Feyerabend and the Cranks: A Response to Shaw

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Jamie Shaw (2017) has vigorously engaged both my (mild) criticism of Ian Kidd’s take on Feyerabend’s famous defense of astrology (Pigliucci 2016), and Kidd’s own (mild) concessions in light of such criticism. Here I want to push back a little against Shaw’s approach, with two goals in mind: (i) to identify the limits of Feyerabend’s “hard core pluralism”; and (ii) to elaborate on and defend my take on the feasibility of the science-pseudoscience demarcation project. I will proceed by following the steps in Shaw’s argument, highlighting those that in my opinion are the most important bits, and addressing them to the best of my abilities.

Citing Farrell, Shaw writes: “pluralism is the hard-core of the Feyerabendian philosophical program and it came to permeate all aspects of his thought” (2017, 75). That is surely correct. But a constant danger for pluralism of any sort is that it risks becoming a fairly lazy intellectual position, where anything goes because one is not willing to do the hard work of narrowing down its scope. This is, for instance, what is clearly in evidence in the recently (obviously, posthumously) published book by Feyerabend himself, Philosophy of Nature (2016). The book is a perfect display, writ large, of what the author’s take on astrology was in a more narrow instance: erudite scholarship, sharp criticism of others’ positions, and grand claims about methodological anarchism. The whole thing followed, unfortunately, by very little in the way of positive delivery.

**Why Feyerabend is Wrong About Radical Pluralism**

To be specific, Shaw rightly says that

> two principles comprise Feyerabend’s pluralism: the principles of proliferation and tenacity ... the principle of proliferation [says that] we should “[i]nvent, 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.” ... Proliferation must be complemented by the principle of tenacity which states that we should “select from a number of theories the one that promises to lead to the most fruitful results, and stick to this theory even if the actual difficulties it encounters are considerable (2017, 75).

Neither of these two principles would find much resistance from either scientists or philosophers of science, in part because they are so vague that it is hard to see what, exactly, one would resist. Moreover, there are plenty of instances in the history of science in which precisely what Feyerabend advocates did, in fact, happen. The Copernican revolution (Kuhn 1957), for example, during which Copernicus elaborated a theory that was certainly highly inconsistent with the accepted point of view, with Galileo and others tenaciously keeping it alive for decades, in spite of its obvious difficulties, which were resolved only with Kepler’s adoption of the non-circularity of planetary orbits.
A second example, from biology, is the period of so-called “eclipse” of the Darwinian theory (Bowler 1992), between the end of the 19th and the early parts of the 20th centuries, when criticism of Darwinism from paleontology first, and the new science of genetics later, brought about a proliferation of radically alternative theories, from orthogenesis to saltationism. These theories were tenaciously defended for decades, despite increasing issues confronting them, and which eventually led to their rejection in favor of the so-called Modern Synthesis in evolutionary biology (Huxley 1942/2009).

Many other examples could be plucked from the history of science, and it seems to me that Feyerabend was engaging in much complaining about nothing, since pluralism has always been a hallmark of scientific theorizing. That, after all, is how science makes progress in the first place. What does not seem reasonable, however, is for Feyerabend to think that astrology, or demonology, or homeopathy, are alternative “theories” that ought to be included in the modern pluralist portfolio. Sure, there is always the logical possibility that fringe notions may turn out to contain a kernel of truth, but Feyerabend does not provide us with an iota of reason for why we should keep clearly discredited ones such as those just mentioned around as potentially viable alternatives to investigate, particularly when there is only so much time, money, and resources that go into the scientific enterprise in the first place.

“If we were to abandon theories the moment they came into difficulties,” Shaw continues (2017, 76), “we would have abandoned many of the most successful theories throughout the history of science.” Notice the conditional: turns out, historically, that scientists often did no such thing. Not even Popper (1934/1959) at his most strictly falsificationist ever advocated such a stance.

Feyerabend’s mature view of tenacity is exceptionally radical in two ways. Firstly, it has no conditions for acceptance; any theory can be held tenaciously. ... Even theories that have blatant internal contradictions or seem to conflict with facts can be, and often are, developed into useful research programs ... The principle of tenacity does not, of course, commit us to indefinitely pursuing every line of research we inquire about but simply that it is always perfectly rational to continue developing ideas despite their extant problems (2017, 77).

