

Feyerabend and Kuhn on Monism and Pluralism

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Feyerabend had many interlocutors in his controversial career, and one of them was Kuhn. One key point of contention in their interaction was the divergence between the monism inherent in Kuhnian normal science and Feyerabend's pluralism about the content and methodology of science and other systems of knowledge. In this paper I offer my perspective on this disagreement. After presenting Feyerabend's critique of Kuhn, I argue that the disagreement between Kuhn and Feyerabend on this point was not as radical as it may appear. Feyerabend respected the autonomy of diverse cultural and epistemological traditions, and such traditions are often monistic within themselves, in the manner of Kuhnian normal science. On the other hand, Kuhnian revolutions require the presence of competing paradigms at least during periods of extraordinary science. I propose a pluralist position that can accommodate local monism, but ultimately recommends going beyond monism for the purpose of productive interactions between different systems of practice. Such a pluralism can incorporate the advantages of both Feyerabend's liberal epistemology and Kuhn's advocacy of disciplined normal science.

Keywords: Feyerabend, Kuhn, monism, pluralism, normal science, dogmatism, paradigm

Фейерабенд и Кун о монизме и плюрализме

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Кун был одним из многих собеседников в противоречивой карьере Фейерабенда. В спорах Фейерабенда с Куном одним из ключевых вопросов было расхождение между монизмом, присущим куновской нормальной науке, и плюрализмом, который Фейерабенд защищал в отношении науки и других систем знаний. В этой статье я предлагаю свой взгляд на это разногласие. Я рассматриваю критику Фейерабенда в адрес Куна и показываю, что разногласие между Куном и Фейерабендом по этому вопросу не было таким радикальным, как может показаться. Фейерабенд уважал автономию различных культурных и эпистемологических традиций, и такие традиции часто являются монистическими внутри себя, подобно куновской нормальной науке. С другой стороны, куновские революции требуют наличия конкурирующих парадигм, по крайней мере, в периоды экстраординарной науки. Я предлагаю плюралистическую позицию, которая может учитывать локальный монизм, но в конечном итоге рекомендует выходить за пределы монизма для продуктивного взаимодействия между различными системами практики. Такой плюрализм может включать в себя преимущества как либеральной эпистемологии Фейерабенда, так и позиции Куна, защищающей нормальную науку.

Ключевые слова: Фейерабенд, Кун, монизм, плюрализм, нормальная наука, догматизм, парадигма



To a casual observer of the philosophy of science, Paul Feyerabend and Thomas Kuhn would seem to have played very similar roles in the 1960s and 1970s. They each contributed greatly to the demise of the traditional picture of science that was built on both the common sense among scientists and the philosophical legacy of logical positivism. However, appearances can be deceiving. In this paper I will try to delineate the complex relation between Feyerabend's and Kuhn's philosophies of science. There are two layers of deceptive appearances to be peeled away. First of all, even though the two philosophers had some common ideas, Feyerabend's critique of Kuhn's views advanced in *The Structure of Scientific Revolutions* was very sharp, particularly in relation to the Kuhnian concept of "normal science". But it would also be a mistake to take Feyerabend's critique of Kuhn simply at face value. There were deeper commonalities between their ideas than Feyerabend was ready to admit, and it is possible to reconcile their positions in a productive way, though perhaps neither of them would have been happy to adopt the synthesis that I will offer here.

This paper has three main objectives. First, I will try to come to a clear and detailed understanding of Feyerabend's critique of Kuhn, which was given in detail in two particular places: Feyerabend's letters to Kuhn commenting on a draft of *The Structure of Scientific Revolutions* [Hoyningen-Huene, 1995; 2006], and his paper "Consolations for the Specialist" published in the volume *Criticism and Growth of Knowledge* [Lakatos, Musgrave, 1970] arising from the famous symposium on Kuhn's work held in London in 1965 [Feyerabend, 1970].¹ The core of Feyerabend's objection was based on his pluralism, against the kind of dogmatic monism shown in Kuhn's conception of normal science. Second, I will show that the divergence between Kuhn and Feyerabend was not as large as Feyerabend made it out to be. On the one hand, Feyerabendian pluralism can and should allow each autonomous tradition to have disciplined coherence, which is quite liable to slide into monism. On the other hand, the health of Kuhnian normal science depends on a degree pluralism at least during the periods of extraordinary science. Third, I will argue that in Feyerabend's early critique of Kuhn there were clear pointers to a systematic kind of pluralism that the bravado of Feyerabend's later philosophy concealed. This kind of pluralism can in fact accommodate the best aspects of Kuhn's philosophy, as well as Feyerabend's own.

