1970), pp. 85–87; Feyerabend, "Consolations for the Specialist...", pp. 155–156). However, if all empirical evidence is theorized, then there is no way to verify this evidence independently of theory. On *the linguistic level*, the incommensurability thesis boils down to the claim that when moving from one universal theory to another, certain terms change their meaning. In turn, this effectively makes it difficult to achieve accurate translations of the claims of alternative theories. Such theories do not use terms that share common meanings as the terms of each theory owe their meaning to the fundamental principles of the theory from which they derive (see Feyerabend, "Problems of Empiricism...", p. 227, n. 19; Feyerabend, "Explanation, Reduction...", p. 77–78).

[58] Those are not all the reasons of Feyerabend's shift to the anarchist camp. Another (and main, according to Feyerabend himself) reason was an encounter in 1965 with Carl Friedrich von Weizsäcker, thanks to him Feyerabend realised that poor, abstract, philosophical reasoning could not account for the changes occurring in science. (see e.g., Feyerabend's letter to Lakatos, dated 20 Jan. 1972, in: Lakatos and Feyerabend, **For and Against Method**..., s. 272; **Science in a Free**..., p. 117; **Against Method** 1993, p. 262; **Farewell to Reason**..., s. 316–317; Jung, „Paul K. Feyerabend...", p. 162–163). Another event was the International Colloquium on the Philosophy of Science held in London between 11 and 17 July 1965. The fourth volume of the colloquium proceedings included Feyerabend's text "Consolations for the Specialist", which proved to be an important step towards

> *Today* epistemology is sick and in need of a medicine. The medicine is anarchism. Anarchism, I say, will heal epistemology and *then* we may return to a more enlightened and more liberal form of rationality. [62]

Epistemological anarchism consists in a particular way of using methodological rules:

> Does […] [„anything goes"] mean[s] that there will not be a theory of knowledge? Not at all. There will be lots of rules of thumb with practical advice about their limits and

---

Against Method (see Feyerabend's letter to Lakatos, dated 17 Dec. 1967, in: Lakatos and Feyerabend, **For and Against Method**..., pp. 120 and 125). Still another event, beginning in 1964, was Feyerabend's contact with the mosaic of American cultures at the University of California, Berkeley, where he was teaching, , and the conclusion he came to after surveying these cultures more closely, that we should start learning from them (see e.g., Paul K. Feyerabend, "Letters to the Director of the Department of Philosophy", in: Lakatos and Feyerabend, **For and Against Method**..., pp. 385–386, 391–392 [382–393]; Feyerabend's letter to Lakatos, dated 18 Nov. 1968, in: Lakatos and Feyerabend, **For and Against Method**..., p. 153; Feyerabend's letter to Lakatos, dated 2 June 1969, in: Lakatos and Feyerabend, **For and Against Method**..., p. 167; Feyerabend, **Science in a Free**..., pp. 117–118, Feyerabend, **Against Method...** (1993), pp. 263–265; Feyerabend, **Farewell to Reason**..., pp. 317–318).

[59] Still, the first two editions (1970 and 1975) of Feyerabend's *Opus magnum* were subtitled "Outline of an Anarchistic Theory of Knowledge". Feyerabend later maintained that he proposed the term "anarchist theory of knowledge" deliberately, considering it a "Dadaist trick". "Theory" suggests order, while "anarchism" suggests disorder (see e.g., Paul K. Feyerabend, **The Tyranny of Science**, ed. by Eric Oberheim, Polity Press, Cambridge UK, Malden USA 2011, pp. 129–130; Feyerabend, "Concluding Unphilosophical...", pp. 488–489). It went largely unnoticed and the term was taken quite seriously (see e.g., Frederick Suppe, "Afterword — 1977", in: Suppe (ed.), **The Structure of Scientific Theories...**, p. 643 [617–730]; Philip Steedman, "Review of **Against Method: Outline an Anarchistic Theory of Knowledge**: **Science in a Free Society**", *Theory and Society* 1982, Vol. 11, No. 5, p. 724 [724–728]; Mark Wilson, "Review of **Against Method**", *The Philosophical Review* 1978, Vol. 87, No. 1, p. 106 [106–108]), and this became a source of much misunderstanding. Perhaps precisely because the term was taken too seriously, he dropped the subtitle "Outline of an Anarchistic Theory of Knowledge" in the second (1988) and third (1993) editions of **Against Method**. Feyerabend also admitted (see e.g., Feyerabend, **Killing Time...**, pp. 144–146) that he himself had contributed largely to the disarray that followed the publication of **Against Method**.

The 1975 edition can be considered a Dadaist trick, as the book was planned as part of a joint venture with Imre Lakatos and, as Feyerabend later recalled, it was "a letter written tongue in cheek" (Feyerabend, „Concluding Unphilosophical...", p. 489). In that edition he wrote explicitly that „it is a long and rather personal *letter* to Imre and every wicked phrase it contains was written in anticipation of an even more wicked reply from the recipient" (Feyerabend, **Against Method...** (1975), p. 9). Moreover, in the subject index to this essay, on p. 337, next to the term "rhetoric", Feyerabend even included a reference to the entire text of **Against Method**. However, it would be extremely difficult to assert that the article "Against Method..." is a Dadaist trick, as the text gives the impression of a serious treatise, as is evidenced both by the titles of the individual sections and by the manner

their judicious application inside the limits, but there will not be any general principles. [63]

This approach, contrary to appearances and Feyerabend's vague way of expressing himself, [64] does not centre around the universal principle of anything goes, [65] since the latter, according to Feyerabend's own views, is cognitively empty: [66]

There is no rule that is valid under all circumstances just as there is no measuring in-

---

of argumentation, in which the mechanisms of science are explained using Hegelian dialectics. For example: „How can this immobility [of science] be overcome? […] I would like to indicate, very briefly, how certain ideas of Hegel can be used to get a tentative first answer, and thus to make a first step in our attempt to reform the sciences" (Feyerabend, „Against Method…" (1970), pp. 31–32).

[60] Feyerabend, **Science in a Free…**, p. 186, n. 3 [italics in the original]. See also Feyerabend's letter to Lakatos, dated 26 Dec. 1970, in: Lakatos and Feyerabend, **For and Against Method…**, pp. 232–233.

[61] Feyerabend, **Science in a Free…**, p. 162, n. 26.

[62] Feyerabend, **Science in a Free…**, p. 127 [italics in the original].

[63] Paul K. Feyerabend, "Logic, Literacy, and Professor Gellner", *British Journal for the Philosophy of Science* 1976, Vol. 27, No. 4, p. 388 [381–391].

[64] For example, in his article "Against Method..." (1970) he wrote as follows: „To those who look at the rich material provided by history […] it will seem that there is only one principle that can be defended under all circumstances, and in all stages of human development. It is the principle: anything goes. This abstract principle […] is the one and only principle of our anarchistic methodology […]" (pp. 25–26). And in footnote 38 (p. 105) he explained that: „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". Both comments suggest that anything goes is not a fundamental principle of anarchist methodology.

However, in the same year, in another text he wrote something different: „Neither Galileo, nor Kepler, nor Newton use specific and well-defined methods. They are eclectics, methodological opportunists. […] [L]ooking at the actual historical situation we see that science was advanced in many different ways and that scientific problems were attacked by many different methods. In practice the only principle that is constantly adhered to seems to be *anything goes*" (Paul K. Feyerabend, "Experts in a Free Society" (1970), in: Feyerabend, **Philosophical Papers**. **Vol. 3**..., pp. 122–123 [112–126][italics in the original], see also Feyerabend, „Consolations for the Specialist…", p. 161).

In 1974, in "Thesen zum Anarchismus", he wrote in the same vein: "[Epistemological anarchist] […] will try to convince his audience that the only universal rule that can safely be in agreement with the moves the scientist must make to advance his subject is anything goes" (Feyerabend, "Theses on Anarchism…", p. 116). There is no indication here that anything goes is not a fundamental principle. Similarly, he wrote in **Against Method…** (1975): „To those who look at the rich material provided by history, and who are not intent on impoverishing it in order to please their lower instincts, their craving for intellectual security in the form of clarity, precision, »objectivity«, »truth« it will be-

> strument that measures everything and in all circumstances, but it is possible to construct such a rule in a *purely formal manner*: it is the rule "anything goes". [67]

This approach has the following components. „[I]t merely rejects the absolute validity of any rule in all historical epochs". [68] In the first book edition (1975), Feyerabend acknowledged that the term "anarchism" was misleading, implying a total absence of principles, [69] while his aim was not to eliminate all principles, [70] but to change attitudes towards them:

---

come clear that there is only *one* principle that can be defended under *all* circumstances and in *all* stages of human development. It is the principle: *anything goes*" (pp. 27–28 [italics in the original]).

In 1978, clearing up a number of misunderstandings about the principle of anything goes, he wrote: "»*anything goes*« *does not express any conviction of mine, it is jocular summary of the predicament of the rationalist:* if you want universal standards, I say, if you cannot live without principles that hold independently of situation, shape of world, exigencies of research, temperamental peculiarities, then I can give you such a principle. It will be empty, useless, and pretty ridiculous — but it will be a »principle«. It will be the »principle« »anything goes«" (Feyerabend, **Science in a Free Society...**, p. 188 [italics in the original], see also pp. 39–40).

In the "Preface" to the second edition of **Against Method...** (1988) he wrote similarly: "[...] Imre Lakatos loved to embarrass serious opponents with jokes and irony and so I, too, occasionally wrote in a rather ironical vein. An example is the end of Chapter 1: »anything goes« is not a »principle« I hold — I do not think that »principles« can be used and fruitfully discussed outside the concrete research situation they are supposed to affect but the terrified exclamation of a rationalist who takes a closer look at history" (p. vii). The same is repeated in the "Preface" to **Against Method...** (1993) on p. vii. However, this did not prevent him from repeating what he had written in the 1975 edition ("To those who look...", p. 19). He also repeated it in **Against Method...** (1988), p. 19.

[65] It is most likely that Feyerabend took the phrase "anything goes" from the title of a Cole Porter's musical entitled **Anything Goes**. In this musical, one of the title songs is "Anything Goes", which includes the following: "But now, God knows, Anything Goes". He may have watched the 1936 film version of the musical in a cinema near his home in Berkeley, for he liked to watch films from the 1930s (see Feyerabend, **Killing Time...**, p. 121; Russell, „Anything Goes...", pp. 452–453).

[66] See Feyerabend, **Science in a Free Society...**, p. 188; Oberheim, **Feyerabend's Philosophy**..., p. 33. For discussion see Jamie Shaw, "Was Feyerabend an Anarchist? The Structure(s) of »Anything Goes«", *Studies in History and Philosophy of Science Part A* 2017, Vol. 64, p. 12 [11–21].

[67] Feyerabend, "Logic, Literacy, and Professor Gellner...", p. 388 [italics added].

[68] Kazimierz Jodkowski, „Nauka w oczach Feyerabenda", in: Kazimierz Jodkowski (red.), **Czy sprzeczność może być racjonalna?**, *Realizm, Racjonalność, Relatywizm*, Vol. 4, Wydawnictwo Uniwersytetu Marii Curie-Skłodowskiej, Lublin 1986, pp. 251–252 [227–270].

[69] In this edition, he further explained why he preferred to use the term "Dadaism": "[A]narchism [...] has features I am not prepared to support. [...] I now prefer to use the term »dadaism«. [...] A Dadaist is utterly unimpressed by any serious enterprise [...]. A Dadaist is convinced that

> I argue that all rules have their limits and that there is no comprehensive "rationality", I do not argue that we should proceed without rules and standards. I also argue for a contextual account but again the contextual rules are not to replace the absolute rules, they are to *supplement* them. [71]

> For me the rules of context-dependent rationalism [i.e. contextual rules] are just as limited as the rules of old-fashioned rationalism [i.e. absolute rules]. [72]

In "Die »Rationalität« der Forschung" he explained it more comprehensively:

> I want to expand our inventory of rules — the more rules the better — and also propose a new use for all rules and criteria. My position is characterised by this *use*, and not by a particular rule *content*. [73]

---

a worthwhile life will arise only when we start taking things lightly and when we remove from our speech the profound but already putrid meanings it has accumulated over the centuries (»search for truth«; »defence of justice«; »passionate concern«; etc., etc.)" (Feyerabend, **Against Method...** (1975), p. 21, n.12, this footnote was removed by Feyerabend from the other editions of **Against Method**). "»Dada«, says Hans Richter in **Dada: Art and Anti-Art**, »not only had no programme, it was against all programmes«" (Feyerabend, **Against Method...** (1975), p. 33, n. 4). Why, then, in the five years that elapsed between the publication of the article and the book, did Feyerabend not change the term "anarchism" to "Dadaism"? One possible answer can be found in Richter's book cited by Feyerabend: "Our feeling of freedom from rules [...] [and] precepts [...] was a major stimulus. *The freedom not to care a damn about anything, the absence of any kind of opportunism*, which in any case could have served no purpose, brought us all the closer to the source of all art, the voice within ourselves" (Hans Richter, **Dada: Art and Anti-Art**, transl. by David Britt, Thames & Hudson Ltd, London 1965, p. 50 [italics added]). The *credo* formulated in this way makes the Dada programme more distant from Feyerabend's intention than the forms of anarchism he criticised.

Feyerabend also sparsely used the terms "epistemological anarchodadaism" (see Feyerabend, **Science in a Free...**, p. 163) and "dialectical rationalism" (see Feyerabend's letter to Lakatos, dated 30 July 1970, in: Lakatos and Feyerabend, **For and Against Method...**, pp. 207–208). With the latter term, he expressed a willingness to change the rules applied under the influence of circumstances.

[70] "I neither want to *replace* rules, nor do I want to show their worthlessness [...]" (Feyerabend, „Changing Patterns...", p. 368, n. 1 [italics in the original].

[71] Feyerabend, **Against Method...** (1988), p. 249 [italics added]; Feyerabend, **Against Method...** (1993), p. 231. See also Feyerabend, **Science in a Free...**, p. 164. In earlier editions, he did not express this thought as clearly. For example, he wrote: "there no longer exists a single set of rules that will guide us through all the, twists and turns of the history of thought (science)" (Feyerabend, "Against Method..." (1970), p. 78). "My intention is not to replace one set of general rules by another such set: my intention is, rather, to convince the reader that all methodologies, even the most obvious ones, have their limits" (Feyerabend, **Against Method...** (1975), p. 32). See also Feyerabend, **Science in a Free...**, p. 145.

[72] Feyerabend, **Science in a Free...**, p. 164.

> [W]e keep all the rules we know and all the criteria in a large conceptual toolbox and use them according to the requirements of the research. [74]

This does not mean, however, that Feyerabend claimed that there are no rules that adequately describe scientific practice. He therefore did not deny the claim that, in a particular situation, some method would be better than another:

> It is indubitable that the application of clear, well-defined, and above all "rational" rules *occasionally* leads to results. A vast number of discoveries owe their existence to the systematic procedures of their discoverers. [75]

> [I]n my case studies I not only try to show the *failure* of traditional methodologies, I also try to show what procedures *aided* the scientists *and should therefore be used. I criticize some procedures but I defend and recommend others*. [76]

He questioned the wisdom of any attempt to turn them into universally applicable rules:

> But from that, it does not follow that there are rules which must be obeyed for *every* cognitive act and *every* scientific investigation. On the contrary, it is totally improbable that there is such a system of rules, such a logic of scientific discovery, which permeates all reasoning without obstructing it in any way. [77]

> [T]here is not a single methodological rule that does not occasionally inhibit science and not a single "irrational" move that may not further it, given the right circumstances. [78]

He justified this thesis with the following cosmological assumption:

---

[73] Paul K. Feyerabend, "Die »Rationalität« der Forschung", in: Paul K. Feyerabend, **Ausgewählte Schriften. Band 1. Der wissenschaftstheoretische Realismus und die Autorität der Wissenschaften**, Friedr. Vieweg & Sohn Verlagsgesellschaft mbH, Braunschweig 1978, p. 345 [339–350]. See also Feyerabend, **Science in a Free...**, pp. 32–33, 164; Feyerabend, „Changing Patterns of Reconstruction...", p. 368, n. 1.

[74] Feyerabend, "Die »Rationalität« der Forschung...", p. 349.

[75] Paul K. Feyerabend, "On the Limited Validity of Methodological Rules" (1972), transl. by Eric Oberheim i Daniel Sirtes, in: Feyerabend, **Philosophical Papers. Vol. 3...**, p. 138 [138–180].

[76] Feyerabend, **Science in a Free...**, p. 188 [italics in the original]. See also pp. 15, 32, 164.

[77] Feyerabend, "On the Limited Validity...", p. 138.

[78] Feyerabend, "Theses on Anarchism...", p. 115. See also Feyerabend, **Science in a Free...**, p. 142.

> The world in which we live is very complex. Its laws do not lay open to us, rather they present themselves in diverse disguises (astronomy, atomic physics, theology, psychology, physiology, and the like). Countless prejudices find their way into every scientific action, making them possible in the first place. It is thus to be expected that every rule, even the most "fundamental", will only be successful in a limited domain, and that the forced application of the rule outside of its domain must obstruct research and perhaps even bring it to stagnation. [79]

His famous anything goes principle is also subjected to the same limitations [80] — it does not apply absolutely, regardless of the circumstances. However, the interpretation of this principle has been the source of many "never-ending misunderstanding[s]". [81]

However, in fact, it is no new fundamental principle of doing science: [82]

„Anything goes" is *not* the one and only „principle" of a new methodology, recom-

[79] Feyerabend, "On the Limited Validity…", p. 138. See also Feyerabend, "Theses on Anarchism…", p. 115.

[80] See Feyerabend, **Science in a Free…**, p. 31.

[81] Oberheim, **Feyerabend's Philosophy…**, p. 33. See also, e.g. Shaw, "Was Feyerabend an Anarchist…", pp. 17–18. Russell discussed the following list of the most common misunderstandings linked to this Feyerabend "principle": "Anything goes = science proceeds counter-inductively" (Russell, "Anything Goes…", p.443); "Anything goes = methodological pluralism" (Russell, "Anything Goes…", p. 444); "Anything goes = methodologies should guide, and be guided by practice" (Russell, "Anything Goes…", p. 445); "Anything goes = all methodological rules are useless" (Russell, "Anything Goes…", p. 447). On the misinterpretation of "anything goes" cf. e.g.: Michael Burawoy, "Critical Sociology: A Dialogue Between Two Sciences", *Contemporary Sociology* 1998, Vol. 27, No. 1, s. 13 [12–20]; H.M. Collins, Graham Cox, "Recovering Relativity: Did Prophecy Fail?", *Social Studies of Science* 1976, Vol. 6, No. 3/4, pp. 425–426 [423–444]; Noretta Koertge, "For and Against Method", *The British Journal for the Philosophy of Science* 1972, Vol. 23, No. 3, p. 280 [274–290]; Noretta Koertge, "Review of **Science in a Free Society**", *The British Journal for the Philosophy of Science* 1980, Vol. 31, No. 4, p. 388 [385–390]; Jean Curthoys and Wal Suchting, "Feyerabend's Discourse Against Method: A Marxist Critique", *Inquiry* 1977, Vol. 20, Nos. 2–3, p. 340, n. 7, p. 251 [243–397]; Gunnar Andersson, **Criticism and the History of Science: Kuhn's, Lakatos's, and Feyrabend's Criticism of Critical Rationalism**, E.J. Brill, Leiden, New York — Köln 1994, p. 5; Ian I. Mitroff, "Review of **Against Method: Outline of an Anarchistic Theory of Knowledge**", *Contemporary Sociology* 1976, Vol. 5, No. 3, p. 347 [346–347]; James E. McGuire, "Scientific Change: Perspectives and Proposals", in: Merrilee H. Salmon (ed.), **Introduction to the Philosophy of Science**, Hackett Publishing Company, Indianapolis 1999, pp. 159–160 [132–178].

[82] See Kazimierz Jodkowski, **Wspólnoty uczonych, paradygmaty i rewolucje naukowe**, *Realizm, Racjonalność, Relatywizm*, Vol. 22, Wydawnictwo UMCS, Lublin 1990, p. 108; Russell, "Anything Goes…", p. 440; Gonzalo Munévar, "Science in Feyerabend's Free Society", p. 180 [179–198].

mended by me. [83]

On the one side, anything goes is a statement of the fact that there are neither rules nor methods that are unconditionally valid at all stages of development of science: [84]

> [R]ules and standards are not abolished — one does not enter research without any Methodist equipment — but *are used tentatively and changed when the results are not as expected*. These changes do not prove that there are more general rules which decide when specific rules can be used and when they have to be suspended for individuals when behaving in an orderly manner, both constitute rules and follow them. [85]

Given the above statement by Feyerabend, his epistemological anarchism can be termed "methodological fallibilism". [86]

According to classical fallibilism, scientific knowledge is subject to permanent revision and the possibility that it may turn out to be false to a large extent cannot be ruled out. [87] As for scientific theories, their truth is not pronounced categorically, but only in the category of probability. What is exposed is not so much the falsity or non-availability of such knowledge as its essentially provisional character. And according to methodological fallibilism understood in this way, carefully developed methods used to acquire knowledge work in some cases, which does not mean that they will always be effective. It cannot be said of any methodology

[83] Feyerabend, **Science in a Free...**, p. 39 [italics in the original].

