ISSN 2299-0356

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

Philosophical Aspects of Origin s. 1–34

https://doi.org/10.53763/fag.2023.20.2.224

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 counter-productive.

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 $O \sim O'$. 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.