¹ The opening footnote in Feyerabend's paper [Feyerabend, 1970, p. 197] indicates that an earlier version of this paper was given at Popper's seminar in March 1967, and the preface to the volume [Lakatos, Musgrave, 1970] confirms that Feyerabend did not speak at the 1965 symposium. Kuhn gave a response to all the papers in the volume, in which he was quite brief on Feyerabend and mostly treated him as one of the Popperians [Kuhn, 1970c, pp. 245, 254].



Feyerabend's Critique of *Structure*

As is well known, Feyerabend and Kuhn overlapped briefly at the University of California at Berkeley during a brief period around 1960. This was a crucial period of development for both of them. In 1962 they both published major works in the philosophy of science: Kuhn's now-classic book *The Structure of Scientific Revolutions* (henceforth *Structure*), and Feyerabend's now-neglected paper "Explanation, Reduction, and Empiricism". These publications contained some of the key ideas which made them connected with each other in many people's minds, including the notion of "incommensurability", by which they meant similar enough things. But Feyerabend was a very harsh critic of the draft of *Structure* that Kuhn shared with him. There is much evidence that they had involved philosophical discussions with each other in person.² Those conversations were not recorded, but Feyerabend also wrote enormously long letters to Kuhn at the time, detailing numerous points of criticism. Four letters of Feyerabend commenting on *Structure* were discovered by Paul Hoyningen-Huene in the papers left by Feyerabend and Kuhn after their deaths; Feyerabend did not put dates on these letters, but Hoyningen-Huene [Hoyningen-Huene, 2006, pp. 611–612] reaches a reasonable conclusion that they must have been written between May 1961 and sometime in 1962 before *Structure* appeared in print. These letters were edited and published by Hoyningen-Huene with careful annotation pointing to relevant passages in the published version of *Structure*. These letters deserve careful scrutiny, which I will seek to give here.

The main target in Feyerabend's critique of *Structure* was monism, particularly as manifested in Kuhn's description of "normal science". Near the start of his first letter to Kuhn, Feyerabend articulated this target clearly: "You have expressed to me your belief, and you express it again in your essay[,] that it is only by concentrating *on a single paradigm*, by trying to fit nature into it despite all apparent difficulties, that scientific progress is achieved." [Hoyningen-Huene, 1995, pp. 355–356]³ Later on he used the term "monism" to describe his target, calling out Kuhn's "hidden predilection for monism (for one paradigm)" [Ibid., p. 367]. The word itself is not used in his 1970 paper, but the meaning remains very clear in this passage: "He [Kuhn] defends not only the *use* of theoretical assumptions, but the *exclusive choice* of one particular set of ideas, the monomaniac concern with only one single point of view." [Feyerabend, 1970,

² Feyerabend [Feyerabend, 1970, p. 197] opened "Consolation for the Specialist" by remembering these personal interactions.

³ Feyerabend's letters will be cited as [Hoyningen-Huene, 1995] or [Hoyningen-Huene, 2006], but all quotations are Feyerabend's own statements. All emphases are by Feyerabend himself; he underlined a great number of words and phrases in his letters to Kuhn.



p. 201] He saw the monism in Kuhn's thinking clearly and identified it explicitly as such, rather than just expressing worries about dogmatism, as Karl Popper [Popper, 1970], John Watkins [Watkins, 1970] and others did.

Feyerabend attacked Kuhn's monism from various angles. First, he quickly dismissed a psychological argument in favor of monism that Kuhn might have had in mind: "You seem to think it psychologically impossible for a scientist and, indeed, for any human being to be able to entertain various alternative hypotheses and to discuss them impassionately. I think you are a little too pessimistic." The only argument Feyerabend provided for his view here was historical: "Faraday did so... and so did the Presocratics, so did Einstein". [Hoyningen-Huene, 1995, p. 356] Here Feyerabend was also indicating an objection to Kuhn's description of history, about which I will say more below.

After waving away the psychological argument for monism, Feyerabend proceeded to make normative arguments against Kuhnian monism. His initial point was that theoretical pluralism increased the empirical content of science. It is the critical contention between opposing theories that allows scientists to maintain and increase the empirical content of those theories:

Also I think I have shown in my own essay⁴ that considering a set of mutually inconsistent but factually adequate theories *increases* the empirical content of any element of the set and this for the simple reason that many tests *presuppose* the existence of an alternative! (they are crucial tests) If this is the case then we must make the decision: what do we prefer, increased empirical content of the theories we possess, or that unanimity of research and the close fitting produced by it in the periods which you call the normal periods. [Ibid.]