[84] See e.g., Feyerabend, "Against Method…" (1970), p. 21, p. 105, n. 38; Feyerabend, "On the Limited Validity…", p. 260; Feyerabend, **Against Method...** (1988), p. 249; Feyerabend, **Against Method...** (1993), p. 231; Feyerabend, "Die »Rationalität« der Forschung…", p. 343, fn. *; Parascandalo and Hösle, "Three Interviews with Paul…", p. 117; Jung, "Paul K. Feyerabend…", p. 162.

[85] Feyerabend, **Science in a Free...**, p. 166 [italics added].

[86] Feyerabend's anarchism had already been described by the term "sceptical fallibilism". However, with this term McEvoy was not emphasising Feyerabend's novel approach to methodological rules. He merely emphasised „the movement of his thought from the empiricism of critical rationalism which characterized his early work, to the relativism […] expressed in his later work" (McEvoy, "A »Revolutionary« Philosophy of Science…", p. 49–50).

[87] Such a view was fully embraced by Feyerabend, cf. e.g., Paul K. Feyerabend, "Knowledge Without Foundations", p. 76 [50–77]; Paul K. Feyerabend, "On the Improvement of the Sciences and the Arts and the Possible Identity of the Two", in: Robert S. Cohen, and Marx W. Wartofsky, **Proceedings of the Boston Colloquium for the Philosophy of Science, 1964/1966. In Memory of Norwood Russell Hanson**, *Boston Studies in the Philosophy of Science*, Vol. III, D. Reidel Publishing Company, Dordrecht 1967, p. 403 [387–415].

that it is successful, but only that it has been successful in some cases. It is not possible to distinguish a single set of methodological rules that will always contribute to the growth of knowledge. What is exposed here is not so much the inaccuracy of the methodological rules as their essentially provisional nature.

On the other hand, *given the above statement*, anything goes expresses four heuristic recommendations. „[O]bject to rules, standards, arguments which are general, and independent of the situation in which they are applied" [88] — as cases affirming the existence of such general rules, standards and arguments, in the light of the history of science, can hardly be considered more distinguished than those that are in disagreement with these rules, standards and arguments. [89] „[D]on't restrict your imagination"[90] — do not hastily dismiss bizarre viewpoints, as it is impossible to know in advance where researching them might lead. [91] „[D]o your own thing" [92] — do what you are convinced is the best means to solve the problem you are working on. [93] „[T]ry anything, see if it goes" [94] — literally any

[88] Feyerabend, "Logic, Literacy, and Professor Gellner…", p. 387.

[89] See e.g., Feyerabend, **Science in a Free…**, pp. 192, 212–213; Feyerabend, **Killing Time…**, p. 91.

[90] "So, »anything goes« means only »don't restrict your imagination« because a very silly idea can lead to a very solid result" (Feyerabend, **The Tyranny of Science…**, p. 130–131).

[91] The prohibition against uncritically rejecting absurd-sounding viewpoints can be found in Feyerabend, for example in the paper titled "Realism and Instrumentalism…" (p. 199): „we never know in advance which theory will be successful and which theory will fail. It takes a long time to decide this question, and every single step leading to such a decision is again open to revision. Nor can the absurdity of a point of view count as a general argument against it". See also Feyerabend, "Knowledge without Foundations…", p. 75.

[92] "»Anything goes« […] means that […] I am convinced that Mankind, and even Science, will profit from everyone doing his own thing: a physicist might prefer a sloppy and partly incomprehensible paper full of mistakes to a crystal-clear exposition because it is a natural extension of his own, still rather disorganized, research and he might achieve success as well as clarity long before his rival who has vowed never to read a single woolly line […]". Feyerabend, **Against Method…** (1975), p. 215; Feyerabend, **Against Method…** (1988), p. 165; Feyerabend, **Against Method…** (1993), p. 159.

[93] An outline of this way of thinking can be found in "How to be a Good Empiricist…" (p. 94) and in "Problems of Empiricism…" (p. 177) in the following statement: „After all, a man can do only so many things at a time, and it is better when he pursues a theory in which he is interested rather than a theory he finds boring".

[94] Marx W. Wartofsky, "How to Be a Good Realist", p. 28 [25–40]. I have used Wartofsky's formulation here as it clearly and briefly captures what Feyerabend himself said: "»[A]nything goes« […] means: anything goes, therefore also law and order, argument, irrationalism etc" (Feyerabend's let-

idea can be useful to increase knowledge, [95] as no interesting idea is ever completely suppressed, no matter how little evidence there is to support it. [96] Furthermore „there is no guarantee that the known forms of rationality will succeed and that the known forms of irrationality will fail. Any procedure, however ridiculous, may lead to progress, any procedure, however sound and rational, may get us stuck in the mud". [97]

This last recommendation also expresses Feyerabend's opportunism, allowing „whatever procedure seems to fit the occasion". [98] Scholars should be effective in achieving the stated purpose of their activity. They should use every opportunity to solve a problem, no matter how absurd or irrational that opportunity may seem at first glance.

However, these recommendations are not absolute prescriptions. Anything goes is therefore just another heuristic tool added to a large conceptual toolbox, which should be used skilfully (i.e. according to the specific situation). Literally any slogan, including anything goes, gets in the way of doing science when it is not adapted to a specific research situation. [99]

---

ter to Lakatos, dated March 1973 without a precise date, in: Lakatos and Feyerabend, **For and Against Method...**, p. 324, see also Feyerabend, **Science in a Free...**, pp. 127–128, 179, 189). "I regard every action and every piece of research both as a potential instance of the application of rules and as a test case: we may permit a rule to guide our research, or the kinds of actions we are interested in, we may permit it to exclude some actions, to mould others and on the whole to preside like a tyrant over our activities, but we may also permit our research and our activities to suspend the rule or to regard it as it as inapplicable even though all the known conditions demand its application. [...] No system of rules and standards is ever safe and the scientist who proceeds into the unknown may violate any such system, however »rational«. This is the polemical meaning of the phrase »anything goes«" (Feyerabend, **Science in a Free...**, p. 165).

[95] This idea appears in Feyerabend's "How to be..." (p. 100) and in "Problems of Empiricism..." (p. 182), when he referred to the following statement by Mach: "as a means of research, any idea is permissible which can help and really helps [...]" (Ernst Mach, **Die Principien der Wärmelehre. Historisch-kritisch entwickelt**, Johan Ambrosius Barth, Leipzig 1900, pp. 362–363).

[96] See Paul K. Feyerabend, "Al termine di una passeggiata non filosofica tra i boschi", in: Paul K. Feyerabend, **Dialoghi sulla conoscenza**, Gius. Laterza & Figli Spa, Roma-Bari 1991, p. 71 [61–114].

[97] Feyerabend, „Changing Patterns…", p. 368, n. 1 [italics in the original].

[98] Feyerabend, **Against Method...** (1988), p. 10.

[99] See Feyerabend, "Concluding Unphilosophical…", p. 503.

5. Summary

In this article, the basic components of Feyerabend's epistemological anarchism are discussed. Its first component is Weltanschauungen analyses. According to this approach, evaluations and methodological decisions, and the content of newly developed scientific claims, do not depend solely on facts and logic, observation and sound thinking, since scientific theories do not arise in an intellectual vacuum. Then the general assumptions of this anarchism are presented: the cosmological assumption (the world is so complex that it cannot be explained by a predetermined set of research rules); the methodological assumption (anarchism allows any idea of progress to be realised); the epistemological assumptions (not only is science an efficient way of acquiring knowledge and what is called science is not an orderly system of non-contradictory claims); the assumption of the way society and culture develops through competition and social exchange. These are followed by a presentation of Feyerabend's aims for epistemological anarchism. In the final section, the explanation is suggested why this anarchism is a recent move in the evolution of 20th century philosophy of science. I also explained what this specific approach to knowledge that frees science from the myth of a one-size-fits-all method consists of, naming it "methodological fallibilism".

Krzysztof J. Kilian

References

1. Adúriz-Bravo Agustín, "Methodology and Politics. A Proposal to Teach the Structuring Ideas of the Philosophy of Science through the Pendulum", *Science & Education* 2004, Vol. 13, pp. 717–731.
2. Amsterdamski Stefan, **Między historią a metodą**, Państwowy Instytut Wydawniczy, Warszawa 1983.
3. Andersson Gunnar, **Criticism and the History of Science: Kuhn's, Lakatos's, and Feyrabend's Criticism of Critical Rationalism**, E.J. Brill, Leiden — New York — Köln 1994.
4. Ben-Israel Isaac, "Philosophy and Methodology of Military Intelligence. Correspondence with Paul Feyerabend", *Philosophia* 2001, Vol. 28, No. 1–4, pp. 71–101.

5. Burawoy Michael, "Critical Sociology: A Dialogue Between Two Sciences", *Contemporary Sociology* 1998, Vol. 27, No. 1, pp. 12–20.

6. Collins H.M. and Cox Graham, "Recovering Relativity: Did Prophecy Fail?", *Social Studies of Science* 1976, Vol. 6, No. 3/4, pp. 423–444.

7. Curthoys Jean and Suchting Wal, "Feyerabend's Discourse Against Method: A Marxist Critique", *Inquiry* 1977, Vol. 20, No. 2–3, pp. 243–397.

8. Feigl Herbert, Feyerabend Paul K., Hanson Norwood R., Hempel Carl G., Hesse Mary, Maxwell Grover, and Rozeboom William, "Discussion at the Conference on Correspondence Rules", in: Michael Radner and Stephen Winokur (eds.), **Analyses of Theories and Methods of Physics and Psychology**, *Minnesota Studies in the Philosophy of Science*, Vol. 4, University of Minnesota Press, Minneapolis 1970, pp. 220–259.

9. Feyerabend Paul K., "Against Method. Outline of an Anarchistic Theory of Knowledge", in: Michael Radner and Stephen Winokur (eds.), **Analyses 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.

10. Feyerabend Paul K., **Against Method. Outline of an Anarchistic Theory of Knowledge**, New Left Books, London 1975.

11. Feyerabend Paul K., **Against Method. Revised Edition**, Verso, London — New York 1988.

12. Feyerabend Paul K., **Against Method. Third Edition**, Verso, London 1993.

13. Feyerabend Paul K., "Al termine di una passeggiata non filosofica tra i boschi", in: Paul K. Feyerabend, **Dialoghi sulla conoscenza**, Gius. Laterza & Figli Spa, Roma-Bari 1991, pp. 61–114.

14. Feyerabend Paul K., "An Attempt at a Realistic Interpretation of Experience" (1958), in: Paul K. Feyerabend, **Philosophical Papers. Vol. 1. Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 17–36.

15. Feyerabend Paul K., "Changing Patterns of Reconstruction", *British Journal for the Philosophy of Science* 1977, Vol. 28, No. 4, pp. 351–369.

16. Feyerabend Paul K., "Concluding Unphilosophical Conversation", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul K. Feyerabend**, *Boston Studies in the Philosophy of Science*, Vol. 132, Kluwer Academic Publishers, Dordrecht — Boston — London 1991, pp. 487–528.

17. Feyerabend Paul K., "Consolations for the Specialist" (1970), in: Paul K. Feyerabend, **Philosophical Papers. Vol. 2. Problems of Empiricism**, Cambridge University

Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 131–167.

18. Feyerabend Paul K., "Die »Rationalität« der Forschung", in: Paul K. Feyerabend, **Ausgewählte Schriften. Band 1. Der wissenschaftstheoretische Realismus und die Autorität der Wissenschaften**, Friedr. Vieweg & Sohn Verlagsgesellschaft mbH, Braunschweig 1978, pp. 339–350.

19. Feyerabend Paul K., "Experts in a Free Society" (1970), in: Feyerabend Paul K., **Philosophical Papers**. **Vol. 3. Knowledge, Science and Relativism**, John Preston (ed.), Cambridge University Press, Cambridge — New York — Melbourne — Madrid — Cape Town — Singapore — São Paulo 2008, pp. 112–126.

20. Feyerabend Paul K., "Explanation, Reduction and Empiricism", in: Paul K. Feyerabend, **Philosophical Papers. Vol. 1. Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 44–96.

21. Feyerabend Paul K., **Farewell to Reason**, Verso, London, New York 1996.

22. Feyerabend Paul K., "How to be a Good Empiricist: A Plea for Tolerance in Matters Epistemological" (1963), in: Paul K. Feyerabend, **Philosophical Papers**. **Vol. 3. Knowledge, Science and Relativism**, John Preston (ed.), Cambridge University Press, Cambridge — New York — Melbourne — Madrid — Cape Town — Singapore — São Paulo 2008, pp. 78–103.

23. Feyerabend Paul K., "Introduction to Volumes 1 and 2", in: Paul K. Feyerabend, **Philosophical Papers. Vol. 1. Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. IX–XIV.

24. Feyerabend Paul K., **Killing Time**, University of Chicago Press, Chicago — London 1995.

25. Feyerabend Paul K., "Knowledge Without Foundations", in: Paul K. Feyerabend, **Philosophical Papers**. **Vol. 3. Knowledge, Science and Relativism**, John Preston (ed.), Cambridge University Press, Cambridge — New York — Melbourne — Madrid — Cape Town — Singapore — São Paulo 2008, pp. 50–77.

26. Feyerabend Paul K., "Letters to the Director of the Department of Philosophy", in: Imre Lakatos and Paul K. Feyerabend, **For and Against Method: Including Lakatos' Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence**, ed. Matteo Motterlini, The University of Chicago Press, Chicago — London, 1999, pp. 382–393.

27. Paul K. Feyerabend, "Logic, Literacy, and Professor Gellner", *British Journal for the Philosophy of Science* 1976, Vol. 27, No. 4, pp. 381–391.

28. Paul K. Feyerabend, "The Methodology of Scientific Research Programmes", in: Feyerabend Paul K., **Philosophical Papers. Vol. 2. Problems of Empiricism**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 202–230.

29. Paul K. Feyerabend, "More Clothes from the Emperor's Bargain Basement. A Review of Laudan's. Progress and its Problems" (1981), in: Feyerabend Paul K., **Philosophical Papers. Vol. 1. Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 231–246.

30. Paul K. Feyerabend, "On the Critique of Scientific Reason", in: Cohen Robert S., Feyerabend Paul K., and Wartofsky Marx W. (eds.), **Essays in Memory of Imre Lakatos**, *Boston Studies in the Philosophy of Science*, Vol. 39, D. Reidel Publishing Company, Dordrecht, Holland 1976, pp. 109–143.

31. Feyerabend Paul K., "On the Improvement of the Sciences and the Arts and the Possible Identity of the Two", in: Robert S. Cohen and Marx W. Wartofsky (eds.), **Proceedings of the Boston Colloquium for the Philosophy of Science, 1964/1966. In Memory of Norwood Russell Hanson**, *Boston Studies in the Philosophy of Science*, Vol. III, D. Reidel Publishing Company, Dordrecht 1967, pp. 387–415.

32. Feyerabend Paul K., "On the Limited Validity of Methodological Rules" (1972), transl. by Eric Oberheim i Daniel Sirtes, in: Paul K. Feyerabend, **Philosophical Papers**. **Vol. 3. Knowledge, Science and Relativism**, John Preston (ed.), Cambridge University Press, Cambridge — New York — Melbourne — Madrid — Cape Town— Singapore — São Paulo 2008, pp. 138–180.

33. Feyerabend Paul K., "On the »Meaning« of Scientific Terms" (1964), in: Paul K. Feyerabend, **Philosophical Papers. Vol. 1. Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 97–103.

34. Feyerabend Paul K., "Physics and Ontology" (1954), in: Paul K. Feyerabend, **Philosophical Papers**. **Vol. 4. Physics and Philosophy**, Stefano Gattei and Joseph Agassi (eds.), Cambridge University Press, New York 2016, pp. 9–24.

35. Feyerabend Paul K., "Preface to the Second Edition", in: Paul K. Feyerabend, **Farewell to Reason**, Verso, London, New York 1996, pp. V–VIII.

36. Feyerabend Paul K., "The Problem of the Existence of Theoretical Entities" (1960), in: Paul K. Feyerabend, **Philosophical Papers**. **Vol. 3. Knowledge, Science and Relativism**, John Preston (ed.), Cambridge University Press, Cambridge — New York — Melbourne — Madrid — Cape Town— Singapore — São Paulo 2008, pp. 16–49.

37. Feyerabend Paul K., "Professor Bohm's Philosophy of Nature" (1960), in: Paul K. Feyerabend, **Philosophical Papers. Vol. 1. Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 219–235.

38. Feyerabend Paul K., "Problems of Empiricism", in: Robert G. Colodny (ed.), **Beyond the Edge of Certainty: Essays in Contemporary Science and Philosophy**, Prentice-Hall, Englewood Cliffs, New Jersey 1965, pp. 145–260.

39. Feyerabend Paul K., "Rationalism, Relativism and Scientific Method" (1977), in: Paul K. Feyerabend, **Philosophical Papers**. **Vol. 3. Knowledge, Science and Relativism**, John Preston (ed.), Cambridge University Press, Cambridge — New York — Melbourne — Madrid — Cape Town — Singapore — São Paulo 2008, pp. 200–211.

40. Feyerabend Paul K., "Realism and Instrumentalism: Comments on the Logic of Factual Support", in: Paul K. Feyerabend, **Philosophical Papers. Vol. 1. Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 176–202.

41. Feyerabend Paul K., "Reply to Criticism. Comments on Smart, Sellars and Putnam", in: Paul K. Feyerabend, **Philosophical Papers. Vol. 1. Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 104–131.

42. Feyerabend Paul K., **Science in a Free Society**, Verso, London 1983.

43. Paul K. Feyerabend, "Theses on Anarchism" (1973), in: Imre Lakatos and Paul K. Feyerabend (ed.), **For and Against Method. Including Lakatos' Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence**, ed. Matteo Motterlini, The University of Chicago Press, Chicago — London, 1999, pp. 113–118.

44. Feyerabend Paul K., "Two Letters of Paul Feyerabend to Thomas Kuhn on a Draft to **The Structure of Scientific Revolutions**", Paul Hoyningen-Huene (ed.), *Studies in History and Philosophy of Science* 1995, Vol. 26, No. 3, pp. 353–387.

45. Feyerabend Paul K., **The Tyranny of Science**, Eric Oberheim (ed.), Polity Press, Cambridge — Malden 2011.

46. Fuchs Stephan, "Metatheory and the Sociology of Sociology", *Sociological Perspectives* 1992, Vol. 35, No. 3, pp. 531–535.

47. Gale George, Walter Edward , "Kordig and the Theory-Ladenness of Observation", *Philosophy of Science* 1973, Vol. 40, No. 3, pp. 415–432.

48. Godfrey-Smith Peter, **Theory and Reality: An Introduction to the Philosophy of Science**, University of Chicago Press, Chicago — London 2003.

49. Jodkowski Kazimierz, „Filozofia nauki Paula K. Feyerabenda. Stadium umiarkowane", *Studia Filozoficzne* 1979, nr 11, s. 59–75.

50. Jodkowski Kazimierz, „Nauka w oczach Feyerabenda", w: Kazimierz Jodkowski (red.), **Czy sprzeczność może być racjonalna?**, *Realizm, Racjonalność, Relatywizm*, Vol. 4, Wydawnictwo UMCS, Lublin 1986, s. 227–270.

51. Jodkowski Kazimierz, „Polskie wydanie rozpraw Feyerabenda", *Studia Filozoficzne* 1981, t. 2 nr 183, s. 159–169.

52. Jodkowski Kazimierz, **Teza o niewspółmierności w ujęciu Thomasa S. Kuhna i Paula K. Feyerabenda**, *Realizm. Racjonalność. Relatywizm*, Vol. 1, Wydawnictwo UMCS, Lublin 1984, https://tiny.pl/tlk98 [28.01.2024].

53. Jodkowski Kazimierz, **Wspólnoty uczonych, paradygmaty i rewolucje naukowe**, *Realizm, Racjonalność, Relatywizm*, Vol. 22, Wydawnictwo UMCS, Lublin 1990.