I take it Shaw (and Feyerabend) and I subscribe to different concepts of what counts as “perfectly rational.” If there are no conditions for acceptance (or rejection) of a theory, then how, exactly, does the principle of tenacity not commit us to indefinitely pursuing every line of research? Who, and on what grounds, makes the decision to stop being tenacious? It seems that Shaw and Feyerabend simply want their cake and eat it too. As for the statement that theories affected by blatant internal or factual contradictions are “often” developed into useful research programs, it is curious that Shaw does not provide us with a single example. Feyerabend is awfully vague about this point as well.
Shaw then goes on to state that “[a]lternatives will be more efficient the more radically they differ from the point of view to be investigated. ... what is ‘non-scientific’ one day is ‘scientific’ the next and the transition between the two requires being placed within scientific debates.” But hold on. Why, exactly, should such a counterintuitive relation between radicality and efficiency hold? What historical evidence has been marshaled for that being the case? Indeed, what criterion of efficiency is being deployed here? And yes, sometimes what may appear non scientific may turn out to be so later on (e.g., the idea of continental drift in geology: Frankel 1979). But I never proposed that the science / quasi-science / pseudoscience territory is demarcated a-temporally. Rather, it is a territory marked by fluid boundaries that evolve because they reflect the understanding of the world on then part of the scientific community at any given moment. That said, my colleague Maarten Boudry and I (2013) have observed that there don’t seem to be cases where a notion has been seriously entertained by the scientific community, then relegated to the pseudoscience bin, and later on somehow re-emerged to find new life and success. We refer to this, informally, as the “pseudoscience black hole”: once in, you never get out. It is true for astrology just as much as for demonology and homeopathy, and we have yet to find exceptions.

**Once More on Demarcation**

Shaw then proceeds to consider my proposal for the science-pseudoscience demarcation problem. Correctly noting that I do not think classical attempts based on small sets of necessary and jointly sufficient conditions could possibly work, he acknowledges that my proposal is that of considering both science and pseudoscience as Wittgensteinian family resemblance concepts, with no sharp boundaries and no set of criteria that are instantiated in all cases. To which I would add the above reminder that I am also explicit about the fact that the science-pseudoscience territory is temporally fluid, to a point (Pigliucci 2013).

In that paper, I propose two axes (not really “criteria”) that may help us map said territory: one that has to do with the internal coherence and theoretical sophistication of a given theory, the second that captures the empirical content of the theory. So for instance, the Standard Model in physics scores high on both counts, as does modern evolutionary theory. Accordingly, they are (currently) considered very solid sciences. At the opposite extreme, astrology and intelligent design creationism are both empirically and theoretically poor, so they are classed as obvious instances of pseudosciences (again, for now). The interesting stuff lies in the middle, e.g., fields that are high on theoretical but low on empirical content (economics) or vice versa (psychology). The borderlands also include fields of inquiry that are usually considered pseudoscientific, such as parapsychology, but are still sufficiently interesting—either theoretically or empirically—to warrant further study.

Shaw acknowledges all this and yet says that “theories that contain low degrees of empirical support (or even conflict with known facts) or are theoretically confused are perfectly pursuit-worthy on Feyerabend’s account,” and that “Pigliucci’s criteria fail to provide reasonable grounds to prevent the consideration of ‘pseudosciences’” (2017, 79). Well, but my criteria are not those of Feyerabend, so the fact that we disagree may be taken as an indictment of my views just as much as of his, it all depends on which position one finds
more plausible. And the latter part of the quote misunderstands what my criteria were developed to do: not to prescriptively separate science from pseudoscience, but rather to provide a compass of sorts to navigate the territory and its complex, temporally fluid, boundaries.