Feyerabend thought that normal science ran the risk of becoming dogmatic: "this method of excluding novelties, this attempt to press nature into the boxes of the theory will gradually *decrease* the empirical content of the theory until it is finally almost zero." This is in direct contradiction to Kuhn's view that normal science was the most effective method of gaining factual knowledge about nature. Feyerabend thought that a revolution was needed in order to shake scientists out of dogmatic stagnation: "A scientific revolution which shows the limitation of the theory and which points out very plainly where it is wrong therefore *gives back empirical content* to the theory". Therefore "*Revolution in Permanence* should be the battle cry of every empiricist." [Ibid., p. 358] Note an unspoken pluralist rendering of scientific revolutions here. In the Kuhnian picture the vanquished old paradigm is discarded by scientists, only to be remembered by historians henceforth. Not so for Feyerabend: revolutionary

⁴ I think here he was probably referring to [Feyerabend, 1962].



struggle (and even defeat) actually revitalizes the old theory by making it meaningfully testable again, thereby restoring its empirical content and making it scientific again in the Popperian sense. So the old theory will live on after a revolution, even though it is not the leading contender any more: “after all we still use the classical mechanics for the calculation of the behavior of the upper planets.” [Hoyningen-Huene, 2006, p. 626]

While Kuhn argued that the most strenuous test of a paradigm was made through the detailed and precise esoteric research carried out in normal science, Feyerabend stressed the limitations of this process. Kuhn responded to the worry that the dogmatism of normal science would prevent revolutionary developments by pointing out that “a puzzle-solving tradition can prepare the way for its own displacement” [Kuhn, 1970b, p. 10]. In *Structure* itself he argued: “Research under a paradigm must be a particularly effective way of inducing paradigm change” [Kuhn, 1970a, p. 52]. Unlike the typical scientist whose vision is dominated by present triumphs, Kuhn had a seasoned pessimism of the historian: no matter how successful a paradigm is, it will eventually uncover anomalies that it cannot handle, and fall into a crisis, and then make way to a new paradigm that can resolve the crisis. But Feyerabend asked: is the dogmatic pursuit of the dominant paradigm the only way, or even the best way, to arrive at a crisis? Normal science only reveals anomalies in certain directions. For example, Feyerabend argued that phenomenological thermodynamics by itself would not have uncovered the challenge of Brownian motion. Rather, this anomaly was “discovered by the elaboration of an *alternative account*, viz. of the kinetic theory which then produced predictions that could be tested by experiment.” One could dispute the details of the history of Brownian motion implied in Feyerabend’s claim here, but his general point is clear and plausible: “Your [Kuhn’s] insistence upon faithfulness to one and only one paradigm is bound to result in the elimination of otherwise very important tests and it is bound in this way to reduce the empirical content of the paradigm”. He invoked David Bohm in support of the point that “the limitations of the present point of view will become evident only if one has first introduced an alternative and shown that it is preferable” [Hoyningen-Huene, 1995, p. 365]. Feyerabend chided Kuhn for claiming that “invention of alternatives is just what scientists do not, and probably ought not undertake”.⁵

Feyerabend returned to this point in his second letter to Kuhn. In this rendition of the argument, Feyerabend was perhaps responding to a defence given in conversation, which Kuhn articulated in print only a decade later: contrary to what Watkins alleged [Watkins, 1970, pp. 29–32], Kuhn

⁵ Kuhn quoted by Feyerabend, in [Hoyningen-Huene, 1995, p. 365]. The statement is from p. 70 of the manuscript of *Structure*, and Hoyningen-Huene locates a similar passage on p. 76 of the second edition of the published version of *Structure* [Kuhn, 1970a].



was not “down-valuing” scientific revolutions in comparison to normal science. On the contrary, Kuhn [Kuhn, 1970c, p. 241] stated that he shared “the conviction that the central episodes in scientific advance – those which make the game worth playing and the play worth studying – are revolutions.” Well, then, Feyerabend inferred, it must be a good thing to drive normal science into a crisis so that we can have a revolution: “if you welcome acceleration of the development towards crises *you must also welcome consideration of alternative paradigms* which, *as you admit yourself*, may lead to crises.” [Hoyningen-Huene, 1995, p. 374] Feyerabend could see no reason why only crises generated from the dogmatic pursuit of normal science were to be welcomed. In fact he sketched out a general theory of scientific crises, which he didn’t develop further to the best of my knowledge. There are three distinct types of causes for a crisis in a paradigm: “(1) failure to fit nature into its categories...; (2) inconsistency with successful alternatives that have been developed, either with the intention of elaborating the main paradigm... or on the basis of a completely different metaphysics that has been sleeping for some time...; (3) internal unclarity”. Then he added: “It seems to me that every crisis contains *all these three elements*.” [Ibid., p. 375]