54. Jodkowski Kazimierz, "»Wszystko ujdzie«. Anarchizm epistemologiczny Paula K. Feyerabenda", *Akcent* 1982, t. 2, nr 8, pp. 127–134.

55. Jung Joachim, "Paul K. Feyerabend. Last Interview", 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 — Oxford 2000, pp. 159–168.

56. Koertge Noretta, "For and Against Method", *The British Journal for the Philosophy of Science* 1972, Vol. 23, No. 3, pp. 274–290.

57. Koertge Noretta, "Review of **Science in a Free Society**", *The British Journal for the Philosophy of Science* 1980, Vol. 31, No. 4, pp. 385–390.

58. Kuhn Thomas S., "Commensurability, Comparability, Comunicability", in: Thomas S. Kuhn (ed.), **The Road Since Structure. Philosophical Essays, 1970–1993, with an Autobiographical Interview**, James Conant and John Haugeland (ed.), University of Chicago Press, Chicago 2000, pp. 33–57.

59. Mach Ernst, **Die Principien der Wärmelehre. Historisch-kritisch entwickelt**, Johan Ambrosius Barth, Leipzig 1900.

60. McEvoy John, "A »Revolutionary« Philosophy of Science: Feyerabend and the Degeneration of Critical Rationalism into Sceptical Fallibilism", *Philosophy of Science* 1975, Vol. 42, No. 1, pp. 49–66.

61. McGuire James E., "Scientific Change: Perspectives and Proposals", in: Merrilee H. Salmon (ed.), **Introduction to the Philosophy of Science**, Hackett Publishing Company, Indianapolis 1999, pp. 132–178.

62. McMullin Ernan, "Review of Science, Revolution, and Discontinuity", *Isis* 1983, Vol. 74, No. 4, pp. 577–579.

63. Mitroff Ian I., "Review of **Against Method: Outline of an Anarchistic Theory of Knowledge**", *Contemporary Sociology* 1976, Vol. 5, No. 3, pp. 346–347.

64. Munévar Gonzalo, "Science in Feyerabend's Free Society", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul K. Feyerabend**, *Boston Studies in the Philosophy of Science*, Vol. 132, Kluwer Academic Publishers, Dordrecht — Boston — London 1991, pp. 179–198.

65. Nickles Thomas, "Historicist Theories of Scientific Rationality", in: Edward N. Zalta (ed.), **The Stanford Encyclopedia of Philosophy**, Spring 2021 Edition, https://tiny.pl/czp6f [27.01.2024].

66. Oberheim Eric, **Feyerabend's Philosophy**, *Quellen und Studien zur Philosophie*, Bd. 73, Walter de Gruyter, Berlin — New York 2006.

67. Parascandalo Renato and Hösle Vittorio, "Three Interviews with Paul K. Feyerabend", *Teleos. A Quarterly Journal of Critical Thought* 1995, No. 102, pp. 115–148.

68. Poincaré Henri, **Science and Hypothesis**, The Walter Scott Publishing Co, New York 1905.

69. Poincaré Henri, **Science and Method**, Thomas Nelson and Sons, London — Edinburgh — Dublin —New York 1914.

70. Popper Karl R., **The Logic of Scientific Discovery**, Routledge Classics, London — New York 2002.

71. Preston John, **Feyerabend: Philosophy, Science and Society**, Polity Press, Cambridge — Malden 1997.

72. Richter Hans, **Dada: Art and Anti-Art**, transl. David Britt, Thames & Hudson Ltd, London 1965.

73. Russell Denise, „Anything Goes", *Social Studies of Science* 1983, Vol. 13, No. 3, 437–464.

74. Shaw Jamie, "Was Feyerabend an Anarchist? The Structure(s) of »Anything Goes«", *Studies in History and Philosophy of Science Part A* 2017, Vol. 64, pp. 11–21.

75. Steedman Philip, "Review of **Against Method: Outline an Anarchistic Theory of Knowledge**: **Science in a Free Society**", *Theory and Society* 1982, Vol. 11, No. 5, pp. 724–728.

76. Suppe Frederick, "Afterword – 1977", in: Suppe Frederick (ed.), **The Structure of Scientific Theories**, University of Illinois Press, Urbana — Chicago — London 1977, pp. 617–730.

77. Suppe Frederick, “Search for Philosophic Understanding of Scientific Theories”, in: Frederick Suppe (ed.), **The Structure of Scientific Theories**, University of Illinois Press, Urbana — Chicago — London 1977, pp. 1–241.

78. Wartofsky Marx W., “How to Be a Good Realist”, in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul K. Feyerabend**, *Boston Studies in the Philosophy of Science*, Vol. 132, Kluwer Academic Publishers, Dordrecht — Boston — London 1991, pp. 24–40.

79. Wilson Mark, “Review of **Against Method**”, *The Philosophical Review* 1978, Vol. 87, No. 1, pp. 106–108.

Late Philosophy

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 237–258

https://doi.org/10.53763/fag.2023.20.2.232

ARTYKUŁ ORYGINALNY / ORIGINAL ARTICLE

Ian James Kidd

University of Nottingham

Feyerabend on Pluralism, Contingency, and Humility [1]

Received: November 30, 2023. Accepted: February 12, 2024. Published online: May 10, 2024.

Abstract: Throughout the writings of Paul Feyerabend, there are constant references to the historical contingency of the scientific enterprise, often accompanied by philosophical claims about the significance of that contingency. This paper presents those contingentist claims, situates them in the context of more recent work on the contingency of science, and offers an interpretation of their significance. I suggest that Feyerabend's sense of contingency was connected to his defences of pluralism, and also to the "conquest of abundance" narrative developed in the very late writings.

Keywords:

contingency; Feyerabend; historiography; pluralism

1. Introduction

An emphasis on the historical and social contingency of the scientific enterprise is a neglected theme in the work of Paul Feyerabend. The emergence and development of the intellectual and practical structures of the sciences, he argued, depended on events, trends, and developments which could have gone differently, or not occurred at all. Appeals to contingency recur, consistently if implicitly, through the classic papers of the late 1960s, in the books of the 1970s and '80s, continuing to his last writings. An important theme of **Conquest of Abundance**, for instance, is the role of "idiosyncratic historical developments" in shaping the

[1] I am grateful for helpful comments on an earlier draft of this paper by Karim Bschir, Paul Hoyningen-Huene, John Preston and an anonymous referee. These led to several improvements.

scientific worldview and its associated "form of life". [2] In the 1970s, Feyerabend began work on an ambitious history of Western "philosophies of nature", whose aim was "to understand the contingencies that helped the [scientific] endeavour to succeed". [3]

Interpreting Feyerabend's remarks on contingency is difficult. There are many of them, scattered across many different writings, and their purpose is often unclear. The appeals to contingency, indeed, were put to plausible and provocative purposes. In the 1960s publications, a sense of contingency is a concomitant of the serious study of history of science. From the mid-1970s, however, themes of contingency become tied to Feyerabend's experiments in "relativism" and provocative counter-cultural polemics. [4] The theme of contingency persisted into the 1980s. **Farewell to Reason** describes "powerful traditions" promoting "uniformity" competing with more pluralistic tendencies. The contingencies of science now include new and unanticipated theories, "interdisciplinary developments [...] grand unifying schemes" and a blurring of once-important distinctions. [5] The sociology of science is cited in support of the massive, ineliminable role of material and social contingency. Into the 1990s, a longstanding interest in what Feyerabend called the "rise of rationalism" modulated into the titular theme of what we know as **Conquest of Abundance**.

In this paper I want to organise these remarks on the contingency of science and offer an account of their importance to Feyerabend's developing ideas. I suggest that contingency can be intimately related to pluralism; that we can describe a shift from contingencies in science to the deep contingency of scientific enquiry; and that we can interpret the "conquest of abundance" narrative as the culmination of Feyerabend's sensitivity to contingency.

---

[2] Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago 2001, p. 144.

[3] Paul K. Feyerabend, **Philosophy of Nature**, in: Eric Oberheim and Helmut Heit (eds.), Polity, Cambridge 2016, p. 205.

[4] Martin Kusch, "Relativism in Feyerabend's Later Writings", *Studies in History and Philosophy of Science Part A* 2016, Vol. 57, p. 110ff [106–113].

[5] See Paul K. Feyerabend, **Farewell to Reason**, Verso, London 1987, pp. 1–3ff.

2. Contingency

The contingency of science became a topic in philosophy of science in the last thirty years. Its early forms involved a dialectic between claims that the results of successful scientific enquiry are inevitable or contingent. [6] Earlier forms of philosophy of science, however, were sensitive to contingency claims and their epistemic significance. Two important, if neglected examples are feminist epistemology of science and what came to be called postcolonial science and technology studies. Each explores the role of specific events and developments on our scientific inheritance. Colonial histories, gendered traditions of enquiry, technoscientific imperatives and much else helped shape the direction and ethos of what came to be called science. Feyerabend did not engage in any serious way with feminist approaches to science, but showed more interest in certain postcolonial accounts, if in unsystematic ways. **Farewell to Reason**, for instance, opens by announcing an interest in cultural diversity and its erosion by the imperialist-epistemic projects of Global North cultures. Contingentist accounts of science are also found in the later work of Edmund Husserl, whose **Crisis of European Sciences** — an account of the tendencies to abstraction characteristic of "post-Galilean" science — are praised by Feyerabend. [7]

The modern contingency debate moved past concerns with the polarised inevitabilist and contingentist stances. The main questions, currently, concern (i) the object of contingency claims and (ii) the significance, epistemic or otherwise, of their being contingent. [8] Without good answers to this pair of questions, many contingency claims will be bland — no–one, after all, denies *any* role to social and historical developments that might not have occurred at all. [9] On the first ques-

[6] See Ian Hacking, "How Inevitable are the Results of Successful Science?", *Philosophy of Science* 2000, Vol. 67, pp. 58–71.

[7] See Edmund Husserl, **The Crisis of European Sciences and Transcendental Phenomenology**, Northwestern University Press, Evanston 1970.

[8] See Lená Soler, "Are the Results of our Science Contingent or Inevitable?", *Studies in History and Philosophy of Science* 2008, Vol. 39, pp. 221–229, https://doi.org/10.1016/j.shpsa.2015.05.013; Lená Soler, Emiliano Trizio and Andrew Pickering, **Science as It Could Have Been: Discussing the Contingency/Inevitability Problem**, The University of Pittsburgh Press, Pittsburgh 2015.

[9] See Katerina Kinzel, "State of the Field: Are the Results of Science Contingent or Inevitable?", *Studies in History and Philosophy of Science* 2015, Vol. 52, pp. 55–66, https://doi.org/10.1016/j.shpsa.2015.05.013.

tion, many different aspects of science can be contingent. Some examples include, *inter alia*, concepts, values, methodological norms, aims of enquiry, institutional and disciplinary arrangements, and theoretical and metaphysical commitments. Such objects can be more or less contingent, and their actual degree of contingency can change over time. In the broader sorts of claims, what is contingent is very abstract and collective — "the scientific worldview", "scientific culture", or "the European sciences". In narrower claims, contingency can best be attributed to objects with definitional specificity, such as a particular theory. [10]

On the question of why contingency claims matter, there are different opinions. Most discussions focus on the epistemological and historiographical implications. Early sociologists of science used contingency to probe the epistemic significance of social structures of enquiry. [11] Others have tried hard to identify possible alternatives to the scientific theories we did inherit. [12] Others appeal to contingency claims to motivate arguments for pluralism — as a emans, for instance, to try and recover sciences that, due to historical contingencies, never came to be. [13] Some philosophers of science also debate the implications of contingency for scientific realism. [14]

The discussion about contingency claims in relation to historiography are of-

[10] See Joseph D. Martin, "Is the Contingentist/Inevitabilist Debate a Matter of Degrees?", *Philosophy of Science* 2013, Vol. 80, No. 5, pp. 919–930, https://doi.org/10.1086/674003; Lená Soler, "Revealing the Analytical Structure and some Intrinsic Major Difficulties of the Contingentist/Inevitabilist Issue", *Studies in History and Philosophy of Science* 2008, Vol. 39, No. 2, pp. 230–241, https://doi.org/10.1016/j.shpsa.2008.03.015.

[11] See Andrew Pickering, **Constructing Quarks: A Sociological History of Particle Physics**, University of Chicago Press, Chicago 1984; James T. Cushing, **Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony**, University of Chicago Press, Chicago 1994.

[12] See Jeroen Bouterse, "Contingentism for Historians", *Studies in History and Philosophy of Science* 2022, Vol. 96, pp. 27–34, https://doi.org/10.1016/j.shpsa.2022.08.001; Veli Virmajoki, "Could Science be Interestingly Different?", *Journal of the Philosophy of History* 2018, Vol. 12, No. 2, pp. 303–324, https://doi.org/10.1163/18722636-12341388.

[13] See Hasok Chang, **Is Water H_2O? Evidence, Realism, Pluralism**, Springer, Dordrecht 2012, ch. 5.

[14] See Howard Sankey, "Scientific Realism and the Inevitability of Science", *Studies in History and Philosophy of Science* 2008, Vol. 39, No. 2, pp. 259–264, https://doi.org/10.1016/j.shpsa.2008.03.018; Luca Tambolo and Gustavo Cevolani, "Multiple Discoveries, Inevitability, and Scientific Realism", *Studies in History and Philosophy of Science* 2021, Vol. 90, pp. 30–38, https://doi.org/10.1016/j.shpsa.2021.09.001.

ten explored using counterfactual history. Gregory Radick has argued, at some length, for a contingentist stance on the history of biology. There was, he argues, nothing inevitable about the emergence of the genic biology central to modern biological thinking. Had history gone another way, our biology may have become non-genic. [15] Other well-developed cause studies include oxygen and phlogistonic chemistry [16] and Darwinian forms of biology. [17] Whatever one thinks of counterfactual scientific developments, one should recognise the historiographical lessons. Philosophers should not assume that successful sciences can only develop in a single way. [18] Triumphalist insistences about only the "true theory" winning out should be rejected, as self-legitimating conceits. If the history of science could have developed in quite different ways, then this is a fact worth taking seriously in historical practice.

This is a brief *resumé* of the contingency debates in the history and philosophy of science. Several potential points of contact with Feyerabend's work should already be obvious. Contingency resonates with his defences and celebrations of proliferation and pluralism. Contingency, as a feature of science, is a lesson of serious attention to its history and practice. Contingency also encourages the sorts of epistemic virtues celebrated by Feyerabend, such as imaginativeness, humility, and open-mindedness. This set of themes suggests lots of work to do. Surprisingly, though, there is little discussion of Feyerabend in current contingency scholarship. The few mentions are usually too brief to be really informative. [19] An

[15] See Gregory Radick, "Other Histories, other Biologies", in: Anthony O'Hear (ed.), **Philosophy, Biology, and Life**, Cambridge University Press, Cambridge — New York — Melbourne 2005, pp. 21–47; Gregory Radick, **Disputed Inheritance: The Battle over Mendel and the Future of Biology**, University of Chicago Press, Chicago 2023.

[16] See Hasok Chang, "We Have Never Been Whiggish (About Phlogiston)", *Centaurus* 2009, Vol. 51, No. 4, pp. 239–264, https://doi.org/10.1111/j.1600-0498.2009.00150.x.

[17] See Peter Bowler, **Darwin Deleted: Imagining a World without Darwin**, University of Chicago Press, Chicago 2012.

[18] See Steve Clarke and Adrian Walsh, "Imperialism, Progress, Developmental Teleology, and Interdisciplinary Unification", *International Studies in the Philosophy of Science* 2014, Vol. 27, No. 3, pp. 341–351, https://doi.org/10.1080/02698595.2013.825493; Ian James Kidd, "Historical Contingency and the Impact of Scientific Imperialism", *International Studies in the Philosophy of Science* 2013, Vol. 27, No. 3, pp. 317–326, https://doi.org/10.1080/02698595.2013.825494.

[19] See Ian Hacking, **The Social Construction of What?**, Harvard University Press, Cambridge 1999, pp. 98–99; Hans-Jörg Rheinberger, **On Historicising Epistemology: An Essay**, *Cultural Memory*

honourable exception is the work of Hasok Chang, whose own brand of "active normative epistemic pluralism" enjoys contingentist and Feyerabendian inspirations. [20]

The failure to appreciate the philosophical interest of Feyerabend's remarks on contingency is, however, partly his own fault. As everyone knows, he eschewed kinds of "systematic" treatments that offended his temperament but do make life easier for his readers. [21] Moreover, some of Feyerabend's remarks are too vague or too polemical. **Science in a Free Society**, a very hyperbolic book, announces that "science as we know it is not inescapable", since "we can construct a world in which it plays no role whatever". [22] But none of these claims are explained, so readers cannot distinguish between banal and extreme interpretations. Similarly, the references to science depending on "historical processes", while interesting, are too vague for one to draw any clear unambiguous conclusions. [23] As is often the case, Feyerabend's remarks need interpreting carefully — and augmented, as and where necessary.

In what follows I offer a tentative interpretation of the remarks on contingency in Feyerabend's work. The main themes are *pluralism* and *abundance*. I suggest that there is an enduring, if implicit conviction that appreciation of pluralism is sustained by a sense of contingency. Being a pluralist requires a sense of how different our inheritance could have been — something Feyerabend dramatizes as a "conquest of abundance".

3. Pluralism and proliferation

The best-known theme of Feyerabend's work are his defences of pluralism

in the Present, trans. David Fernbach, Stanford University Press, Stanford 2010, pp. 63–64.

[20] See Chang, **Is Water H_2O?...**; Hasok Chang, "Cultivating Contingency: A Case for Scientific Pluralism", in: Lená Soler, Emiliano Trizio and Andrew Pickering (eds.), **Science as it Could Have Been: Discussing the Contingency/ Inevitability Problem**, Pittsburgh University Press, Pittsburgh 2015, pp. 359–382.

[21] See Eric Oberheim, **Feyerabend's Philosophy**, Walter de Gruyter, Berlin 2006, ch.1.

[22] Paul K. Feyerabend, **Science in a Free Society**, New Left Books, London 1978, p. 228.

[23] See e.g., Paul K. Feyerabend, **Against Method. Third Edition**, Verso, London 1993, p. 215; Feyerabend, **Conquest of Abundance...**, p. 15.

about science, and his related exemplification of a pluralist style of philosophy. There are different arguments for scientific pluralism, historical and epistemological, which evolved over time. There are also different conceptions of what it means for science to be pluralistic. The earlier writings, famously, focused on *methodological pluralism* — or, as Feyerabend came to call it, "epistemological Dadaism". In one formulation, a study of the history of science reveals no single set of defined, formalised methodological rules: no Scientific Method. The physical, earth, life, and human sciences all employ a range of different investigative methods, which makes sense given their different objects of investigation and motivating aims and interests. Any references to *the* scientific method, therefore, falsifies the realities of what one finds in the laboratory or in the field. The real target of Feyerabend's **Against Method** — or the several works sharing that title — is therefore *methodological monism*. The positive thesis — both descriptive and normative — is methodological pluralism, perhaps most clearly stated in the Preface to the Chinese edition of **Against Method**:

> The thesis is: *the events, procedures and results that constitute the sciences have no common structure*; there are no elements that occur in every scientific investigation but are missing elsewhere. Concrete developments (such as the overthrow of steady state cosmologies and the discovery of the structure of DNA) have distinct features and we can often explain why and how these features led to success. But not every discovery can be accounted for in the same manner, and procedures that paid off in the past may create havoc when imposed on the future. Successful research does not obey general standards; it relies on one trick, now on another; the moves that advance it, and the standards that define what counts as advance are not always known to the movers. [24]

Feyerabend emphasises both *methodological plurality* (as a descriptive, historical fact about the sciences) and *methodological pluralism* (as a normative ideal). Method is one possible dimension of pluralism; one can also add, as Feyerabend did, pluralism about *theories* and *values*. An "anarchistic" science, while over-dramatically named, is characterised by a plurality of aims, values, methods, theories and styles of reasoning — an ideal the best serves our epistemic interests and, for Feyerabend, enhances the "humanitarian" possibilities of scientific enquiry.

[24] Feyerabend, **Against Method...**, p. 1.

My question in this section concerns the connections of epistemic pluralism to the theme of contingency. How could a sense of contingency contribute to the case for an normative epistemic pluralism? To answer that question I will draw on Eric Oberheim's analysis of Feyerabend as a philosophical pluralist. [25] Oberheim is careful to emphasise the changing forms of pluralism one can find across Feyerabend's writings; however there is, he argues, a fairly stable general structure to those forms. Oberheim calls this the *pluralistic test-model*, construed, not as "a cohesive, stable set of doctrines" or "principles", but as "a pluralistic philosophical method". [26] Some of its main components are the related concepts of *criticism*, *alternatives*, and *proliferation*. The general line thought, sufficient for my purposes, are that (a) criticism plays a variety of essential epistemic roles — strengthening theories; maximising empirical content; exposing contestable assumptions; (b) criticism is often most effective when using *alternatives* to the theories in question — since, for instance, a set of theories that share a questionable assumption by that fact tend to conceal it; (c) such alternatives, while sometimes naturally existing, often have to be deliberately generated, by the relaxation of constraints and/or acts and policies of *proliferation*. In Feyerabend's writings one finds variations of this line of thought, inspired — in an appropriately eclectic manner — by John Stuart Mill, Karl R. Popper and the philosopher-physicists beloved by Feyerabend. [27] An ideal scientific enterprise is therefore deeply pluralistic, a rich "ocean of alternatives", of robust and well-developed methods, theories, and projects. At certain points some of this naturally-occurring diversity might diminish, and the proper response — for Feyerabend but not for Kuhn — was to encourage proliferation. Alternatives must be carefully and deliberately created and supported, through historical identification of earlier underdeveloped ideas, supporting epistemic experimentation or innovation, and the eclectic appeals to disciplines and traditions outside the sciences. [28]

---

[25] See Oberheim, **Feyerabend's Philosophy…**, pp. 286–283.

[26] Oberheim, **Feyerabend's philosophy…**, p. 287.

[27] See Elizabeth A. Lloyd, "Feyerabend, Mill, and Pluralism", *Philosophy of Science* 1997, Vol. 64, *Supplement. Proceedings of the 1996 Biennial Meetings of the Philosophy of Science Association. Part II: Symposia Papers*, pp. S396–S407.; Gonzalo Munévar, "Historical Antecedents to the Philosophy of Paul Feyerabend", *Studies in History and Philosophy of Science* 2013, Vol. 57, No. 3, pp. 9–16, https://doi.org/10.1016/j.shpsa.2015.11.002.

[28] See Feyerabend, **Against Method…**, ch. 2–4.

This is a very generalised account of Feyerabend's style of argument for epistemic pluralism, which requires a well-developed social epistemology of enquiry. It does, however, help us connect pluralism to contingency. I will mention two connections, the first being that alternatives, as Feyerabend well-knew, do not always emerge, due to diverse kinds of contingent events and developments. To take some examples: social, material, historical, cultural and religious contingencies can shape the *kinds* of theory favoured in a community of enquiry; or establish certain developmental trajectories to the exclusion of others; or encourage cross-fertilisation with some disciplines to the exclusion of others; and so on. These kinds of influence are better described in a vocabulary of "encouraging", not one of "determining". Some social constructionists spoke as if the content and course of scientific enquiry is utterly unconstrained by objective facts. Most contingentists are more sensible. For a Feyerabendian pluralist, the point is that certain alternatives fail to emerge — or fail to develop — due to contingent events. Crucially, we should not infer from these failures the demerits of those alternatives. The failure can be explained in other ways — for instance, that alternative emerged too late with rival theories already in place, or the person and resources needed to develop that epistemic possibility into a compelling alternative did not appear or were obstructed. [29]

The second connection of contingency to pluralism concerns the possibility of proliferation. Many things are needed to imagine, generate and sustain a diversity of robustly well-developed alternative theories, methods, and projects. These can include personnel, technologies, funding and material investment, institutions, specialist equipment, rare or expensive materials, supportive social and political conditions and epistemically supporting intrapersonal and inter-community relationships. What Feyerabend appreciated in the history and sociology of science was, among other things, the articulation of this complexity. However, social and material complexities are always products and causes of contingencies. Funds, equipment, artefacts, social institutions and other aspects of scientific enquiry are massively structured by socio-historical contingencies. Our scientific inheritance, while extremely rich, is a product of both deliberate decisions, at local

[29] Think of the impact of David Bohm's exile from the United States on the subsequent fortunes of Bohmian mechanics. Matthew J. Brown, "The Abundant World: Paul Feyerabend's Metaphysics of Science", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 142–154, https://doi.org/10.1016/j.shpsa.2015.11.015, § 3.4.4.

and more global levels, but also of contingencies playing out at different levels and different timescales. In Feyerabend's famous Galileo case study, there are many references to such contingencies — the sorts of "natural interpretations" of natural phenomena typical of the time; the empirical and theoretical content of Copernicanism; a particular set of "paradigmatic cases" for theorising about motion; the rhetorical and "propagandistic" tricks used by Galileo; new epistemological ideas about experiment and proof; new mathematical philosophies; reactions to these astronomic claims shaped by then-prevailing theological views — and so on. [30]

In a summary of some of his main epistemological and historical points, Feyerabend remarks:

> We see here very clearly how misguided it is to try reducing the process "Copernican Revolution" to a single principle, such as the principle of falsification. Falsifications played a role just as new observations played a role. But both were imbedded in a complex pattern of events which contained tendencies, attitudes, and considerations of an entirely different nature. [31]

Scientific enquiry proceeds within an inherited array of social, material, historical and cultural contingencies — a "pattern of events" which could unfold in different ways, whose "tendencies, attitudes, and considerations" could have been quite different. Decisions and actions do play a role, for sure, but as responses to an epistemic context whose structure, content, and possibilities are inherited, and not self-consciously created. How does this relate to the possibility of proliferation?

For Feyerabend, our scientific inheritance incorporates certain epistemic possibilities but not others. Some kinds of scientific theory, say, come to be serendipitously well-supported by the resources, opportunities, and structures already in place. Others are not, which does not preclude their becoming well-established, even if it does mean the process of their development requires far more work. As a case in point, consider Feyerabend's criticism of what he calls *the consistency condition*, which "demands that new hypotheses agree with accepted theories", and which he criticises as "reasonable because it preserves the older the-

[30] See Feyerabend, **Against Method...**, pp. 6–10.

[31] Feyerabend, **Against Method...**, p. 145.

ory, and not [necessarily] the better theory". [32] The consistency condition is problematic for several reasons; for instance, it entrenched problematic kinds of conceptual conservatism and finds little support from the history of science. [33] However, the consistency condition also fails to account for the contingencies of the history of science. Consistency with the established can only be defended *if* that establishment was a result of careful procedures of careful deliberation and decision. But this is not the case. Many contingent factors cooperated to help establish the scientific inheritance with which consistency is being demanded. The consistency condition works to conceals this contingency by pretending that what was established was established for procedural reasons.

The upshot is that certain kinds of proliferation can be obstructed because the epistemic possibilities latent in our scientific inheritance are products of contingencies. In many cases, efforts to proliferate are obstructed by the lack of necessary resources, political obstruction, vested interests, dogmas, and other social-epistemic factors. The deeper obstacle, however, is the constrained range of epistemic possibilities that we, contingently, came to inherit. Had our history gone in other ways, other kinds of proliferation could have become possible. To put the point another way, contingencies shape the actualities of the sciences and also their developmental possibilities. In Gregory Radick's felicitous contingentist slogan, "Other histories, other biologies". [34]

Contingency should be understood as a source of at least two problems for the ideal of normative pluralism in the sciences. Our scientific inheritance is a product of a complicated multigenerational history shaped in many ways at different levels by social, material, historical and intellectual contingencies. Such contingencies will affect (a) the kinds of theories that emerged and the alternatives that did or did not emerge and (b) the possibility of kinds of proliferation available to us. Feyerabend was sensitive to these contingencies, I think, even if they were implicit, background worries. The consistency condition, for one, should be interpreted as a failure to appreciate the messy contingency of our scientific inheritance.

[32] Feyerabend, **Against Method...**, p. 24.

[33] See Preston Jonh, **Feyerabend: Philosophy, Science and Society**, Polity Press, Oxford 1996, p. 83ff; Oberheim, **Feyerabend's Philosophy**, p. 91.

[34] Gregory Radick, "Counterfactuals and the Historian of Science", *Isis* 2008, Vol. 99, pp. 547–551.

In the next section I turn to more explicit discussions of contingency in Feyerabend's writings, which focus on the implications of contingency for our attitudes towards our scientific achievements.

4. Competition and confidence

The debate about contingency in science includes reflections on two questions: how should contingency affect scientific practice, and, relatedly, how should it affect our epistemological relationship to our actual scientific theories? The assumption is that reflection on contingency is not idle. It can change scientific practice and also change our epistemic relationship to scientific knowledge. One can, for instance, use contingency-based arguments to argue for scientific antirealism, or persuade us to sympathetically revisit seemingly "dead" theories, or even to "foster a profound change of spirit regarding science". [35] The hope is that reflection on contingency offers us real practical implications for how we perform, organise, and understand scientific enquiry.

Feyerabend certainly thought contingency could yield epistemic and practical changes in the sciences. I want to reconstruct one argument for this claim. The general idea is that a cultivated sense of contingency fosters a kind of epistemic humility concerning our scientific inheritance. To start the argument, consider the claim — which no-one would deny — that the emergence and entrenchment of the sciences owed to a complex series of contingencies. Feyerabend refers to the sciences' "historical grounding", and the ways the modern scientific enterprise depends on a complex "historical process [it] did not initiate". [36] This grounding consists of events, processes, and "idiosyncratic historical developments", that change over time as they become more or less entrenched, rather like Wittgenstein's "river-bed propositions". [37] The shape and direction of the sciences, as Feyerabend often says, depends on experiments and reasoning, but also on "an entire arsenal

[35] Cooper, **The Measure of Things...**, p. 202ff; Chang, "We Have Never Been Whiggish..."; Soler, Trizio and Pickering, **Science as It Could Have Been...**, p. 42.

[36] Feyerabend, **Against Method...**, p. 214; Feyerabend, **Conquest of Abundance...**, p.15.

[37] Feyerabend, **Conquest of Abundance...**, p. 144; Ian James Kidd, "Reawakening to Wonder: Feyerabend, Science, and Scientism", in: Karim Bschir and Jamie Shaw (eds.), Interpreting Feyerabend, Cambridge University Press, Cambridge 2021, pp. 172–190.

of reasons, facts, prejudices [and] social pressures". [38] Such contingencies play out at different levels—the lives of individual scientists; their interpersonal and social circumstances; the epistemic and cultural dialectics of enquiry. In the writings of the 1960s and 1970s, Feyerabend's point was that development of the sciences is "more complex, more conjectural" than was recognised by then-popular philosophical models of science. [39] But these points were later used in quite different ways.

The next stage of the argument involves making two comments on these points about dependence on various contingent events and developments. First: they might not have happened. Certain events, decisive for science as we know it, might not have happened. History could have gone other ways. Certain developments might not have developed. Second: the establishment of what was received by us, as our scientific inheritance, was not the outcome of long careful processes of deliberation and decision, informed by critical competition between well-developed rivals. [40] As Feyerabend put this point, the triumph of certain theories "depended on historical conditions", of a contingent kind, and not "a critical study of alternatives". [41] The entrenchment of one research programme, far from being an inevitable convergence on truth, may be explained by a community becoming "swept along by overwhelming historical forces". [42] Feyerabend need not deny *any* role for successful rational competition: some theories or research programmes *do* prove their empirical, predictive or explanatory superiority over their rivals. But this may be the *exception* rather than the *rule*. Feyerabend's worry was that, too often, "ideas are rejected before they can show their strength", since entrenched theories have contingent advantages — they emerged first and accrued, *inter alia*, attention, credibility and resources — that make it far easier for them to "assemble successes". [43] In some writings, Feyerabend gives examples, including

[38] Feyerabend, **Philosophy of Nature...**, p. 168.

[39] See Feyerabend, **Against Method...**, p. 136.

[40] See Cooper, **The Measure of Things...**, p. 193f; Ian James Kidd, "Inevitability, Contingency, and Epistemic Humility", *Studies in History and Philosophy of Science* 2016, Vol. 55, pp. 12–19, https://doi.org/10.1016/j.shpsa.2015.08.006.

[41] Feyerabend, **Against Method...**, p. 216.

[42] Feyerabend, **Conquest of Abundance...**, p. 101.

[43] Paul K. Feyerabend, "Introduction: Proliferation and Realism as Methodological Principles", in: Paul K. Feyerabend, **Philosophical Papers, Vol. 1, Realism, Rationalism & Scientific Method**, Cam-

the gatekeeping methods used by Big Bang cosmologists to lock out research supporting steady-state cosmologies. [44] More recent work in sociology and social epistemology of science could offer more detailed cases. Established theories can find it easier to "win out", to the point that, in some cases, no fair critical competition can occur. To be fair, Feyerabend sometimes put this point too strongly. *Science in a Free Society* — a notoriously polemical book — declares that, with science, "the show has been rigged in its favour". [45] But that is too strong.

The more moderate claim is this: one can only warrant confidently claims about the superior or privileged status of a scientific theory *if* that status has been established by sustained critical competition with robust alternatives. As Oberheim emphasises, this point is vital to the pluralistic test-model. Any "final assertion" of the superior status of a theory must, says Feyerabend, come *after* it has been "confronted with alternatives". [46] In the absence of such confrontations, assertions cannot be made about its superiority, since that is a comparative notion. Assertion of superiority without the proof of any successful in critical competition has "success — but it is the success of a maneuverer carried out in a void". [47] Confident assertions would require the advocates of a theory to have "gone through all possible trials", and, if certain trials are not possible, then to remain silent. [48] Some trials cannot be performed because the rivals in question never emerged. A critic may reply that, even if a trial cannot be practically performed, it could still be abstractly performed, in a kind of simulation. Feyerabend anticipated this response and rejected it. Competition and trials are practical endeavours. Simulation cannot capture their complexities — a point nicely described by Emiliano Trizio. The "hopelessly collective and highly specialized character of enquiry pre-

bridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, p. 139.

[44] See Feyerabend, **Conquest of Abundance...**, pp. 149, 151.

[45] Feyerabend, **Science in a Free Society...**, p. 102.

[46] Oberheim, **Feyerabend's Philosophy...**, p. 243.

[47] Feyerabend, **Against Method...**, p. 30.

[48] See Paul K.Feyerabend, "Concluding Unphilosophical Conversation", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul K. Feyerabend**, *Boston Studies in the Philosophy of Science*, Vol. 132, Kluwer Academic Publishers, Dordrecht — Boston — London 1991, pp. 516 [487-527].

vents, in practice, any private reconstruction of the entire edifice of knowledge". [49] Moreover, a simulated trial is vulnerable to self-vindication. As Chang argues, it is all-too-easy to assume that one's own theories will prevail in critical competition with rivals — especially if one *knows* one will never actually have to engage in the competition. [50] Too often, adds Chang, presupposing the inevitable success of one's own theories against their rivals is an "unreflective triumphalism that celebrates the winning side in an episode, whichever it may happen to be". [51] Or as John Preston puts the point in a discussion of Feyerabend, the relevant competition "has never been staged, and we cannot even anticipate its outcome". [52]

5. Humility and "Abundance"

How does this argument foster a kind of humility? In my reconstruction, Feyerabend argued (a) our scientific inheritance is significantly the product of historical and social contingencies; (b) a variety of actual or possible alternatives to our inheritance never emerged or emerged too late to become established; (c) the relative superiority of our actual inheritance over these alternatives could be confidently asserted only after long critical competitions; (d) these competitions did not take place and cannot be simulated, predicted, anticipated or otherwise confidently determined; therefore (e) we cannot confidently issue the 'final assertion' of the superiority of what we inherited in relation to possible alternative inheritances. The outcome should not be radical epistemic anxiety, the abandonment of our inheritance and all our trust in it. That would be an overreaction. Nor is the correct response a retreat into the extreme constructionism voiced by Richard Rorty. [53]

---

[49] Emiliano Trizio, "How Many Sciences for one World? Contingency and the Success of Science", *Studies in History and Philosophy of Science* 2008, Vol. 39, No. 2, pp. 253–258, https://doi.org/10.1016/j.shpsa.2008.03.017, p. 258; Kidd, "Inevitability, Contingency…", § 3.

[50] Hasok Chang, "The Hidden History of Phlogiston: How Philosophical Failure can Generate Historiographical Refinement", Hyle 2010, Vol. 16, No. 2, pp. 47–79.

[51] Chang, "We Have Never Been…", p. 240.

[52] Preston, **Feyerabend: Philosophy…**, p. 205.

[53] Richard Rorty, **Contingency, Irony, and Solidarity**, Cambrdige University Press, Cambridge 1989, p.16.

The correct response is more like the one described by Trizio. Confronted with a sense of contingency, a mature and reflective scientist should:

> accept the existing science, without being able to rule out the possibility that it would have been different if the decisions of our predecessors had been different. And there is no way to prove that our predecessors had no choice, but to do what they did. [54]

Feyerabend was sometimes unusually moderate when he put the point. The "Interlude" in **Conquest of Abundance**, for instance, reminds us that, not matter how successful or "obvious" our inherited systems of thought seem to us, we must appreciate that "this is not the only possible approach, that there are alternatives, and that they may lead to different conclusions". [55] We can continue to employ and esteem our scientific inheritance, or as much of it as proves epistemically and practically valuable. But we should avoid triumphalism, abandon premature "final assertions", and recognise our scientific inheritance as an impressive if contingent edifice. Retrospectively we are well-advised to actively search history for undeveloped and underdeveloped alternatives. Chang has defended an ideal like this in the form of *complementary science*. [56] Prospectively we must be receptive to possible alternatives and try to create conditions which actively foster epistemic plurality. If we appreciate the contingency of our inheritance, then we can come to see its *particularity*. Our inheritance enables certain kinds of scientific projects while also disabling others. This could encourage certain kinds of epistemic attitudes, many that invite description using a vocabulary of humility. We can appreciate our successes without assuming that no other kinds of success could be possible. We can trust and take pride in our scientific achievements without prematurely issuing "final assertions". We can regard the prospect of alternative epistemic possibilities with curiosity or even wonder, rather than dismiss them out of hand. [57]

---

[54] Trizio, "How Many Sciences for one World?...", p. 258.

[55] Feyerabend, **Conquest of Abundance...**, p. 87.

[56] See Chang, **Is Water H_2O?...**, p. §5.3.4.

[57] See Alex Aylward, "Against Defaultism and Towards Localism in the Contingency/Inevitability Conversation: Or, why we Should shut up about Putting-up", *Studies in History and Philosophy of Science* 2019, Vol. 74, pp. 30–41, https://doi.org/10.1016/j.shpsa.2019.01.008; Luca Tambolo, "An Unappreciated Merit of Counterfactual Histories of Science", *Studies in History and Philosophy of Biological and Biomedical Sciences* 2020, Vol. 81, A:101183, https://doi.org/10.1016/j.shpsc.2019.101183.

I have only indicated some general modes of humility in connection to contingency and science. More detailed accounts are available elsewhere.[58] These modes of humility are all either indicated by Feyerabend's remarks or consistent with the ethos of his philosophical pluralism. Contingency fosters an appreciation of alternatives — our inheritance is partial rather than comprehensive. Contingency fosters criticism by encouraging us to be perpetually alert for problems or objections that were not part of our inheritance. Some of the most important objections to our theories may remain currently unrealised. Contingency also makes us think in new ways about proliferation. On the one hand, the deep contingencies of our inheritance constrain our ability to proliferate. Certain epistemic possibilities were not part of our inheritance and it may be difficult, or impossible, to retrieve them. On the other hand, proliferating *now* to reduce the future effects of contingency seems a prudent strategy. Contingency also energises our appreciation of alternatives — a sense of contingency, at its broadest, will include a sense of possible alternatives to our scientific inheritance.

Feyerabend would endorse all these points. A sense of contingency can reinforce his arguments for pluralism, and also resonate with his hostility to dogmatism and his celebration of creativity, imaginativeness, and open-mindedness. A sense of the contingency and particularity of one's own received ways of thinking should sustain an emancipatory sense of there being *other* ways. The other ways could be found in in many places — other scientific communities, earlier traditions, or other cultures. All this fits the expansive historical and cultural vision expressed in **Against Method**, **Farewell to Reason**, and **Conquest of Abundance**. Contingency and pluralism can even be seen as mutually reinforcing themes. Feyerabend's later ideas about the "conquest of abundance" are complex and need further study. One can, however, interpret them as a call to humility. The world is "abundant" because it inspires, sustains and rewards a diversity of perspectives, theories, and worldviews.[59] Such abundance can be understood as a feature of the world — its ontological complexity. But it also reflects the marvellous richness, variety and changeability of human interests, sensibilities and

[58] See David E. Cooper, **The Measure of Things...**; Kidd, "Inevitability, Contingency…"; Ian James Kidd, "Humility, Contingency, and Pluralism in the Sciences", in: Mark Alfano, Michael Lynch, and Alessandra Tanesini (eds.), **The Routledge Handbook on the Philosophy of Humility**, Routledge, New York 2020, pp. 346–358.