It is worth noting that what we see in these arguments is not quite the later, more (in)famous Feyerabend of *Against Method* [Feyerabend, 1975a] and *Science in a Free Society* [Feyerabend, 1978]. Feyerabend in the early 1960s was still guided strongly by the epistemological dimension of Popperian philosophy, focused on testability and empirical content. His third letter to Kuhn gave a very accurate representation of the sophisticated falsificationism only familiar to those who had really delved into the middle of Popper’s *Logic of Scientific Discovery* [Hoyningen-Huene, 2006, pp. 624–628]. In fact, far from being “against method”, Feyerabend’s 1961/2 critique of Kuhn continually invoked the importance of methodology and methodological rules. He also resisted Kuhn’s inclination to take methodology to be paradigm-dependent. In a surprisingly universalist register, Feyerabend argued that the “only non-arbitrary elements” of science were “the methodological ones, i.e. the stipulations which demand such relatively trivial things as that the theories be testable, that *ad hoc* hypotheses... be avoided etc. etc.” [Hoyningen-Huene, 1995, p. 359]. Advocating for stronger empirical tests of theories, he argued that “in order to be able to give reasons for one’s predilections one has to refer to methodological considerations” [Ibid., p. 362]. While accepting that methodological rules could not fix all scientific decisions, Feyerabend argued that there were some basic ground-rules to be respected in all of science: “Although there is no set of rules capable of explaining *every* move that is being made, there are rules which definitely *forbid* that certain moves are made.” As examples he singled out the prohibition of *ad hoc* hypotheses (again), and of “dogmatic moves, i.e. moves which decrease the testability of a given hypothesis” [Ibid., p. 363;



Hoyningen-Huene, 2006, p. 626]. In another passage he pointed to basic empiricism as part of universal scientific methodology, in saying that not all theories and perspectives were to be allowed in science: “the limits are set by the methodological rules which exclude some of the ‘ways of seeing the world’ on account of the fact that they are not about the world at all, but interesting fairy tales (their ‘logic’ is different from the ‘logic’ of scientific theories).” [Hoyningen-Huene, 1995, p. 357]

There was a strong normativism in Feyerabend’s critique of Kuhn, which was in fact a lasting tendency in Feyerabend’s philosophy of science more generally, and throughout his academic life. “Anything goes” should not be mistaken as an expression of relativism free of value-judgment, in any phase of Feyerabend’s philosophy. And Feyerabend had a violent objection to what he saw as Kuhn’s pretence that he was simply giving a description of science as it has been practiced over the centuries. In fact this was the very first point that Feyerabend launched against Kuhn in his first letter: “What you are writing is not just history. It is *ideology covered up as history*.” [Ibid., p. 355] Feyerabend stressed this point again in his published critique of Kuhn: “Whenever I read Kuhn, I am troubled by the following question: are we here presented with *methodological prescriptions* which tell the scientists 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... I venture to guess that the ambiguity is *intended* and that Kuhn wants to fully exploit its propagandistic potentialities.” [Feyerabend, 1970, pp. 198–199]

Feyerabend saw “danger” [Hoyningen-Huene, 1995, p. 354] in Kuhn’s blurring of the descriptive – normative boundary, especially when it came to the monist presentation of “normal” science:

What I do object to most emphatically is the way you present this belief of yours; you present it not as a *demand*, but as something that is an obvious consequence of historical facts. Or rather, you do not even talk about this belief, you let it as it were emerge from history as if history could tell you anything about the way you *should* run science (is *does not* imply ought!). It is this bewitching way of representation to which I object most, the fact that you take your readers in rather than trying to persuade them. [Ibid., p. 355]

This point erupted again later in the same letter, with more passion and vitriol:

...you present an ideology, and a very questionable monolithic ideology at that, in the covers of history. In this respect you are really very similar to those who point to history in order to justify their crimes. You are a mystic, an irrationalist. And by this I mean that you not only hold certain beliefs (conservative character of normal science), but that you are not prepared to let these beliefs speak for themselves; you rather present



them in a manner which suggests that they are facts and thereby force people to swallow them without criticising them. What are you afraid of? [Hoyningen-Huene, 1995, p. 367]

What Feyerabend demanded of Kuhn was that he should be upfront about his belief about how science should be done, so that the readers can be aware that they are being confronted with someone's normative view, which they can then evaluate for themselves. Pretending that one can "just do history" has an insidious effect: "Historical presentations have a curious influence. They tell what is the case. But sometimes they make people feel that this is what ought to be done. And they make people feel that way especially when the writer of the history has this belief himself." [Ibid., p. 361] If Kuhn thought monist normal science was the best way to learn about nature, Feyerabend thought he should come out and say that, and provide philosophical arguments for that claim, the same way Feyerabend himself was arguing explicitly for his pluralist philosophy. He kept returning to this point, devoting his entire third letter to it [Hoyningen-Huene, 2006, pp. 614–618].

Feyerabend also thought that Kuhn's hidden normative monism resulted in a descriptive distortion of history. What Feyerabend denounced as Kuhn's "hidden predilection for monism" (quoted above) actually led to "a false report of historical events". For example, Kuhn presented "classical physics" as one paradigm, when it was actually "a bundle of alternatives" (contact action vs. action at a distance, reversibility vs. irreversibility, etc.) [Hoyningen-Huene, 1995, p. 367]. Feyerabend made the same critical descriptive point concerning revolutions, claiming that he could not find a single historical case in which a crisis was only (or almost exclusively) caused solely by a monistic pursuit of the single dominant paradigm [Ibid., p. 376].

All in all, Feyerabend thought that Kuhn had not given an accurate description of what science is – neither in the descriptive-sociological sense of what happens in those activities that have commonly been called "science", nor in the more normative-philosophical sense of what it is that we commonly value and admire when we call something "scientific". This sense of disappointment found its most acute expression in Feyerabend's taunt that Kuhnian normal science was no different from organized crime:

According to this [Kuhn's] interpretation it is the existence of a puzzle-solving tradition that *de facto* sets the sciences apart from other activities... But if the existence of a puzzle-solving tradition is so essential, if it is the occurrence of this property that unifies and characterizes a specific and well recognizable discipline; then I do not see how we shall be able to exclude say, Oxford philosophy, or, to take an even more extreme example, *organized crime* from our considerations. For organized crime, so it would seem, is certainly puzzle-solving *par excellence*. Every



statement which Kuhn makes about normal science remains true when we replace ‘normal science’ by ‘organized crime’; and every statement he has written about the ‘individual scientist’ applies with equal force to, say, the individual safebreaker [Feyerabend, 1970, pp. 199–200].

Then Feyerabend went on to actually carry out this exercise for a long paragraph, which I will not quote in full. This point was already indicated briefly in Feyerabend’s first letter to Kuhn [Hoyningen-Huene, 1995, p. 360], but in the 1970 paper we begin to see the outrageous and effective rhetorician that Feyerabend became in his later writings.

By the time Feyerabend wrote *Against Method* [Feyerabend, 1975a] and *Science in a Free Society* [Feyerabend, 1978], Kuhn was no longer at the centre of his polemical universe. On the one hand he had started feuding with the Popperians (amicably with Lakatos and less so with others), and on the other hand he directed his critique to science itself, or at any rate to the hegemonic aspect of Western science that was tied up with colonial and post-colonial domination of the rest of the world and with the military-industrial complex. But the kind of dogmatic monism that Kuhn regarded as an essential feature of science in its mature and normal state remained an anathema to Feyerabend, and seemingly an exact antithesis of the epistemological “anarchism” or “dadaism” that Feyerabend was articulating in the 1970s and beyond. Pluralism would remain at the core of this new phase of Feyerabend’s thinking.

I will not elaborate much on Feyerabend’s pluralism in the 1970s and beyond, as that is well-trodden ground [Lloyd, 1987; Preston, 1997, ch. 7; Oberheim, 2006, Part III; Shaw, 2018]. I just want to note that the chief expression of pluralism in Feyerabend’s work in the 1970s and beyond was the principle of proliferation: “invent and elaborate theories which are inconsistent with the accepted point of view, even if the latter should happen to be highly confirmed and generally accepted.” In *Against Method* he retained his old idea that proliferation increased empirical content, and added the idea that proliferation was “also an essential part of a humanitarian outlook.” [Feyerabend, 1975a, pp. 26–27] It is important to recognize that the main point of “anything goes” was the methodological freedom that allowed the challenging of dominant modes of thinking by any plausible method:

...the first step in our criticism of customary concepts and customary reactions is to step outside the circle and either to invent a new conceptual system, for example a new theory, that clashes with the most carefully established observational results and confounds the most plausible theoretical principles, or to import such a system from outside science, from religion, from mythology, from the ideas of incompetents, or the ramblings of madmen. [Ibid., p. 68]

All this is well-known to anyone familiar with Feyerabend’s major works. What I may usefully add is the observation that similar thoughts



were already expressed in Feyerabend's response to Kuhn in 1961/2. Along with Feyerabend's own early work in the philosophy of modern physics, his engagement with Kuhn's work gave Feyerabend an important occasion for developing and articulating his thoughts on proliferation: "The more I think and the more I write, the more reasons I find why scientists should consider alternatives at any time, *and not only in a crisis.*" [Hoyningen-Huene, 1995, p. 377]

The Co-existence of Monism and Pluralism

So far I have recounted Feyerabend's critique of Kuhn without indicating whether I agreed with it. Let me now come to the task of evaluating Feyerabend's critique and also assessing the defence that Kuhn gave, or could have given. In the course of this assessment, I arrive at a realization that Feyerabend's pluralism allowed a kind of monism, and that Kuhn's monism required a degree of pluralism. In other words, the clash between Feyerabendian pluralism and Kuhnian monism was not as simple and stark as presented by Feyerabend in his critique of Kuhn.