[59] See Feyerabend, **Conquest of Abundance...**, p. 3; Feyerabend, **Farewell to Reason...**, p. 179.

forms of life. This is one way to interpret the cryptic remark — in one of the essays — that "abundance occurs in history, not in the world". [60] Or better: what we see across the history of human cultures are wonderfully rich experiments with a plurality of ways of experiencing and engaging with the world. Such "abundance" is impressive — which is why Feyerabend lamented its "conquering" by processes of epistemic and cultural homogenisation.

I see contingency as a rich theme in Feyerabend, one that resonates with many of his ideas and concerns. This includes his advocacy of pluralism, criticisms of closed-mindedness and dogmatism, celebrations of "abundance" and, at a more abstract level, the ethos of humility that is such an attractive feature of his work. In her Preface to **Conquest of Abundance**, Grazia Borrini-Feyerabend notes that, compared to earlier works, it has "a quieter, more wondering attitude". [61] One of the things that invites our wonder is the "abundance" manifested in the history of human life. Another object of wonder, though, are the unrealised alternatives — the other histories, other sciences and other forms of life whose possibilities show the wonderful richness available when human beings live in ways that are creative, humble, tolerant, and humane. I see this same sensibility in Hasok Chang's own defences of a pluralistic science, whose debts to Feyerabend's own work he openly acknowledges:

> The most fundamental motivation for pluralism is *humility*: we are limited beings trying to understand and engage with an external reality that seems vastly complex, apparently inexhaustible, and ultimately unpredictable […]
>
> [M]ature scientists and mature scientific communities would value tolerance, humility and circumspection, combined with a tough questioning attitude. They would display an awareness of humility fragility and fallibility and the multifarious complexity of nature, and try to create institutional structures that can handle this awareness. [62]

Feyerabend would applaud this richly humane conception of science and human life, and, if my remarks in this paper are correct, then the cultivation of a sense of contingency will help us to achieve it.

---

[60] Feyerabend, **Conquest of Abundance…**, p. 134.

[61] Feyerabend, **Conquest of Abundance…**, p. xi.

[62] Chang, **Is Water H_2O?…**, p. 255, 238.

Acknowledgements

I am grateful to the editors for their generous invitation to contribute to this collection and for their editorial comments.

Ian James Kidd

References

1. Aylward Alex, "Against Defaultism and Towards Localism in the Contingency/Inevitability Conversation: Or, why we Should shut up about Putting-up", *Studies in History and Philosophy of Science* 2019, Vol. 74, pp. 30–41, https://doi.org/10.1016/j.shpsa.2019.01.008.
2. Bouterse Jeroen, "Contingentism for Historians", *Studies in History and Philosophy of Science* 2022, Vol. 96, pp. 27–34, https://doi.org/10.1016/j.shpsa.2022.08.001.
3. Bowler Peter, **Darwin Deleted: Imagining a World without Darwin**, University of Chicago Press, Chicago 2012.
4. Brown Matthew J., "The Abundant World: Paul Feyerabend's Metaphysics of Science", *Studies in History and Philosophy of Science* 2016, Vol. 57, pp. 142–154, https://doi.org/10.1016/j.shpsa.2015.11.015.
5. Chang Hasok, "We Have Never Been Whiggish (About Phlogiston)", *Centaurus* 2009, Vol. 51, No. 4, pp. 239–264, https://doi.org/10.1111/j.1600-0498.2009.00150.x.
6. Chang Hasok, "The Hidden History of Phlogiston: How Philosophical Failure can Generate Historiographical Refinement", *Hyle* 2010, Vol. 16, No. 2, pp. 47–79.
7. Chang Hasok, **Is Water H_2O? Evidence, Realism, Pluralism**, Springer, Dordrecht 2012.
8. Chang Hasok, "Cultivating Contingency: A Case for Scientific Pluralism", in: Lená Soler, Emiliano Trizio and Andrew Pickering (eds.), **Science as it Could Have Been: Discussing the Contingency/Inevitability Problem**, Pittsburgh University Press, Pittsburgh 2015, pp. 359–382.
9. Clarke Steve and Walsh Adrian, "Imperialism, Progress, Developmental Teleology, and Interdisciplinary Unification", *International Studies in the Philosophy of Science* 2014, Vol. 27, No. 3, pp. 341–351, https://doi.org/10.1080/02698595.2013.825493.
10. Cooper David E., **The Measure of Things: Humanism, Humility, and Mystery**, Clarendon Press, Oxford 2002.

11. Cushing James T., **Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony**, University of Chicago Press, Chicago 1994.

12. Farrell Robert, **Feyerabend and Scientific Values: Tightrope-Walking Rationality**, Kluwer, Dordrecht 2003.

13. Feyerabend Paul K., **Against Method. Third Edition**, Verso, London 1993.

14. Feyerabend Paul K., "Concluding Unphilosophical Conversation", in: Gonzalo Munévar (ed.), **Beyond Reason: Essays on the Philosophy of Paul K. Feyerabend**, *Boston Studies in the Philosophy of Science*, Vol. 132, Kluwer Academic Publishers, Dordrecht — Boston — London 1991, pp. 433–448.

15. Feyerabend Paul K., **Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago 2001.

16. Feyerabend Paul K., **Farewell to Reason**, Verso, London 1987.

17. Feyerabend Paul K., "Introduction: Proliferation and Realism as Methodological Principles", in: Paul K. Feyerabend, **Philosophical Papers, Vol. 1, Realism, Rationalism & Scientific Method**, Cambridge University Press, Cambridge — New York — Port Chester — Melbourne — Sydney 1981, pp. 139–145.

18. Feyerabend Paul K., **Philosophy of Nature**, in: Eric Oberheim and Helmut Heit (eds.), Polity, Cambridge 2016.

19. Feyerabend Paul K., **Science in a Free Society**, New Left Books, London 1978.

20. Hacking Ian, **The Social Construction of What?**, Harvard University Press, Cambridge 1999.

21. Hacking Ian, "How Inevitable are the Results of Successful Science?", *Philosophy of Science* 2000, Vol. 67, pp. 58–71.

22. Husserl Edmund, **The Crisis of European Sciences and Transcendental Phenomenology**, Northwestern University Press, Evanston 1970.

23. Kidd Ian James, "Historical Contingency and the Impact of Scientific Imperialism", *International Studies in the Philosophy of Science* 2013, Vol. 27, No. 3, pp. 317–326, https://doi.org/10.1080/02698595.2013.825494.

24. Kidd Ian James, "Inevitability, Contingency, and Epistemic Humility", *Studies in History and Philosophy of Science* 2016, Vol. 55, pp. 12–19, https://doi.org/10.1016/j.shpsa.2015.08.006.

25. Kidd Ian James, "Humility, Contingency, and Pluralism in the Sciences", in: Mark Alfano, Michael Lynch, and Alessandra Tanesini (eds.), **The Routledge Handbook on the Philosophy of Humility**, Routledge, New York 2020, pp. 346–358.

26. Kidd Ian James, "Reawakening to Wonder: Feyerabend, Science, and Scientism", in: Karim Bschir and Jamie Shaw (eds.), **Interpreting Feyerabend**, Cambridge University Press, Cambridge 2021, pp. 172–190.

27. Kinzel Katerina, "State of the Field: Are the Results of Science Contingent or Inevitable?", *Studies in History and Philosophy of Science* 2015, Vol. 52, pp. 55–66, https://doi.org/10.1016/j.shpsa.2015.05.013.

28. Kusch Martin, "Relativism in Feyerabend's Later Writings", *Studies in History and Philosophy of Science Part A* 2016, Vol. 57, pp. 106–113.

29. Lloyd Elizabeth A., "Feyerabend, Mill, and Pluralism", *Philosophy of Science* 1997, Vol. 64/4, "Feyerabend, Mill, and Pluralism", *Philosophy of Science* 1997, Vol. 64, *Supplement. Proceedings of the 1996 Biennial Meetings of the Philosophy of Science Association. Part II: Symposia Papers,* pp. S396–S407.

30. Martin Joseph D., "Is the Contingentist/Inevitabilist Debate a Matter of Degrees?", *Philosophy of Science* 2013, Vol. 80, No. 5, pp. 919–930, https://doi.org/10.1086/674003.

31. Munévar Gonzalo, "Historical Antecedents to the Philosophy of Paul Feyerabend", *Studies in History and Philosophy of Science* 2013, Vol. 57, No. 3, pp. 9–16, https://doi.org/10.1016/j.shpsa.2015.11.002.

32. Oberheim Eric, **Feyerabend's Philosophy**, Walter de Gruyter, Berlin 2006.

33. Pickering Andrew, **Constructing Quarks: A Sociological History of Particle Physics**, University of Chicago Press, Chicago 1984.

34. Preston Jonh, **Feyerabend: Philosophy, Science and Society**, Polity Press, Oxford 1996.

35. Radick Gregory, "Other Histories, other Biologies", in: Anthony O'Hear (ed.), **Philosophy, Biology, and Life**, Cambridge University Press, Cambridge — New York — Melbourne 2005, pp. 21–47.

36. Radick Gregory, "Counterfactuals and the Historian of Science", *Isis* 2008, Vol. 99, pp. 547–551.

37. Radick Gregory, **Disputed Inheritance: The Battle over Mendel and the Future of Biology**, University of Chicago Press, Chicago 2023.

38. Rheinberger Hans-Jörg, **On Historicising Epistemology: An Essay**, *Cultural Memory in the Present*, trans. David Fernbach, Stanford University Press, Stanford 2010.

39. Rorty Richard, **Contingency, Irony, and Solidarity**, Cambrdige University Press, Cambridge 1989.

40. Sankey Howard, "Scientific Realism and the Inevitability of Science", *Studies in History and Philosophy of Science* 2008, Vol. 39, No. 2, pp. 259–264, https://doi.org/10.1016/j.shpsa.2008.03.018.

41. Soler Lená, "Are the Results of our Science Contingent or Inevitable?", *Studies in History and Philosophy of Science* 2008, Vol. 39, pp. 221–229, https://doi.org/10.1016/j.shpsa.2015.05.013.

42. Soler Lená, "Revealing the Analytical Structure and some Intrinsic Major Difficulties of the Contingentist/Inevitabilist Issue", *Studies in History and Philosophy of Science* 2008, Vol. 39, No. 2, pp. 230–241, https://doi.org/10.1016/j.shpsa.2008.03.015.

43. Soler Lená, Trizio Emiliano and Pickering Andrew, **Science as It Could Have Been: Discussing the Contingency/Inevitability Problem**, The University of Pittsburgh Press, Pittsburgh 2015.

44. Tambolo Luca, "An Unappreciated Merit of Counterfactual Histories of Science", *Studies in History and Philosophy of Biological and Biomedical Sciences* 2020, Vol. 81, A:101183, https://doi.org/10.1016/j.shpsc.2019.101183.

45. Tambolo Luca and Cevolani Gustavo, "Multiple Discoveries, Inevitability, and Scientific Realism", *Studies in History and Philosophy of Science* 2021, Vol. 90, pp. 30–38, https://doi.org/10.1016/j.shpsa.2021.09.001.

46. Trizio Emiliano, "How Many Sciences for one World? Contingency and the Success of Science", *Studies in History and Philosophy of Science* 2008, Vol. 39, No. 2, pp. 253–258, https://doi.org/10.1016/j.shpsa.2008.03.017.

47. Virmajoki Veli, "Could Science be Interestingly Different?", *Journal of the Philosophy of History* 2018, Vol. 12, No. 2, pp. 303–324, https://doi.org/10.1163/18722636-12341388.

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 259– 263

https://doi.org/10.53763/fag.2023.20.2.222

LIST DO REDAKCJI / LETTER TO THE EDITOR

Grazia Borrini-Feyerabend

President of the Paul K. Feyerabend Foundation

We Can Choose to Live in a World that Makes Sense to Us

Received: December 4, 2023. Accepted: December 12, 2023. Published online: February 2, 2024.

Twenty-five years ago, when I worked with Bert Terpstra to compile the posthumous **Conquest of Abundance**, I was in doubt about whether Paul would have liked to see the book in print. [1] I knew it was very dear to him, surely a "labor of love". He had kept working on it for years, reading an immense variety of materials, weaving stories and arguments, paying attention to form and style. He very much wanted the book to be pleasant to read, more a piece of craft than an intellectual product. Before dying, he did not ask me to publish the book, nor did he work at it during his stay in the hospital (except for some notes on the third version of the introduction). I had asked him what he wanted me to do about the manuscript, and he had said: "keep it".

While going through Paul's unanswered mail, after his death, I found a long and thoughtful letter written to him by Bert Terpstra. The letter dealt with the very subjects Paul was writing about in **Conquest of Abundance** and struck me as having been written with sincerity, intelligence, and care. I replied to the letter, mentioning the manuscript of the book. After a correspondence that lasted some months and after Bert read part of the original manuscript, I realized that the

[1] Paul K. Feyerabend, **Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago 1999. This letter is based on the text of the Preface and Acknowledgements included in the original version of that volume.

book was potentially useful, and pleasant to read, to some individuals. In fact, its very incompleteness and fragmentary nature added a layer of ambiguity to the text and openness to its meaning — qualities that Paul was far from despising and are treated somehow in the text. Bert offered to work on compiling the book out of the various materials left by Paul. He mentioned that he was going to enjoy doing it (a fact that would have been essential for Paul) and that he was going to treat the material "as the fragments of an ancient vase" (a fact that was essential to me).

I accepted Bert's offer and soon became immensely grateful to him. As he was otherwise fully employed, he worked on the book in his free time. I may only imagine the care and constancy that it must have taken, as he dealt with a large amount of material, sometimes not available simultaneously and indeed with the attention and selflessness of a restorer of an ancient vase. The result, beautifully set to print by Chicago University Press, was an "unfinished product" — something inevitably far from what Paul would have liked to see. But we could enrich the work with a collection of many of the short pieces, reviews and commentaries that Paul enjoyed preparing in his last years. We compiled them hoping that his writings would give a special reading pleasure to some readers, that they may take them through a journey not unlike a music narrative, or a wondrous walk in the woods.

Conquest of Abundance recounts some particular moments of the evolving Western culture, times when complex worldviews, overflowing with an abundance of possible interpretations of Being — and thus realities — gave way to a few abstract concepts and stereotypical accounts. One of the main consequences of this "conquest of abundance", this coming to power of crude and monolithic ideas, is the drab world some of us live in today, a world obedient only to scientific dicta and economic imperatives.

Paul does not argue for any favored interpretation of "reality", nor does he aim at convincing anyone. Rather, the reader may feel taken, through detailed pathways, to a high vista. From there, a large and wondrous landscape opens up. Our sensory and culturally cast patterns of interpretation, which provide us with habitual and usually convenient ways of understanding and living with "reality", are perceived as the filters they actually are. The humane ebullience of Homeric Gods, the stochastic regularity of elementary particles and the devoted craftsmanship of Renaissance artists, all elements of the landscape, appear as entry points of

holograms. The elements create different realities while being part of them, and offering them a reading key.

As an observer of the landscape, I felt a sense of spontaneous tolerance toward all cultures and worldviews, and an appreciation of their open, changeable, ambiguous borders and distinctions. Exploring such cultural openness and ambiguity, I believe, is an aspect of the book that empowers the reader. If "potentially every culture is all cultures", then "efforts to achieve peace need no longer respect some alleged cultural integrity that [may be] nothing but the rule of one or another tyrant". [2] And such awareness does not lead to detachment or cynicism, but rather, in unison with Paul, to passionate engagement. The second empowering aspect of the book is an ontological consideration. If worldviews interact with Being in a mutually creating fashion, we do affect and shape "reality". We can choose to live in a world that makes sense to us.

Readers familiar with Paul's previous writings may find in his later work a quieter, more wondering attitude. They will also recognize, however, his bold connections, his impulsive remarks, and the usual stretching of any point of view away from comfortable positions. Paul was the first to subject himself to such gymnastics of the mind. He constantly revisited and challenged his own previous work. (Authors often write the same book several times under different titles. It has been said that Paul wrote three different books under the same title — the three published editions of **Against Method**). [3] In the last decade of his life, for instance, Paul was not at all pleased with **Science in a Free Society**, which he did not want to see reprinted. [4] Even the detailed treatment of relativism found in **Farewell to Reason** is further extended and, shall I say, overcome by the later writings. [5]

This, in fact, is the main motivation that convinced me that Paul would have liked to see his last book in print. He liked to let in some fresh air often, in the living room of our home as in any sort of intellectual construction. **Conquest of Abundance** — unorthodox, "ambiguous", open, unfinished — lets in some of that

[2] See Feyerabend, **Conquest of Abundance...**, note 25.

[3] See Ian Hacking, "Paul Feyerabend, Humanist", *Common Knowledge* 1994, Vol. 3, No. 2, pp. 23–28.

[4] See Paul K. Feyerabend, **Science in a Free Society**, NLB, London 1978.

[5] See Paul K. Feyerabend, **Farewell to Reason**, Verso, London 1988.

fresh air. [6] Even less than ever before, Paul is there not arguing nor striving to explain. By taking us through some questions and some stories, he just points at the abundance of Being, at the human openness and tolerance of ambiguity that allow us to savor it, and at the sensory and cultural filters that mediate our relation with it. As the poet Maulana Jajal al-Din gracefully expressed nearly eight centuries ago, those filters may be lived, studied, and enjoyed. [7] As Paul hints in his later work, those filters may also be compared in terms of how they reduce the richness and complexity of Being, how they support, or demean, a life that makes sense to us.

A story is like water
that you heat for your bath.
It takes messages between the fire
and your skin. It lets them meet
and it cleans you!

Very few can sit down
in the middle of the fire itself,
like a salamander or Abraham.
We need intermediaries.

A feeling of fullness comes,
but usually it takes some bread to bring it.

Beauty surrounds us,
but usually we need to be walking
in a garden to know it.

[6] See Feyerabend, **Conquest of Abundance...**

[7] The poem "Story Water" was composed in the thirteenth century A.D. by the Sufi Persian poet Maulana Jajal al-Din, better known as Rumi. The translation I quote comes from Coleman Barks, **The Essential — Rumi Reissue: A Poetry Anthology**, trans. Coleman Barks, J. Moyne, A. J. Arberry, and R. Nicholson, Harper San Francisco, New York 1995.

The body itself is a screen
To shield and partially reveal
the light. that's blazing
inside your presence.

Water, stories, the body,
all the things we do, are mediums
that hide and show what's hidden.

Study them, and enjoy this being washed
with a secret we sometimes know,
and then not.

Grazia Borrini-Feyerabend

References

1. Barks Coleman, **The Essential — Rumi Reissue: A Poetry Anthology**, trans. Coleman Barks, J. Moyne, A. J. Arberry, and R. Nicholson, Harper San Francisco, New York 1995.
2. Feyerabend Paul K., **Conquest of Abundance: A Tale of Abstraction Versus the Richness of Being**, Bert Terpstra (ed.), University of Chicago Press, Chicago 1999.
3. Feyerabend Paul K., **Farewell to Reason**, Verso, London 1988.
4. Feyerabend Paul K., **Science in a Free Society**, NLB, London 1978.
5. Hacking Ian, "Paul Feyerabend, Humanist", *Common Knowledge* 1994, Vol. 3, No. 2, pp. 23–28.

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 265–285

https://doi.org/10.53763/fag.2023.20.2.230

ARTYKUŁ ORYGINALNY / ORIGINAL ARTICLE

Paul K. Feyerabend

Knowledge without Epistemology [1]

Received: January 30, 2024. Accepted: February 13, 2024. Published online: April 27, 2024.

Abstract: The universality of scientific principles, theories, laws is never purely "objective", it has a strong anthropological component. A theory of knowledge invoking transhistorical agencies is therefore not only dead — it was never alive; its so-called successes are nothing but an immense chimera. Scientific research knows no universal boundary conditions or standards whether of a conventional, aprioristic, or empirical kind but uses and invents rules according to circumstance without regarding the selection as a separate "epistemic" act and often without realising that an important choice is being made.