Let us start by considering a quandary for Feyerabendian pluralism: what should a pluralist think about communities that choose monism as their credo or their mode of operation? A preliminary note, before we tackle the question directly: if we examine Feyerabend's pluralism articulated in the mid-1970s, we find that he was by then thinking more and more in terms of traditions, while in his discussions with Kuhn he had spoken mostly about theories (rather than buying too much into Kuhn's talk of paradigms). Feyerabend's principle of proliferation can and should be extended into an advocacy of the cultivation of multiple epistemic traditions, and respect for them. For example, he famously admired the communist regime in China for restoring legitimacy and authority to traditional Chinese medicine so that it could co-exist as equals with the medical tradition imported from the West [Feyerabend, 1975a, pp. 50–51, 220, 305–306; Feyerabend, 1978, pp. 88, 102–105].

The consideration of traditions, like that of paradigms, invites the consideration of communities. Any pluralism worth its name would cultivate respect for different communities distinct from our own. As Rory Kent [Kent, 2024] stresses, Feyerabend's later pluralism had at its core an advocacy for the autonomy of diverse communities who maintain their own traditions. His earlier theory-focused and methodology-focused pluralism can easily be subsumed under this pluralism of traditions and communities, because theories are developed, and methodologies are used, by communities of people following some sort of tradition. Here an individual may be taken as a limiting case of a small community, inevitably forged within a larger one.



Now, it cannot be denied that human communities are often strongly monist, believing themselves to be superior to other communities, if not the practitioners of the only correct way of knowing and living. This may simply be a fact of social psychology, at least in the stage of development that we humans have managed to reach so far, or possibly it is something deeply rooted in “human nature”. Whatever the case may be, a pluralist society does need to deal with monist sub-sections of itself. If we take a broad view on science in its history, we observe various communities of scientists forming, flourishing and declining over time. Most of these scientific communities have been monist in their outlook, and often battled each other for supremacy. In fact, modern scientists are notoriously monist, perhaps nearly as much as religious fundamentalists are. This is not about whether scientists’ beliefs are more correct than others’, but about how they regard other (i.e., nonscientific) sets of beliefs or ways of forming beliefs.

I cannot see an easy argument based on Feyerabend’s pluralism that would forbid communities to be monist. The Feyerabend of 1961/2, the writer of those passionate letters to Kuhn, did have such an argument, based on his Popperian insistence on universal methodology aimed at the increase of empirical content. The Feyerabend of 1975, the author of *Against Method*, could no longer avail himself to that argument, because he would have had to ask himself: why is it always good to increase empirical content? What if there is a culture that values other things more? At least on the surface, it seems that Feyerabend’s own position landed him in a place not so far from Kuhn. Feyerabendian pluralism must allow each community to decide to be monist, or not. And if most scientific communities will opt for monism, then we have a picture of science that is not so different from Kuhn’s. The only difference would seem to be that Feyerabend preferred to have less of the Kuhnian “normal” state of science in which only one paradigm exists in a given field and the whole community is in agreement. But that comes down to a matter of degrees, since Kuhn of course allowed that there were periods of extraordinary science, with multiple paradigms practiced by distinct sub-communities, each monist in their outlook. And Kuhn also allowed that multiple paradigms within a field may survive in the long run, if they go their separate ways as sub-disciplines, in a process that he compared to biological speciation that produces the “tree of life” with ever-increasing number of branches. So the picture of science given by Feyerabendian pluralism and Kuhnian monism may end up looking not so different from each other after all.

Now let us consider the situation from a Kuhnian starting-point. As Kuhn was at pains to emphasize against his critics, his picture of normal science was not one of dogmatic *stagnation*, but dynamic progress through a single-minded pursuit of knowledge. He extolled the twofold progressivist virtue of monistic normal science: it delivered a great deal



of detailed knowledge framed by the ruling paradigm, and eventually it also resulted in large-scale innovations by precipitating paradigm-shifts. In other words, Kuhn's monism was always oriented toward innovation and progress. Feyerabend stressed that Popperian falsificationism was not incompatible with Kuhn's view of scientific development [Hoyningen-Huene, 2006, p. 628]. Popper, Feyerabend, Lakatos and Kuhn were all agreed that progress was a distinct and positive feature of science.