Keywords:

Laudan;
Parmenides;
Xenophanes:
rules;
science;
theory of knowledge

Prophets of decay who are rather numerous in a time they themselves are calling the postmodern age have declared the end of epistemology. They did not use simple words; for example, they did not say "epistemology stinks" or "epistemology is counterproductive" or "epistemology is an empty game". They relied on practices such as deconstruction or hermeneutics which are even less comprehensible than the monster they were trying to exorcise. Now it is true that theories of knowledge lost much of their former bite and that they seem to succeed

[1] Editors' note. This text was written in 1992 in English and then translated into German by Hans Günter Holl. It was published under the title "Erkenntnis ohne Theorie. Vom Nutzen der Abstraktion und vom Recht des Besonderen" (*Lettre International* 16, Frühjahr 1992, pp. 66–71, https://tiny.pl/dq2fk [21.02.2024]). The abstract and keywords were added by the editors.

The editors wish to thank Dr. Grazia Borrini-Feyerabend for permission to publish the paper in *Filozoficzne Aspekty Genezy*. We also wish to thank Dr. Daniel Wilhelm from the Universität Konstanz (KIM/Abteilung Archive) for his assistance.

only because of an astounding blindness on part of their proponents. However, there are better ways of dealing with this phenomenon than entering the subject I just mentioned. One such better way (and one I intend to follow) is to trace the course of epistemology from its beginnings to the present time.

Beginnings are not easy to nail down. When did astronomy begin? Seidenberg and van der Waerden postulate international mathematics and astronomy which between 3000 and 2500 BC spread from Central Europe to Great Britain, the Near East, India and China. De Santiliana and von Dechend assume that the precession of the equinoxes was discovered when vernal equinox left the constellation of Gemini. Alexander Marshack's research suggests astronomically relevant notations around 30 000 BC. And so on. However, this uncertainty concerning *absolute* beginnings does not prevent us from using historical incisions as *relative* starting points of new phenomena. The incision I have in mind occurred in Greece, somewhere between 900 and 500 BC.

This was a period of major changes. Heroic forms of life receded, the city state with its very different requirements took their place (the resulting conflict was one of the main topics of tragedy). Money replaced gift giving and the exchange of goods, local gods merged, gained in power but lost in concreteness and humanity. Abstract laws, not personal relations defined the role of citizens in a democracy, wars were increasingly fought by professional soldiers — and so on. These changes occurred partly "by themselves", partly in the course of attempts to get out of sticky situation (most moves towards democracy were of the latter kind). Epistemology arose amidst this confusion, in the following manner.

We start with a rich spectrum of "epistemic" terms taking account of the many situations human face and the many ways in which they react to the world. Objects were viewed as aggregates of events, not as "real natures" surrounded by deceitful "appearances". One of the oldest arguments for scepticism which is found in Sextus Empiricus and which still occurs in Ayer's **Foundations of Empirical Knowledge** (1940) is that a rudder looks bent in water but feels straight to the touch. Separating appearance and reality and assuming that reality reveals itself through appearances we run into contradiction. No contradiction arises when the felt rudder and the seen rudder are considered as rudder — events which together with other events of this kind constitute the object referred to as/described as "rudder". Finally, there was no strict separation between physical prop-

erties and their emotional impact. The physical heat and the heat of passion were closely connected.

All this changed in the course of the developments I have just described. The changes were sufficiently drastic to call them a "beginning". This beginning or, as I shall call it, the rise of epistemology in the West has the following features.

First, the spectrum of "epistemic" terms is considerably reduced. Some terms disappear, others converge in meaning and finally coalesce. *Secondly*, we have a gradual separation, in objects, between what is later called their "reality" and the (often descriptive) ways in which they "appear". Like the other social changes these two developments occurred without any explicit and clearly planned contributions from individuals or special groups. Nobody said: "We must improve our language and make it more efficient". What happened was rather similar in its anonymity to the gradual transition from gift giving to an exchange of commodities and from there to a monetary economy.

An example of the second type of change occurs in the **Iliad**.[2] Achilles was offended by Agamemnon and withdrew from battle. Emissaries, Odysseus among them, agree that there was an offence; but, they say, matters have been set right and Achilles' honour is now restored. Now honour, like the rudder in my earlier example was an aggregate containing individual and collective actions and events. Some of the elements of the aggregate were: the role (of the individual possessing or lacking honour) in battle, in the assembly, during internal dissension; his place at public ceremonies; the spoils and gifts he received when the battle was finished and, naturally, his behaviour on all these occasions. Honor was present when (most of) the elements of the aggregate were present, absent otherwise.[3] The steps enumerated by the emissaries correspond to the list — Achilles' honour has indeed been restored. Achilles disagrees. "Equal fate" he says, "befalls the negligent and the valiant fighter; equal honour goes to the worthless and the virtuous".[4] Reacting in this way he turns the traditional *elements*, or *parts* of honour into untrustworthy *appearances* of a new entity which clashes with the received conceptions, is poor in content but eventually becomes more powerful than its

[2] Cf. Book 9, 225ff.

[3] Cf. **Iliad** 12, 310ff — Sarpedon's speech.

[4] **Iliad** 12, 318f.

concrete predecessor. This is an early instance of the real-apparent dichotomy which soon became a basic ingredient of knowledge.

I repeat that the changes I described so far were neither supported by argument nor explicitly encouraged by special professions. *They simply occurred*. The *third* characteristic of the "Rise of Epistemology" is that an aggressive new group of social critics, the philosophers, lived off the changes like parasites; they dwelled on them in an exaggerated way, heaped scorn on what has gone on before and were listened to because the ground was already prepared. Take Xenophanes' remark that

> if cattle, or lions, or horses had hands, just like humans;
>
> if they could paint with their hands, and draw, and thus create pictures –
>
> then the horses in drawing their gods would draw horses;
>
> and cattle would give us pictures and statues of cattle;
>
> and therefore, each would picture the gods to resemble their own construction.
>
> Aethiopian gods — snub-nosed and black
>
> Thracians — blue eyed and blond [...]. [5]

Here is what some modern writers have said about these lines. William Guthrie speaks of "destructive criticism". [6] Micrea Eliade, otherwise an intelligent judge of social matters, praises "Xenophanes's acute criticism", [7] while Karl Popper reads the fragments as the "discovery that the Greek stories about the gods cannot be taken seriously because they represent the gods as human beings". [8] Friedrich Nietzsche offered the following piece of bombast:

> No fashion helped them [the philosophers] and paved their way. Thus, they formed what Schopenhauer, in opposition to a republic of scholars called a republic of men of genius: one giant calls out to another across the desolate intervals of time and the

[5] Hermann Diels, Walther Kranz, **Die Fragmente der Vorsokratiker**, Weidmannsche Buchhandlung, Berlin 1959, 11, B 15, 16.

[6] William Keith Chambers Guthrie, **A History of Greek Philosophy**, **Vol. 1**, Cambridge University Press, Cambridge 1962, p. 370.

[7] Micrea Eliade, **Geschichte der religiösen Ideen**, **Band 2**, Freiburg im Breisgau, Herder 1979, p. 407.

[8] Karl R. Popper, **Auf der Suche nach einer besseren Welt**, Piper, München 1984, p. 218.

> lofty exchange between minds continues undisturbed by the noisy doings of the midgets that crawl beneath them [...]. [9]

while Hegel spoke more calmly of a "dissatisfaction with the world of the senses" and the appearance of "pure thought". [10]

Concentrating on the opinions of special individuals and taking it for granted that speech alone, when properly shaped and presented can damage the strongest tradition the gentlemen overlook that Xenophanes' criticism, for example, worked only because anthropomorphic gods were no longer the fashion — otherwise his lines merely elaborate a popular view. "You are so right" — a believer might have said — "our gods are tribal gods; they look like us, think like us, but are much more powerful. I expect that other tribes or nations have different gods, just as they have different rulers and that even the animals have gods of their own". Xenophanes, therefore, did not *start* the removal of anthropomorphic gods; he articulated a phenomenon that had arisen before, among "the midgets that crawl[ed] beneath him" and without whose assent his mockery would only have caused puzzlement. For local gods had indeed merged, they had shed some of their individual properties, had gained power but lost character, definition and humanity — they were well on the way to becoming pure Being. (Gilbert Murray conjectures that the differences between local gods were evened out as the result of travel.) *Given this background* Xenophanes' mockery *sounded* like an argument — that is all we can say.

The situation reveals a feature of argument that is unknown to Platonizing logicians and the philosophers who follow their lead. A story can be read in many ways, as an explanation of things that are being accepted, as an ironical characterization of things that are being rejected, as an artistic display, as an example of a valid argument — and so on. The sequence:

cotton needs a hot and dry climate;

England is cold and damp;

cotton does not grow in England

[9] Friedrich Nietzsche, "Die Philosophie im tragischen Zeitalter der Griechen", in: Friedrich Nietzsche (ed.), **Werke in drei Bänden**, **Band 3**, Carl Hanser, München 1954, p. 355 [353–388].

[10] Georg Wilhelm Friedrich Hegel, **Werke**, **Band 18**, **Vorlesungen über die Geschichte der Philosophie I**, Suhrkamp Verlag Frankfurt am Main 1971, p. 279.

contains three different pieces of information for illiterate peasants in Uzbekistan in the Thirties; [11] for a student of logic in Berkeley it is an example of a simple inference. But the student is not ahead of the peasants; put in their position and presented with a list of facts he will perceive connections, fail to concentrate on the individual items and thus will be slowed down in his reactions to them. At any rate — Xenophanes' lines *became* a criticism because of the surroundings in which they were uttered, they did not *create* these surroundings.

The high point of the development is *Parmenides*. At first sight Parmenides seems to continue an older (and still surviving) research tradition which tried to reduce the manyfold events of this world to a few simple principles. According to Thales (so Aristotle's story goes) the fundamental principle was a substance, and it was water. Thales most likely had arguments for his choice just as Prout had arguments for the assumption that the hydrogen atom was a fundamental building block of nature. Anaximander replaced water by an indefinite substance which could appear in different forms and which he called *apeiron*. Anaximenes replaced it by air, again using plausible arguments. Parmenides makes a suggestion which apparently fits right into this sequence: the entity sought is neither water, nor the apeiron, nor air — it is Being. However, Parmenides used logic, not plausible argument to choose his principle, he nailed down the consequences of his choice and he separated them from tradition and common experience just as scientists today separate their theoretical conceptions from what they see and hear when washing dishes at home. Parmenides did not invented logic — logical forms and patterns of argument played a large role in the practice of Greek and Near Western law. But he simplified the context, made it absolute, and relied almost exclusively on the type of argument now called *reductio ad absurdum*. His premise — *estin*: Being is [12] — is the first conservation law in the West; it declares the conservation of Being. It influenced thinking about nature either directly (Lavoisier, Robert Mayer) or indirectly, until today. It seemed plausible, at least to some of his listeners and readers, because of the general tendency towards abstraction I described above. From the premise Parmenides derives that Being does not change and that it has no parts. It does not change: change, for being, could only be to not-Being;

[11] Cf. Alexandr R. Luria, **The Making of Mind: A Personal Account of Soviet Psychology**, Harvard University Press, Cambridge Mass. 1979, chapter 4.

[12] Cf. Diels-Kranz, 18, B 7,7.

not Being does not exist, hence Being does not change. [13] It has no parts: a part must be different from the rest, the only possible difference is between being and not-Being, not-Being does not exist, hence there are no parts either. [14] (Here follows an interesting theory of continuity that was elaborated by Aristotle and is by far superior to Galileo's theory — and Weyl's.) For the present purpose the most important conclusion is, however, this: Parmenides' results conflict with experience, tradition and commonsense or, to use his expression, that *e'thos poly'peiron* [15] fails to provide knowledge — thought alone does. [16]

With this statement we have the first explicit theory of knowledge in the West. The theory subdivides phenomena into what is real, objective, trustworthy etc., (I am not using modern terms) and what is unreal, subjective, misleading. The subdivision has survived until today. It underlies the distinction between the arts and the sciences and, within the sciences, between systematic (objective, standardised) and anecdotal (subjective, historical) evidence. It played (and still plays) a large role in debates about the scientific status of certain types of historical research. The social sciences especially were distorted by trying to adapt to the Parmenidean framework. The dichotomy also gave intellectual support to Western domination over non-Western tribes, nations, cultures. Note, incidentally, that Parmenides' theory cannot be refuted by just pointing to the empirical fact of change. According to Parmenides this "fact" is a chimaera, just as dreamt levitations are a chimera for a Newtonian. Further means are needed to turn it into a source of truth (events such as the "Copernican Revolution" have been distorted by neglecting this feature of conceptual change).

I now come, *fourth*, to some consequences of the Parmenidean arguments. As I said before these consequences evolved under the impact of general trend towards abstraction and theory. Not all of them were the direct result of a reading of

[13] Cf. Diels-Kranz, 18, B 8, 6ff.

[14] Cf. Diels-Kranz, 18, B 8, 22ff.

[15] Diels-Kranz, 18, B 7, 3.

Footnote added by the editors. "habit born of experience" in Guthrie's translation. "It is a habit of thinking acquired by the repeated cultural experience" (Nicola Stefano Galgano, "Non-being in Parmenides, DK B2", *Anais de Filosofia Clássica* 2020, Vol. 28, No. 6, p. 5 [1–34]).

[16] Cf. Diels-Kranz, 18, B 1, 21.

Parmenides' poem. But the poem did have an influence though often in an underhanded way.

In mathematics definitions and arguments involving constructions were gradually supplemented with and even replaced by abstract arguments. Árpád Szabó ascribes this to the intrusion of indirect proofs and credits Parmenides with having been the first to introduce them. Others disagree. Reliance on the perception of symmetries (cf. the enclosed figure which shows that the sum of angles in a triangle is equal to the "straight angle" — of 180 degrees) was slowly eroded by the demand for a more intellectual approach. The resulting opposition between intuition and thought, construction and logical proof has survived until today.

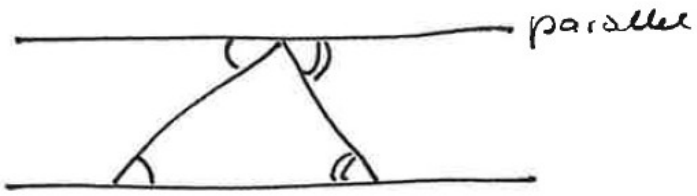

Figure 1. Drawing for the text made by Feyerabend

In medicine the connection with Parmenides is easier to ascertain. Traditional medicine as it is described in some tracts of the *Hippocratic Corpus* was an empirical discipline. There was no general definition of illness; there were lists of afflictions which served as guides for those already trained in recognizing the relevant symptoms. The physician relied on his mind, his eyes, ears, hands, and on his ability to identify complex perceptual patterns. Empedocles who followed Parmenides gave a theoretical definition of illness. He assumed four elements, water fire, earth and air; these were abstract essences, not the substances usually associated with the terms. Everything, the human body, included was said to be composed of the essences and illness was defined as imbalance between them. The reaction of the medical practitioners was swift and clear. "I fail to understand", writes the author of **Ancient Medicine** (chapter 15):

> how those [...] who abandon the old method and rest the *techne* on a postulate treat their patients on the lines of this postulate. For they have not discovered, I think, an

> absolute hot and cold, dry and moist [an alternative way of referring to the abstract elements] that participates in no other form. But I think they have at their disposal the same foods and the same drinks we all use, and to the one they add the attribute of being hot, to another, dry, to another, moist, since it would be futile to order patients to take something hot, as he would at once ask "what hot thing?". So they must either talk nonsense or take recourse to one of the known substances.

The implied dichotomy between body theoreticians and healers has also survived until today.

The most curious outcome of Parmenides' reasoning was its effect upon opponents. Already the sophists formulated their objections and their alternatives in Parmenidean terms. For example, the sensations of the ancient empiricist were not ingredients of commonsense; they were theoretical entities similar to Parmenides' One. And the cultures enumerated by relativists are not living things which change and interact, they are instances of *e'thos poly'peiron*. Moreover, the ancient as well as the modern opponents still offer a *theory* of knowledge, i.e. a general account that tries to assemble all types of information under a single (and rather empty) concept. Only the sceptics (the ancient sceptics, not Hume) avoided that trap. The influence of the ancient theoreticians and of the background which gave substance to their claims was indeed enormous.

However — and now comes an important observation which further complicates the story — it was not only influence. Older attitudes and ways of thought persisted and found influential (though, as I indicated, somewhat contaminated) defenders among the sophists and in Aristotle. Plato, who speaks of the "ancient battle between philosophy and poetry" [17] gives indirect evidence of their power: the first answers to Socrates' "what is...?" questions are always lists and those who provide them resist Socrates' drive toward unity. [18] More recently philosophers such as Wittgenstein and, in greater detail, Austin have shown how complex, many-sided *and sensible* commonsense accounts are when compared with philosophical analyses. They support "the other side" which, incidentally, raised its head numerous times, in the guise of nominalism, empiricism, scepticism, historicism, though always streamlined by the quest for theory. Still, the Parmenidean tradition was, *fifth*, strong enough to give rise to the following problem: if truth is indeed as separated from our common lives as Parmenides seems to indicate,

[17] Plato, **The Republic**, 607b.

[18] Cf. Plato, **Meno**, 72d; Plato, **Theaetetus**, 148b.

then how can we ever reach it? Or, to use a modern formulation — *how is knowledge possible*?

It is important to repeat that the question is not a natural result of an alleged urge for knowledge but obtains its power from a combination of individual machinations and social tendencies. The machinations make sense and seem compelling because their premises (Parmenides) or hidden assumptions (Xenophanes) resonate with the tendencies. The fact that other forms of knowledge remained influential and often raised their heads shows that this sense and this compulsion were limited and empirical, not universal and necessary. The alternatives populated not only books, but real life; they aggravated idea hunters like Hegel who interpreted Plato's use of mythical forms of discourse as a sign of an "impotence of thought"; [19] and they are now being strengthened by development workers who try to remove the damage done by an unthinking imposition of the more abstract parts of Western Civilization, by local initiatives in the Western countries themselves and they are constantly being nourished by the arts, those lasting strongholds of idiosyncracy, anarchy and contrariness. "One of the reasons for the asphyxiating atmosphere in which we live without possible escape and remedy", writes Artaud,

> and in which we all share, even the most revolutionary among us – is our respect for what has been written, formulated, or painted, what has been given form, as if all expression were not at least exhausted, were not a point where things must break apart if they are to start anew and begin afresh. [20]

The only obstacle to fully recognizing the alternatives are the sciences which have tremendous authority and which seem to contain precisely the kind of "objective" knowledge Parmenides defended. It was by reference to the sciences that Kant tried to answer the question "How is knowledge possible" and it is by reference to the sciences that modern realists uphold the appearance/reality dichotomy. Do the sciences support their efforts?

There are two answers to this question and they are both negative. The first answer rests on scientific results, the second on the way in which scientific results

[19] "Ohnmacht des Gedankens", Georg Wilhelm Friedrich Hegel, **Werke**, **Band 19**, **Vorlesungen über die Geschichte der Philosophie II**, Suhrkamp Verlag Frankfurt am Main 1971, p. 31.

[20] Antonin Artaud, **The Theatre and its Double**, trans. Mary Caroline Richards, Grove Press Books, New York 1958, p. 74.

are being obtained.

According to the first answer *the dichotomy conflicts with one of the best confirmed scientific theories that ever existed*, viz. the quantum theory. The conflict was sharpened, in a paradoxical way, by the very thinkers who tried to uphold objectivity.

The second answer is that, contrary to widespread opinion, *science is an elaboration of the "other side", i.e. of the traditions Parmenides wanted to overcome*. It is not easy to show this in detail — too many prejudices lie in our way. But consider the following story and the argument embedded in it.

We start with the usual exploitation of the Parmenidean image of science: Kantianism, Neopositivism and its noisy offspring, critical rationalism. There are philosophers in the sense of Parmenides, abstract accounts of the nature and the conditions of knowledge. They not only claim to have found what scientists do, they also claim to know what they ought to do.

The next step is Kuhn. Kuhn showed that the sciences do not fit the philosophical pattern and that they would wither away if one tried to force them into it. Kuhn still uses general notions like "paradigm", "revolution", "mature science" to make his point. He thereby encouraged abstract accounts of a different kind, which are as unrelated to their subject matter as were the essays of the Kantians and the positivists. The further step that was needed to get out of the Parmenidean trap was taken by younger and very active generation of historians who concentrated on individual episodes, who have almost produced a "history in the raw" and whose conclusions no longer coalesce into a single coherent system of thought: a cookbook of more or less successful recipes is all we get. Science is not therefore "irrational" — cooking certainly is not an irrational activity — there exist arguments for every move that is being made. But whatever universality we find comes from the actions of scientists who, having immersed themselves in various problem situations and using their experience, cunning and other forms of "tacit knowledge", now happen to treat different situations in a similar way. The universality of scientific principles, theories, laws is never purely "objective", it has a strong anthropological component. A theory of knowledge invoking transhistorical agencies is therefore not only dead — it was never alive; its so-called successes are nothing but an immense chimera. To illustrate this situation I shall

now discuss a recent attempt to revive old-style philosophies namely L. Laudan's book **Science and Relativism** (Chicago 1990).