The difficulty for Kuhn in this context, however, is that Kuhnian monism cannot give the whole picture of scientific progress. This is because innovation requires plurality, at least at some point in the developmental process. This point emerged in various ways in the debates between Kuhn and others. (1) Kuhn's own picture of scientific development requires a contention between competing paradigms in the "extraordinary" phase of science, in order to allow a scientific revolution to happen. Any realistic revolutionary change would have to involve a non-trivial period during which the old regime is competing with the new one. (2) The Kuhnian process of revolution does also require that a new paradigm should be able to arise – but from where? As Feyerabend put the question: "Now if normal science is *de facto* as monolithic as Kuhn makes it out to be, then where do the competing theories come from?" [Feyerabend, 1970, p. 206] I think Feyerabend's answer was that the natural method for this was to maintain a bank of diverse ideas and approaches, from which apt new solutions to difficult problems could emerge. (3) As discussed above, Feyerabend argued that the generation of crisis, which is what creates the need for the new paradigm in Kuhn's view, demanded the existence of competing paradigms already. This is because he thought that a genuine test of a theory, the kind that poses a real threat of falsification, often needed to be launched on the basis of a competing theory. (4) Imre Lakatos [Lakatos, 1970] went even further than Feyerabend, arguing that empirical testing was always comparative between competing theories. According to Lakatos there is no absolute refutation or confirmation, but only a relative judgment of how well different theories do in accounting for a given body of empirical evidence – or better, how well competing research programmes do in eliciting and handling a continual stream of new observations. If that is the case, no theory-testing can occur in a truly monist situation. There must at least be competing versions of a theory in question even within a paradigm, for meaningful testing to occur.

All in all, it seems that normal science, even as Kuhn himself intended it, is a monist enterprise that can only be sustained in a broader pluralistic setup. That is to say, starting from Kuhn's picture, too, we come to the same conclusion as before: there is not such a clear gulf between Kuhnian monism and Feyerabendian pluralism.



Pluralism Beyond Conflict

Now I will try to build on the insights gained in the last two sections, to craft my own proposal for a kind of pluralism concerning science that is compatible with the best aspects of both Feyerabendian and Kuhnian philosophies of science. It will be useful to take the framing of pluralism from my earlier work [Chang, 2012, ch. 5], in which I distinguish “tolerant pluralism” and “interactive pluralism”, and consider both in terms of cultivating multiple “systems of practice” in a given domain. Tolerant pluralism consists in allowing multiple systems of practice to exist and flourish, so that each can achieve what it is good at doing and we can collectively enjoy the benefits arising from all of the systems. Interactive pluralism additionally seeks to reap the benefits that can come from different systems of practice interacting with each other (through competition, co-optation and integration).

Tolerant pluralism is compatible with monism present within each system of practice. Benefits of toleration are not negated by monism, even of a dogmatic kind, as long as no system of practice is allowed to suppress or eliminate other systems. Now, making tolerant pluralism fully compatible with Kuhnian thinking does require a renunciation of Kuhn’s view that in normal science the dominant paradigm does and should enjoy a monopolistic allegiance from all serious scientists in a field. But I cannot see why Kuhn needed to insist on this “paradigm monopoly” thesis. Tolerant pluralism does not interfere with the necessity and effectiveness of paradigm-based research within each system. All the benefits of monist (even monomaniacal) focus that Kuhnian normal science brings can be enjoyed within each system of practice, as long as no system of practice actively interferes with others. A community of “normal” scientists can function perfectly as Kuhn intended under an overall pluralist regime. What it needs is protection and autonomy, not dominance over a whole field of science. A tolerant-pluralist field of science can easily avoid the pitfalls shown in the work of the Pre-Socratic philosophers as Kuhn saw them, wasting all of their energy in fruitless disputes with each other. Each school can focus on pursuing its agenda in the way it deems best, rather than spending its effort in arguing with other schools. The main point here is that tolerant pluralism can accommodate monism, as long as the monists are prevented from destroying the overall pluralist organization of society. And of course the distribution of resources will need to be determined. But we know how to make such decisions, imperfectly yet reasonably, in a democracy with competing interests.

This will also be a convenient place to admit clearly that Kuhn’s historical accounts leave something to be desired. He downplayed the plurality that has actually been historically present in science. Among



the various examples brought up by his critics, the long-running competition between particle-optics and wave-optics comes to mind. Each of these two paradigms produced a significant amount of scientific knowledge, and they never quite entered into the Kuhnian pattern of one paradigm completely dominating the field. Rather, after a long period of competition during which proponents on each side held to their system in a monist way, they both met their demise at the hands of the new quantum-mechanical conception of wave-particle duality. I have discussed a similar yet more complex pattern of competition between five systems of atomic-molecular chemistry in the 19th century [Chang, 2012, ch. 3]. Examples can easily be multiplied. Kuhn's response on this point was disappointing. The long section of "Reflections on My Critics" that is promisingly titled "Normal science: its retrieval from history" [Kuhn, 1970c, pp. 249–259] hardly touched upon this crucial descriptive point.