Laudan's book is a dialogue between four characters — a relativist, a pragmatist, a realist and a positivist. Relativism is the main target, pragmatism the philosophy closest to Laudan's heart. The main topic is the nature and growth of scientific *knowledge*. What is actually being discussed is the fate of high knowledge. What is actually being discussed is the fate of *high theory*, to use a term from elementary particle physics. This leaves out (a very incomplete list): botany and its various branches, geography, ecology, descriptive astronomy, in short all those subjects which accumulate and classify observations without transcending them; it also leaves out phenomenology (in the sense in which this term is being used in high energy physics), experimental design, practically oriented approaches such as Noll's mechanics, models such as the models used to calculate, say, lunar perturbations, experimental inquiry which enriches knowledge in its own way (spectroscopic handbooks, lists of resonances, catalogues of celestial objects such as the Messier catalogue or the Cambridge catalogues of radio astronomy) — as well as the approximations, special assumptions, *ad hoc* hypotheses, designed to bring some kind of coherence into this untidy mass of facts, fragments of theory, tacit assumptions etc. etc. No doubt Laudan believes that the idiosyncrasies of the particular can be absorbed by theory and that models, collections, approximations, phenomenology are just steps on the way. The trouble is that there was little research in the past to check this belief and that more recent inquiries (Hacking, van Fraassen, Cartwright and others) have thrown serious doubt upon it. Whatever unity arises in the course of Laudan's debate is therefore a result of omissions — it is a unity of neglect, not a unity produced by synthetic efforts.

The debate further dilutes this already quite reduced material. Epistemology, says the pragmatist, did not vanish with the demise of certainty. There "remain important epistemic questions" [21] such as how best to construct theories, when to regard a theory as well supported, when to prefer it to a rival.

These are important questions indeed. They faced Einstein when he wanted to combine the insight of special relativity with the principle of equivalence; they faced scientists later on when the Brans-Dicke suggestions led to difficult experi-

[21] Larry Laudan, **Science and Relativism: Some Key Controversies in the Philosophy of Science**, University of Chicago Press, Chicago 1990, p. 136.

ments and a proliferation of theories of gravitation; they accompany every step of scientific research. But can they be answered by a person who replaces science by a caricature, is unaware of its divergent ingredients, lacks the mathematical skills, the judgement and especially the "tacit knowledge" which define an area of inquiry? The older epistemologists who believed that such details were part of a general structure which could be detached from science and judged independently said yes. Laudan agrees. True, he rejects any "perfect-being epistemology" [22] and wants to evaluate scientific standards empirically, using "any of the forms of reasoning appropriate to scientific research"; [23] science itself is supposed to tell us which standards are acceptable and which are not. But as I just pointed out, the science Laudan discusses is a fragment, not the real thing and the "forms of reasoning" he refers to are those of logician dealing with stable entities of well-defined content, not those of scientist facing an incomplete and incoherent material.

Our theories, says the pragmatist, "are worthy of acceptance [...] because they work"; [24] "they confer abilities on us — abilities to control, predict and manipulate nature" [25] — "our rules are worthy of acceptance, because they have shown themselves to be able consistently to pick out theories which work with a pretty high degree of reliability". [26]

The quotation suggests the following scenario: there are rules (standards, norms), and there are theories. The rules are let loose on the theories and encouraged to make their favourite choice. Some rules choose losers — they are discarded. Others "consistently pick out theories that work" — they are invited into the Epistemological Hall of Fame.

The scenario is most unrealistic. First, because many theories which have surprising successes in one area fail in others. Newton's mechanics was in this position, all through its history. Shall we say that it worked? All the time? Some of the time? Never? That depends on standards which decide when a theory is supposed

[22] Laudan, **Science and Relativism...**, p. 105.

[23] Laudan, **Science and Relativism...**, p. 99.

[24] Laudan, **Science and Relativism...**, p. 106.

[25] Laudan, **Science and Relativism...**, p. 107.

[26] Laudan, **Science and Relativism...**, p. 106.

to be acceptable — the very standards Laudan wants to define by his procedure. Secondly, high theory and phenomenological approaches often seem to be successful in the same area — but they are constructed in different ways. An honest pragmatist would of course prefer phenomenology and engineering approaches which (usually, but not always) fit the facts much better than the theories allegedly backing them (and which have to go through approximations, *ad hoc* adaptations, questionable assumptions such as Dirac's ocean of occupied states to reach the facts). They "work" better, according to Laudan's own criterion — yet all his examples come from high theory. Thirdly, theories which "work" do not enter the world ready-made and their success is not independent of the treatment they receive. Theories start from modest beginnings, they go through a complicated process of growth, suffer some encouragement and numerous defeats and need different incentives and/or correctives at different stages of their development. Each incentive (corrective) can be formulated as a rule, each rule contributes to the survival and eventual success of the theory, each of them "picks out" the right candidate which means that Laudan's Hall of Fame will contain mutually incompatible standards — unless standards are tied to the situations in which they are used. But then a general account of when a particular standard is to be applied becomes as impossible as a general account of the conditions of historical events. All we can say is who did what in which circumstances, what resulted — and we may try to remember the actions for future reference just as a politician tries to remember what his predecessors did in similar cases.

A brief look at some of the standards Laudan is willing to defend shows the extent to which he still depends on old-style philosophies. According to his mouthpiece (the pragmatist) highly confirmed predictive schemes are to be preferred to theories which make some kind of sense (they conform to plausible symmetry principles or metaphysical views) but lack empirical support. "Our allegiance" says Laudan, should be to the former; the latter are "unacceptable". [27] He overlooks that "unacceptable" views can and often did discredit their "acceptable" rivals. That was done either by a cunning *transfer* of the evidence (example: Copernicus' discussion of the movement of the earth in Book 1 of his **De Revolutionibus**, imitated and elaborated by Galileo) or by an *analysis* which, starting with an "unacceptable" point of view showed the spuriousness of the evidence against it

[27] Laudan, **Science and Relativism…**, p. 30.

(example: Einstein's analysis of Exner's measurements of Brownian motion), [28] or by purely logical analysis of the presuppositions of an apparently decisive experiment which was *motivated* by a strong belief in an "unacceptable" alternative (example: Planck's analysis of Kaufmann's experiments as described in chapter 6 of Zahar's **Einstein's Revolution**) [29] or by simply refusing to take the evidence at its face value (many examples). In all these cases "allegiance" went to the empirical underdog, used him in ways that denied success by almost all the participants in Laudan's dialogue [30] and led to major scientific advances. It may be true that "if an approach is bogged down, and failing to produce interesting new results, [scientists] quickly vote with their feet"; [31] opportunists exist in all professions. But discovery often came from those who opposed the fashions of the time. Of course, even the most metaphysically inclined scientist hopes eventually to confirm his conjectures by novel observations and striking experiments — but to achieve *this* aim, he must first suspend his allegiance to a *given* harmony between theory and fact.

Arguing further along this line we are led to suspect that scientific research knows no universal boundary conditions or standards whether of a conventional, aprioristic, or empirical kind but uses and invents rules according to circumstance without regarding the selection as a separate "epistemic" act and often without realising that an important choice is being made. For example, the arrival of large-scale experimental equipment in high energy physics changed the older (and empirically selectable) demand that experiments must be repeatable, but without any explicit "epistemic" debate.

None of the participants of the dialogue, the relativist included seems to be aware of this feature of scientific practice. Trying to rise to what they think is a more "philosophical" level they engage in a debate where the relativist turns historical facts (defeated theories *occasionally* stage a triumphant comeback discarded views *occasionally* become powerful critics of their successful rivals) into

[28] Cf. Felix M. Exner, "Notiz zu Brown's Molecularbewegung", *Annalen der Physik* 1900, Vol. 502, No. 2–3, pp. 843–847, https://doi.org/10.1002/andp.19003070813.

[29] Cf. Elie Zahar, **Einstein's Revolution: A Study in Heuristic**, Open Court, Chicago and La Salle, Ill. 1989, pp. 201–226.

[30] Cf. Laudan, **Science and Relativism...**, p. 84.

[31] Laudan, **Science and Relativism...**, p. 156.

abstract principles ("any theory [...] is as good as any other") [32] while his opponents think that having thrown doubt upon the principles, they have also disposed of the facts. [33]

Altogether the tendency to emphasise logical possibilities (Duhem-Quine argument etc.) gives the debate an air of unreality. Take the assumption that, given some body of evidence, there exist "indefinitely many — arguably infinitely many" [34] mutually incompatible theories which are compatible with it. The relativist takes the assumption for granted. But how can he do that? Given a finite universe and a finite lifetime for the human race there can only be a finite number of sentences — so, has relativism become Platonistic? Besides, Platonic infinites which are not trivial (infinitely many possible values of a constant within the range of error) and which satisfy certain minimal conditions have to be argued for — they cannot simply be assumed. Are there infinitely many prime numbers? It needs a (simple) proof to decide the question. Yet even when Platonic infinities are assumed, and even when they exist, we still cannot infer that theory selection is "arbitrary", [35] that "all rival hypotheses are on the same footing" [36] or that social factors play a "key role in explaining the doxastic life of scientists". [37] A general may not know the infinitely many ways in which angels can win a battle — but this makes his decisions arbitrary, or one decision as good as another only in the eyes of a god contemptuous of human affairs. In *this* world (and here I apply an argument which Aristotle used vis-a-vis Parmenides) — which is the world where scientists try to understand nature and philosophers scientists — we have restricted resources both as regards the theories which are being offered for choice and the ways of choosing and doing science means operating within these resources. Inferring arbitrariness from the limited nature of our resources also means making a comparison (between the resources and a world they can never reach) which is itself beyond the resources and, therefore, "arbitrary". Strangely

[32] Laudan, **Science and Relativism...**, p. 55, see also p. 76.

[33] Cf. Laudan, **Science and Relativism...**, p. 84 and passim.

[34] Laudan, **Science and Relativism...**, p. 49.

[35] Laudan, **Science and Relativism...**, p. 45.

[36] Laudan, **Science and Relativism...**, p. 54.

[37] Laudan, **Science and Relativism...**, p. 157.

enough it is the relativist of the dialogue who produces metaphysical romances of this kind.

Turning now to the role of social conditions we can at once admit that they have an effect, though not in the ways envisaged by relativism (and some social scientists). To start with, modern pluralistic societies contain many trends and a wide variety of reactions to them — just look at the many different forms of theatre, literature, the arts, some wildly critical of prominent habits. Facing this plurality a scientist has a choice; he is no longer at the mercy of a single ideology. Besides, ideas taken over from "society" are never left unchanged. Aristotle mobilised commonsense against the views of Parmenides and Plato. He *consciously* tried to preserve it — he was not simply overwhelmed — but he also *revised* it, using the achievements of those he opposed. The same is true of the sciences. Even a scientist who has fallen for some powerful fashion has to pay attention to a second fashion, namely, the situation in his own subject.

Do we need a special subject, philosophy, to "explain [...] the success of science"? [38] No, we don't. First, because it is not "science" which is successful — some so-called sciences are a pretty sorry sight — particular models, theories, procedures are. Secondly, because scientists, on the way to success invent and use procedures which, when detached from the area of their application *look* like global principles but lead to success only because they are not so *used*. [39] It is true that scientific standards have been defended by philosophers and may even have been introduced by them. But different scientists then applied the standards in different ways and without paying much attention to their philosophical origin. The biologist Luria prefers "predictions that will be strongly supported or sharply rejected by a clear-cut experimental step", [40] he shows little enthusiasm for a theoretical science that is "loaded with weak inferences" [41] and reports that Fermi, for this very reason, was somewhat cool towards the general theory of relativity. Gauquelin assembled impressive evidence for astrological correlations (his "Mars effect") — yet few empirically minded scientists have shown readiness to accept

[38] Laudan, **Science and Relativism...**, p. 167.

[39] Cf. what I said about Laudan's method of selecting and confirming scientific standards.

[40] Salvador Edward Luria, **A Slot Machine, a Broken Test Tube: An Autobiography**, Harper & Row, New York 1985, p. 115.

[41] Luria, **A Slot Machine...**, p. 119.

his conclusions. Supergravity does not object to tests but makes them dependent on what happens during the early moments of the universe. Naturally, many apparently direct refutations can be and are being circumvented by suitable explanations. Feynman is not at all pleased — he wants a more direct relation between a theory and the facts. [42] All the parties just mentioned are scientists, they are all empiricists, they all favour experiments, but empiricism has a different meaning for each of them. One might say that epistemological principles become effective within the sciences only by losing their (perhaps unambiguous) philosophical content and acquiring a (highly ambiguous) scientific content (Platonic unities becomes lists when turned loose on the world). Thirdly, many so-called successes can be explained by purely scientific means. Why was Newton's theory successful in explaining some simple features of planetary motion? Because space is curved etc. and Newton's approach was a good approximation to that scenario.

Epistemology is not the only discipline that tries to explain and to control an activity capable of standing on its own feet. Every area of human endeavour is surrounded by generalities which may be useful when immersed in and dissolved by practice but which impoverish our resources when imposed unchanged. Brecht had a beautiful and very "rational" theory of theatrical action but his plays either fell flat on their face — which they did when they were as didactic as the theory demanded — or they moved the audience, in which case the theory was left behind. The solution is not, as some extremists have suggested, to abolish generalities altogether; the solution is to bring them in close contact with the topics they are about. More concretely: it does not make sense to have calculating machines advised by epistemologists unaware of the nature of the scientific discoveries they praise with such abandon, or to have playwrights concerned only with box office receipts advised by aestheticians incapable of facing an audience without a heavily annotated manuscript in their hands.

What we need are thoughtful scientists (artists, playwrights, priests, politicians etc. etc.) who are experts in the twin arts of modifying what is general by tying it to particulars and of explaining what is particular in general terms — in other words, we need a marriage of universals and particulars. Such a marriage would be the end of philosophy (epistemology, aesthetics etc.) as a separate subject with standards, problems, solutions of its own but it would also be the begin-

[42] Cf. P.C.W. Davies and Julian Brown (eds.), **Superstrings: A Theory of Everything?**, Cambridge University Press, New York 1988, p. 194.

ning of a rich and fruitful life. Now the interesting thing is that the marriage already exists — it has existed for centuries — and that it has produced numerous offspring. The art works that surround us, the scientific theories which resemble them in so many ways are all products of a close collaboration between far reaching fantasies, the concepts and procedures that were invented to give them shape and the idiosyncratic particulars constituting the lives of all of us. But philosophers — intent on remaining in control (remember Plato's talk about the "ancient battle between philosophy and poetry") have tried and are still trying to prove that we are dealing with a master-slave relation where wise masters pushing around capable but rather ignorant slaves succeed in shaping the world after their own image. Needless to say — education has to be thoroughly changed to remove this farce from our midst.

Now assume that what I have said is true — does preaching this truth contribute towards solving the problems of our time? Can it reduce the mass murders that are happening right now in many countries, can it remove the intolerance, the lack of concern and understanding, the narrow egoism of individuals, businesses, institutions which have ruined the earth, are aware of the crime but still show no signs of regret or reformation? Not all intellectuals are asking questions such as these. Many are content with scoring a victory over fellow intellectuals and thus to accumulate a reputation in small autistic circles. But there are writers, artists, scientists, theologians, professional philosophers among them who do consider the matter, who think that ideas can indeed make a difference to the lives of our generation and of future generations and who let this conviction guide them in their thoughts and their writings. I am less optimistic. Asked what I have done, by this paper, by the talk that preceded it and generally, by my rather chaotic opus to bring a little peace and happiness into the world I can only reply: nothing. Nothing at all. And why? Because ideas are weaker than the most gentle breeze — one can move right through them. They become powerful only if the situation has already been prepared. [43] Does this mean that a writer should strive for a connection with power, that (s)he should adapt his/her text to the actual and potential sources of power or, as the fashionable phrase runs, should they try to be relevant? My answer is no, they should not. And my reason is that *relevance can only be determined after the event*. Social circumstances are ambiguous in the sense that a situation which seems to condemn certain actions and words to futil-

[43] Cf. my comments on Xenophanes and Parmenides in the text above.

ity is often unstable, and may be blown up by the very same actions and words. We do not know whether what we are doing is "relevant" — until we have done it and even then the effect may take a long time to appear. So, all we can do is listen to our friends (if we have anyto read, make music, watch soap operas, if that is our inclination, consider what is going on around us and take our clues from there. I, personally, would add that we should be careful not to recommend or participate in actions which are dictated by hatred and are liable to increase it. I don't say this because I have a theory about the effects of hatred, but because I, I personally, do not intend to be guided by it. That is all I can say. The search for a more objective justification is just as chimerical as the theories which advise us that the justification exists.

Paul K. Feyerabend

References

1. Artaud Antonin, **The Theatre and its Double**, trans. Mary Caroline Richards, Grove Press Books, New York 1958.
2. Davies P.C.W. and Brown Julian (eds.), **Superstrings: A Theory of Everything?**, Cambridge University Press, New York 1988.
3. Diels Hermann, Kranz Walther, **Die Fragmente der Vorsokratiker**, Weidmannsche Buchhandlung, Berlin 1959.
4. Eliade Micrea, **Geschichte der religiösen Ideen**, **Band 2**, Freiburg im Breisgau, Herder 1979.
5. Exner Felix M., "Notiz zu Brown's Molecularbewegung", *Annalen der Physik* 1900, Vol. 502, No. 2–3, pp. 843–847, https://doi.org/10.1002/andp.19003070813.
6. Guthrie William Keith Chambers, **A History of Greek Philosophy**, **Vol. 1**, Cambridge University Press, Cambridge 1962.
7. Galgano Nicola Stefano, "Non-being in Parmenides, DK B2", *Anais de Filosofia Clássica* 2020, Vol. 28, n.6, pp. 1–34.
8. Hegel Georg Wilhelm Friedrich, **Werke**, **Band 18**, **Vorlesungen über die Geschichte der Philosophie I**, Suhrkamp Verlag Frankfurt am Main 1971.
9. Hegel Georg Wilhelm Friedrich, **Werke**, **Band 19**, **Vorlesungen über die Geschichte der Philosophie II**, Suhrkamp Verlag Frankfurt am Main 1971.
10. Laudan Larry, **Science and Relativism: Some Key Controversies in the Philosophy of Science**, University of Chicago Press, Chicago 1990.

11. Luria Alexandr R., **The Making of Mind: A Personal Account of Soviet Psychology**, Harvard University Press, Cambridge 1979.
12. Luria Salvador Edward, **A Slot Machine, a Broken Test Tube: An Autobiography**, Harper & Row, New York 1985.
13. Nietzsche Friedrich, "Die Philosophie im tragischen Zeitalter der Griechen", in: Friedrich Nietzsche (ed.), **Werke in drei Bänden**, **Band 3**, Carl Hanser, München 1954, pp. 353–388.
14. Popper Karl R., **Auf der Suche nach einer besseren Welt**, Piper, München 1984.
15. Zahar Elie, **Einstein's Revolution: A Study in Heuristic**, Open Court, Chicago and La Salle, Ill. 1989.

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 287

https://fag.ifil.uz.zgora.pl/index.php/fag/issue/view/24/71

Volume Reviewers

Published online: May 28, 2024.

Piotr Dahlig — Uniwersytet Warszawski;

Adam Grzeliński — Uniwersytet Mikołaja Kopernika w Toruniu;

John F. Haught — Georgetown University;

Kazimierz Jodkowski — Uniwersytet Zielonogórski;

Sławomir Leciejewski — Uniwersytet im. Adama Mickiewicza w Poznaniu;

Bożena Muszkalska — Uniwersytet Wrocławski;

Andrzej Niemczuk — Uniwersytet Rzeszowski;

David Lamb — University of Manchester;

Andrzej Łukasik — Uniwersytet Kardynała Stefana Wyszyńskiego;

Jacek Paśniczek — Uniwersytet Marii Curie-Skłodowskiej;

Piotr Roszak — Universidad de Navarra;

Marek Słomka — Katolicki Uniwersytet Lubelski Jana Pawła II;

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 289–290

https://fag.ifil.uz.zgora.pl/index.php/fag/issue/view/24/72

Advisory Board

Published online: May 28, 2024.

ISSN 2299-0356

Filozoficzne Aspekty Genezy — **2023, t. 20, nr 2**

Philosophical Aspects of Origin s. 291–302

https://fag.ifil.uz.zgora.pl/index.php/fag/issue/view/24/73

Publishing Policy

Published online: May 28, 2024.

Philosophical Aspects of Origin (Polish title: *Filozoficzne Aspekty Genezy*) (ISSN 2299-0356) is an online philosophical journal devoted to the problem of origin — of the universe, the first life, subsequent life forms, man, mind, consciousness, language, scientific theories, religions etc. The scope of the journal also covers philosophical or methodological analysis of theories or beliefs related to the problem of origin.

We accept submissions written in Polish and, starting from 2014, in English: this includes articles, polemics, translations, book reviews and letters to the editor. In 2022, the journal became a biannual.

Manuscripts should be sent to the deputy editor-in-chief's e-mail address (g.malec@fag.ifil.uz.zgora.pl). You can also use our online submission system (https://fag.ifil.uz.zgora.pl/index.php/fag/about/submissions). Manuscripts written in Polish should be accompanied with a summary and keywords, in both Polish and English, and an English title should also be provided. Authors of manuscripts written in English should include a summary and keywords in English only.

The reviewing process in *Philosophical Aspects of Origin* is based on the *double-blind* principle, where neither the reviewers nor the author know each other's personal details. It is, however, the responsibility of an author to compose the manuscript in such a way so as to conceal his or her identity. Any content that might reveal an author's identity can be added later, after the manuscript has

been accepted for publication. The names of all of the reviewers who contributed to a given volume are provided in the last issue of each volume.

All submitted manuscripts, after initial acceptance by the editor-in-chief, are sent to two independent reviewers affiliated at academic institutions different to that of the author. Based on the opinions of the reviewers, the editorial board will decide whether or not to accept the text in question for publication. Accepted manuscripts, after the typesetting and text makeup processes have been completed, will be sent back to the author(s) as proofs for final adjustment. *Where no proofread version of an article has been sent back by the specified deadline, it will be assumed that the author agrees that no corrections are necessary and that the article can be published as is*.

The editorial board of *Philosophical Aspects of Origin* will do their utmost to publish the accepted PDF version of the article online as quickly as possible. It should be kept in mind, however, that the order of articles in a given volume is decided only after the volume has been closed to further submissions; hence, until then the page numbering of the articles should be treated as temporary.

Ensuring that good scientific practices are being promoted, the editorial board of *Philosophical Aspects of Origin* actively opposes *ghostwriting* and *guest authorship*. *Ghostwriting* is related to not mentioning the name of an individual who has contributed significantly to the article and should be considered the author or a co-author. *Guest authorship* means mentioning an individual as a co-author despite the fact that his or her contribution is negligible or nonexistent. The above are examples of scientific misconduct; hence, any improprieties of this sort that are uncovered will be appropriately documented and publicized. The editorial board will contact the relevant authorities, including the institutions employing the authors of the manuscript in question, as well as other relevant academic institutions or journals. Therefore, all prospective authors are hereby asked to provide appropriate information about who contributed to the work being submitted, and to what extent this was the case. Authors are responsible for ensuring that any such information is true and correct. All queries pertaining to such matters should be addressed to: info@fag.ifil.uz.zgora.pl.

Authors submitting their work are required to confirm the relevant data in our Submission Preparation Checklist. We accept the definitions of “author” and “contributor” formulated by The International Committee of Medical Journal Edi-

tors. All those involved in the running of our journal are excluded from decisions relating to their own texts, and from activities leading to situations in which a conflict of interest may be expected to arise. All queries pertaining to such matters should be addressed to: info@fag.ifil.uz.zgora.pl.

All decisions concerning the acceptance or rejection of submitted texts are to be made by our editorial board after completion of a double-blind process of peer-review. Any such decision will be final and only subject to revision in special cases — e.g., when it transpires that there is a reasonable basis for supposing that a conflict of interest is involved, or that reviews exhibit bias. In such situations, the editor-in-chief can be asked to supply an additional evaluation, and the text will then be accepted or rejected on that basis. Each and every complaint or appeal will be considered by our editorial board as promptly as possible. All queries pertaining to such matters should be addressed to: info@fag.ifil.uz.zgora.pl.

All parties involved in the publication process — authors, co-authors, subject editors, reviewers and all members of the editorial board — should in every instance disclose any circumstances potentially affecting their capacity to adopt an objective standpoint when considering a specific topic or evaluating works by others. Any conflict of interest, or situation that might be considered to constitute such a conflict, should be reported immediately. Each and every case of this sort will be considered by our editorial board as promptly as possible. The editorial board also asks authors to provide the details regarding the funding schemes or funding bodies connected to the submitted manuscript. Submitted manuscripts must be original work and not previously published. Also, there can be no conflicts of interest relating to the financial ties of authors with individuals or institutions such as could negatively influence their research findings. All queries about such matters should be addressed to: info@fag.ifil.uz.zgora.pl.

Citation Rules (abridged version)

Citation numbers should be placed **AFTER** punctuation marks, rather than before (i.e., after a full stop, semi-colon, or comma).

When providing a link to the full, online version of the cited text, you should shorten the link using one of the internet platforms such as https://tiny.pl/ or https://cutt.ly. Shortened links should look like this: https://cutt.ly/LvvW49N [24.11.2019]; https://tiny.pl/r82b2 [24.11.2019]. The link should be followed by the date of the last access, written in [dd.mm.year] format: i.e. [24.11.2017].

A. Quoting Books

(a) First citation: author's name (surname in small caps); title in bold typeface; if the book is translated from a foreign language, then the translator's name should be indicated after the title; if the book has been published as a part of a series, then the name of the series ought to be written in italics, while its number should be written in normal typeface after the comma; publisher; publication location; year; page number(s). Examples:

Karin Knorr-Cetina, **The Manufacture of Knowledge**, Pergamon, New York 1981, pp. 395–396; Richard Dawkins, **The Blind Watchmaker: Why the Evidence of Evolution Reveals a Universe without Design**, Norton & Company, London & New York 1986, p. 142.

(b) Subsequent citations: author's last name (in small caps); abbreviated title (or the whole title when it is short) ending with an ellipsis (which is treated as a punctuation mark substituting all the other bibliographical data of the text, rather than only the further part of the title); page number(s). Examples:

Knorr-Cetina, **The Manufacture…**, pp. 395–396; Dawkins, **The Blind Watchmaker…**, p. 48.

B. Quoting Articles, Reviews, etc.

(a) First citation: author's name (last name in small caps); title in inverted commas; in the case of a translation, "trans." followed by the translator's name; the name of the journal in italics and the year of publication; the number of the volume, issue or part of the volume; the page number; the first and last page of the text in square brackets; if the article appeared in a collective work, the citation

should list the name of the editor followed by the abbreviation "ed." (in brackets) or its equivalent in other languages after the title or after the name of the translator; title of the collective work; publisher; place and year of publication; page (the first and the last page of the text in square brackets). Examples:

Dieter Münch, "Minds, Brains and Cognitive Science", in: Armin Burkhardt (ed.), **Speech Acts, Meaning and Intentions: Critical Approaches to the Philosophy of John R. Searle**, De Gruyter, Berlin 1990, p. 372 [367–390]; Gonzalo Munévar, "Allowing Contradictions in Science", *Metaphilosophy* 1982, Vol. 13, No. 1, p. 76 [75–78].

(b) Subsequent citations: author's last name (in small caps); abbreviated title ending with an ellipsis; page number(s). Examples:

Münch, "Minds, Brains…", p. 373; Munévar, "Allowing Contradictions in Science…", p. 77.

C. Quoting Excerpts from Other Authors' Scientific Papers

If the excerpt extends beyond just a few words, separating it from the rest of the text as an indented block quote is highly recommended: use left-side indentation combined with a small space at the top and bottom, change the font size to 10 points, and apply single-line spacing. This paragraph serves as an example of said format:

> This allows us to achieve a certain visual effect. The text becomes less monotonous and more nuanced and appealing. Excerpts taken from other people's work are highlighted and, consequently, easier to find again later.

However, if the quote is short, it will suffice to put it in quotation marks. In the case of longer excerpts, inverted commas should not be used, as the indentation and other typesetting changes produce the same effect.

D. References

The list of references should be sorted alphabetically by authors' last names. It should look like this:

Boyer Pascal, "Religion: Bound to Believe?", *Nature* 2008, Vol. 455, pp. 1038–1039.

Munévar Gonzalo, "Allowing Contradictions in Science", *Metaphilosophy* 1982, Vol. 13, No. 1, pp. 75–78.

Citation Rules (full version, explanations included)

Submitted texts should use the so-called "Zielona Góra Citation System" developed by Professor Kazimierz Jodkowski, the founder of *Philosophical Aspects of Origin*. Here we provide examples of the proper use of that system, and explain them further down below.

First and foremost, citation numbers should be placed **AFTER** punctuation marks, rather than before (i.e., after a full stop, semi-colon, or comma). This is where our system deviates from the so-called PWN (Polish Scientific Publishers) standard, according to which the superscript numeral is placed before the punctuation mark, after the last word. Unfortunately, the PWN standard can sometimes engender misunderstandings and silly situations, such as these:

(a) Let us suppose that we want to put a citation number at the end of a sentence that ends like this: "[...] in the U.S.". Where, in such a case, should we put it? Before the full stop? This would clash with one of the functions of the full stop, because not only does it end the sentence, but it also indicates an abbreviation; hence, putting the superscript numeral in front of it will undermine the latter function and make the sentence opaque. On the other hand, this problem disappears when we put it after the punctuation mark.

(b) Let us suppose that we want to put such a number at the end of a sentence discussing the number of atoms in the universe, whose closing words are "[...] is 10^{80}". Adhering to the PWN standard, we should put the citation number before the full stop, thus generating a faulty statement: "[...] is 10^{805}". In the Zielona Góra System this problem does not exist, as the citation numeral comes after the punctuation mark, giving us "[...] is 10^{80}.[5]"

When providing a link to the full, on-line version of the cited text, you should shorten the link by means of internet platforms such as https://tiny.pl/ or https://cutt.ly. Shortened links should look like this: https://cutt.ly/LvvW49N [24.11.2019]; https://tiny.pl/r82b2 [24.11.2019]. The link should be followed by the date of the last access, written in [dd.mm.year] format: i.e. [24.11.2017].

A. Quoting Books

(a) First citation: author's name (surname in small caps); title in bold typeface; if the book is translated from a foreign language, then the translator's

name should be indicated after the title; if the book has been published as a part of a series, then the name of the series ought to be written in italics, while its number should be written in normal typeface; publisher; publication location; year; page number(s). Examples:

Richard Dawkins, **The Blind Watchmaker: Why the Evidence of Evolution Reveals a Universe without Design**, Norton & Company, London & New York 1986, p. 142; Paul K. Feyerabend, **Against Method**, Verso, London 1993, p. 211.

(b) Subsequent citations: author's last name (in small caps); abbreviated title (or the whole title when it is short) ending with an ellipsis (which is treated as a punctuation mark substituting all the other bibliographical data of the text, rather than only the further part of the title); page number(s). Examples:

Dawkins, **The Blind Watchmaker...**, p. 142; Feyerabend, **Against Method...**, p. 211.

B. Quoting Articles, Reviews, etc.

(a) First citation: author's name (last name in small caps); title in quotation marks; in the case of a translation, "trans." followed by the translator's name; the name of the journal in italics and the year of publication; the number of the volume; the number or part of the volume; the page number; the first and last page of the text in square brackets; if the article appeared in a collective work, the citation should list the name of the editor followed by the abbreviation "ed." (in brackets) or its equivalent in other languages after the title or after the name of the translator; title of the collective work; publisher; place and year of publication; page (the first and the last page of the text in square brackets). Examples:

Dieter Münch, "Minds, Brains and Cognitive Science", in: Armin Burkhardt (ed.), **Speech Acts, Meaning and Intentions: Critical Approaches to the Philosophy of John R. Searle**, De Gruyter, Berlin 1990, p. 372 [367–390]; Gonzalo Munévar, "Allowing Contradictions in Science", *Metaphilosophy* 1982, Vol. 13, No. 1, p. 76 [75–78].

(b) Subsequent citations: author's last name (in small caps); abbreviated title ending with an ellipsis; page number(s). Examples:

Münch, "Minds, Brains…", p. 372; Munévar, "Allowing Contradictions in Science…", p. 76.

Why do we prefer this method over more conventional ones?

Some authors, when referring to a given publication in the main text or a footnote, give the name of the author and the year of publication. To do so, they may use such formats as: Feyerabend 1965, Feyerabend [1965] or [Feyerabend 1965]. They add page number(s) after a comma or a colon: [Feyerabend 1965, p. 34] or [Feyerabend 1965: 34]. Full bibliographical details are then provided in the bibliographic index at the end of the publication. Some authors go even further and get rid of the author's name altogether, replacing it with the number of the entry in the bibliographic index, i.e. [34, p. 17] or [34: 17]. This citation system, together with its many variants, is arguably the worst possible one for researchers in the humanities; it has some serious flaws that I will now proceed to set out.

1) The method is good for citing works in the area of the natural sciences, where the only important thing is to pinpoint when, and by whom, a given discovery was documented in a publication, and not what the publication's title was. However, in the humanities, apart from the author's name and the year of publication, the title is also relevant. Let us imagine an essay which states: "As Popper showed in 1959, though it was contested by Kuhn in 1962…". It sounds bizarre, doesn't it? That is because we would normally phrase it like this: "As Popper showed in **The Logic of Scientific Discovery**, though it was contested by Kuhn in **The Structure of Scientific Revolutions**…".

2) Another major flaw of this method is that it is extremely easy to make a mistake. A finger might slip and we end up with a wrong date or a wrong letter (a, b, c, etc.) differentiating multiple publications by the same author during one year. On the other hand, when we make a small mistake in the title it is still recognizable. An author publishing a text in our journal had originally employed the method in question. When trying to adjust his work to comply with the Zielona Góra standard, he encountered difficulties due to errors that became apparent and hard to correct. This particular flaw is more forgiving towards works in the area of the natural sciences, as they tend to be shorter and cite less sources. As a result, it is much easier to avoid committing errors. However, texts in the humanities can be up to several times longer and contain many more bibliographical sources.

3) The third defect of the PWN standard is that it makes the footnotes for ancient writers appear quite odd: Aristotle 1985, Plato 2003, and so on. Providing the title of a publication or its abbreviation in accordance with Zielona Góra system appears natural regardless of the era in which the cited author lived. This

flaw is not as striking in works from the natural sciences, as they most frequently refer only to relatively recent publications. Usually, a physicist or astronomer will not be concerned with what Newton or Copernicus had to say about a given subject.

4) The last flaw of this system that we want to point out pertains to citations of authors who have "common" last names. Sometimes one needs to cite several people of the same name (e.g., Hintikka, or Nagel). It then becomes impossible to avoid mentioning the name, and so the approach becomes inconsistent: on one occasion the name is given, on another not.

All of the defects listed above can be avoided by simply providing the first and last name, title, and other bibliographical data for the publication, while quoting.

Why the first name and not, as is usually the case, just the initial? Firstly, because sometimes the name allows us to recognize the gender of the author, and on occasion even their nationality. (We recommend that authors avoid translating names into their Polish equivalents, unless they have already entered common use, as is the case with, for example, Karol Darwin.) If the name Henryk (Henryk Mehlberg) appears on the cover of the book **The Reach of Science,** it is clear that regardless of the author's origin and place of residence, he identified as a Pole. Besides, it is simply worth knowing the names of the authors, as people are so frequently the object of our discussions in the humanities (as opposed to in the natural sciences, which deal mainly with problems for their own sake).

Why should the author's last name be in small caps? For two reasons.

Firstly, sometimes the reader does not know what corresponds to the person's first name, and what to their last name. For example, John Maynard Smith, the famous evolutionist, may pass among those who lack the necessary knowledge as a Smith with two Christian names: John and Maynard. However, his full last name is in fact Maynard Smith, and his first name is John. Small caps prevent these misunderstandings.

Secondly, sometimes publications are written by several authors, and some names are also mentioned in the title. To give an authentic example: Joseph Agassi, Tristram Shandy, Pierre Menard, and All That: Comments on **Criticism and the Growth of Knowledge**, *Inquiry* 1971, Vol. 14, pp. 152–164. If we write the surname(s) of the author(s) in small caps, as in Joseph Agassi, "Tristram Shandy, Pierre Menard, and All That: Comments on **Criticism and the Growth of**

Knowledge", *Inquiry* 1971, Vol. 14, pp. 152–164, then we disambiguate more clearly between Agassi alone writing an article on Shandy and Menard, and Agassi and Shandy and Menard jointly penning one that is exclusively about comments concerning **Criticism and the Growth of Knowledge**.

Why should the title of a book be in bold, and the title of an article not?

In the most popular Polish citation system — the so-called PWN standard — both book and article titles are written in italics. The primary disadvantage of this approach is that it makes it more difficult to identify the type of the publication (is it a book or an article?). Although it does not pose any problems in the first citation — if the publisher, place and year of publication are present, then we know we are dealing with a book, whereas if we see the title of a journal and then the issue number it suggests an article. But what happens with each subsequent citation? It is abbreviated: we do not repeat all the bibliographical details and so, if our memory fails us, we will have trouble deciphering whether we are dealing with an article or a book. Sometimes even a good memory won't help! Dennett wrote a book and an article with the same title: **Darwin's Dangerous Idea**. With an abbreviated citation, only the typeface will allow us to distinguish between the book and the article. Under the PWN System, these two publications are indistinguishable.

If citation rules applied exclusively to footnotes, then we might desist from placing article titles in inverted commas as recommended under the Zielona Góra Citation System. However, we sometimes want to include the title of an article in the main text. In such cases, if we do not put the title in inverted commas, then it will get mixed up with the rest of the text. We eliminate this difficulty by putting the titles of articles in inverted commas — and so, for the sake of consistency, should also do so in footnotes.

For the same reason — that of being rendered distinctive in the main text — journal titles should be written in italics.

An additional disadvantage of the PWN System is that it requires foreign words and expressions to be italicised, even as its norms demand that article titles be likewise written in italics. This leads to a problem when the publication title contains foreign expressions. How to mark italics within italics? This problem is solved by the Zielona Góra System. An authentic example: Nicholas Tiho Mirov, **The Genus *Pinus***, Ronald Press Co., New York 1967.

In the first citation, aside from the specified page number, the first and the last page of the article should be indicated in square brackets. Experience shows that it is extremely helpful from the author's perspective — they do not then need to revisit their sources when preparing their bibliography. It can also, on some occasions, help to identify errors.

In subsequent citations, an ellipsis is employed to indicate that some of the bibliographical data has been omitted.

C. Quoting Excerpts from Other Authors' Scientific Papers

If the excerpt extends beyond just a few words, separating it from the rest of the text as an indented block quote is highly recommended: use left-side indentation combined with a small space at the top and bottom, change the font size to 10 points, and apply single-line spacing. This paragraph serves as an example of said format:

> This allows us to achieve a certain visual effect. The text becomes less monotonous and more nuanced and appealing. Excerpts taken from other people's work are highlighted and, consequently, easier to find again later.

However, if the quote is short, it will suffice to put it in quotation marks. In the case of longer excerpts, inverted commas should not be used, as the indentation and other typesetting changes produce the same effect.

Quoting works not translated into Polish is another important issue here. Quoting a text in its original language is strongly discouraged, and quoting both the original and the translation even more so. There is one exception to this rule: authors can, and even should, quote the original text if there exists an important reason to do so — e.g., if the original has some important features that cannot be properly translated into Polish (such as a certain ambiguity or allusiveness that could get lost in translation, or perhaps a play on words that usually cannot be reproduced in other languages, etc.). Another such reason might be that we are arguing against some author who referred to the passage in question and, in our opinion, was mistaken. In this case, we need to quote the original so that the reader believes us rather than the author we are arguing against. Yet another reason could be that the original text possesses some unique quality we wish readers to savour, such as its particularly deft phrasing or the acuity with which it

puts across some point, such that the excerpt merits being quoted in its original version. Depending on the length of such quote, we may include it in the main text or as a footnote.

D. References

The list of references should be sorted alphabetically by authors' last names. It should look like this:

Boyer Pascal, "Religion: Bound to Believe?", *Nature* 2008, Vol. 455, pp. 1038–1039.

Munévar Gonzalo, "Allowing Contradictions in Science", *Metaphilosophy* 1982, Vol. 13, No. 1, pp. 75–78.

Krzysztof K. Kilian

www.fag.ifil.uz.zgora.pl

CZASOPISMO INTERNETOWE/ONLINE JOURNAL

ISSN 2299-0356

Filozoficzne Aspekty Genezy

Philosophical Aspects of Origin

PÓŁROCZNIK/BIANNUAL

2023

tom 20(2)
vol.

numer specjalny 1
special issue