So much for tolerant pluralism. What about interactive pluralism, which I consider the higher form of pluralism that we should aspire to reach? I think Feyerabend's early debates with Kuhn can give us some useful pointers here, perhaps better than his later and more flamboyant arguments can. When we consider interactive pluralism, I think the limitations of Kuhn's thought start to show themselves and some of Feyerabend's ideas reveal their true promise. Take Feyerabend's critical discussion of Kuhn's view of the demise of classical physics. He pointed out that in the middle of the 19th century physics had "at least three different and mutually incompatible paradigms": mechanics, thermodynamics, and electrodynamics. Anticipating Kuhn's view that these essentially constituted non-interacting and separate sub-fields within physics, Feyerabend argued: "Now these different paradigms were far from 'quasi-independent'. Quite the contrary, it was their *active interaction* which brought about the downfall of classical physics." For example, it was the tension between Maxwellian electrodynamics and Newtonian mechanics that gave rise to the special theory of relativity. [Feyerabend, 1970, pp. 207–208]

The clue that we can take from Feyerabend here is that what we require for true interactive pluralism is for each system of practice to retain autonomy but eschew dogmatic monism, allowing for productive interaction. Even if practitioners believe that their own system is superior, they must not believe it in such a way as to make them regard other systems as not worth interacting with. It is a limitation of Feyerabend's pluralism that its main focus was on critical, even hostile, interaction. But in Feyerabend's work we can also find various subtle clues for more cooperative interactions between systems. His discussion of Galileo's defence of Copernicanism showed clear awareness that the new astronomy and physics needed to be grafted onto the prevailing Aristotelian physics and metaphysics [Feyerabend, 1975a, chs. 5–9]. He advocated medical pluralism with the possibility of syncretism in mind. And listening to the "ramblings of madmen" was about co-opting some ideas to help



our own systems, rather than the wholesale adoption of the madmen's way of thinking and living.

In closing, we must address the question of purpose. What was pluralism intended to achieve, in Feyerabend's view? It is useful to recall how Feyerabend ended his playful and disdainful parallel between normal science and organized crime:

He [Kuhn] has failed to discuss the *aim* of science. Every crook knows that... he wants one thing: money. He also knows that his normal criminal activity is going to give him just this... Money is his aim. What is the aim of the scientist? And, considering this aim, is normal science going to lead up to it? Or are perhaps scientists (and Oxford philosophers) less rational than crooks in that they are 'doing what they are doing' without regard to an aim? [Feyerabend, 1970, p. 201]

It is not true that Kuhn didn't discuss the aims of science, but perhaps his answer (problem-solving ability, most of all) was not satisfactory. What about Feyerabend's own answer? In *Against Method* he seemed to set his sights on no less than general human flourishing – laudable, but ill-defined. Similarly with his “plea for hedonism”, which declared: “the happiness and the full development of an individual human being is now as ever the highest possible value.” [Ibid., pp. 209–210] His philosophy was to contribute to “preventing our species from stagnation” [Ibid., p. 210]. No more helpful were his declarations in the paper of 1975 provocatively titled “How to Defend Society against Science”, ending with “We want to liberate people *so that they can smile.*” [Feyerabend, 1975b, p. 8]

Here again it may be more instructive to go back to the Feyerabend of 1961/2. I have noted the increase of empirical content as an immediate objective articulated by Feyerabend then. But we might ask why the increase of empirical content is so important. Here I want to propose an unconventional answer: Feyerabend was driven by realism, of an empiricist sort (see [Chang, 2021] for further thoughts on Feyerabend's realism). Of course, what he advocated was not the monist kind of realism usually meant by the so-called scientific realists or metaphysical realists, but a realism still based on the idea that science should do its best to learn about reality. Feyerabend saw each theory or paradigm as a vehicle to guide us in our inquiry into reality, a fallible vehicle that may need to be discarded: “I quite agree [with Kuhn]: there is never anything like research without a paradigm. This is the reason why one should always cultivate alternatives in order to be able *both* to drop a falsified theory, *and* to continue *realistic* research into the properties of the universe.” [Hoyningen-Huene, 1995, p. 369]. For Feyerabend pluralism was a realist doctrine, aimed at maximizing our learning by allowing ourselves to investigate reality freely using any and all possible frameworks of inquiry.



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