Demarcation criteria affect people with different intellectual backgrounds. They affect funding distribution policies, taxation policies, those who benefit or are harmed by the creation (or lack thereof) of particular pieces of scientific knowledge, and so on. This is far beyond the domain of scientists or philosophers of science who provide, at best, one perspective on demarcation. ... If scientists are forced to conform to certain views because their education does not provide viable alternatives, if peer review is so conservative that it causes long-term conformity, and so on, then those intuitions aren’t worth taking seriously (2017, 79).

The first bit seems to me to confuse epistemic assessment, which is definitely within the purview of the scientist, with other, surely important, aspects of social discourse. I have never claimed that scientists should be in charge of taxation policies, or more broadly of decisions concerning the broader societal impact of scientific research. Indeed, I most certainly oppose such a stance. Take the issue of climate change, for instance (Bennett 2016). It seems eminently sensible, in that case, to leave the science to the scientists—because they are the ones who are qualified to carrying it on, just like dentists are qualified to take care of teeth—while the much broader and more complex question of how to deal with climate change requires that we call to the high table a number of other actors, including but not limited to economists, various types of technologists, sociologists and even ethicists.

As for the series of conditionals in the second bit quoted from Shaw above, there are far too many unsubstantiated ones. Is it the case that scientists are “forced” to conform because of their education? Is it true that peer review is “too conservative”? On what grounds, according to what criteria? A lot of heavy duty legwork needs to be done to establish those points, work that is obviously beyond the scope of Shaw’s commentary, but that Feyerabend himself simply never did. He was content to throw the bomb in the crowd and watch the ensuing chaos from the outside.

So, was Feyerabend Right in “Defending” Astrology?

The last part of Shaw’s commentary returns to the question that began this whole series of interesting, and I hope useful, exchanges: was Feyerabend right in mounting his peculiar defense of astrology?

“Feyerabend defended the epistemic integrity of some practitioners of astrology because he was practicing the pluralism he preached and decided to defend views that were dismissed or ostracized from the philosophy of science. In other words, Feyerabend was proliferating” (2017, 80). Indeed, but epistemic integrity is a necessary and yet not sufficient condition for being taken seriously as a scientific research program. It is truly astounding that people still
think astrology is worth defending, and I’m not talking just about the horoscope variety, as Shaw suggests. While it was certainly the case that some of the signatories of the infamous anti-astrology manifesto that so railed Feyerabend did not due their homework on astrology, plenty of others have. Among them Carl Sagan, who famously did not sign the manifesto, precisely for the reasons Feyerabend thought it was a bad move, but who nonetheless was a harsh critic of much pseudoscience, including astrology.

Shaw writes that “a view one may have may have reason to reject may still be true. To deny this is to assume our own infallibility. ... A problematic view ‘may and very commonly does, contain a portion of truth.” (2017, 80) Well, no, definitely not. The accusation of infallibility against critics of pseudoscience is ludicrous. If taken seriously that would mean that every time one has very strong theoretical or empirical reasons to reject a given notion (until, and if, proven wrong) one ipso facto thinks of himself as infallible. Yes, a problematic view may turn out to contain a portion of truth, but “very commonly does”? This is another example of Shaw using a page from Feyerabend’s playbook, making grand statements that are accompanied by absolutely no evidence. Why should philosophers of science take them seriously?

“Because its critics are being arrogant, defending a ‘pro-astrology’ perspective is necessary to combat this vice,” continues Shaw (2017, 82). But why is it a good idea to fight vice with vice? Why is it not enough to embarrass some of the signatories of the anti-astrology manifesto, showing to the public that they did not know what they were talking about? Why is it that one has to take the next step and defend the possibility that there is still value in astrology, when one patently does not actually believe it, as Feyerabend did not?

Complaining about my objection that Feyerabend’s attitude is positively dangerous, because it facilitates the acceptance by the public of notions that endanger safety, Shaw replies:”

Feyerabend never, to my knowledge, discusses climate change, anti-vaccination movements, or AIDS denialism; these (mostly) became issues after Feyerabend’s death. Furthermore, there is no legitimate inference from Feyerabend’s pluralism to defending these topics in a direct way. ... Pigliucci cannot ascribe any of these particular consequences as emanating from Feyerabend (2017, 83-84).

Except, of course, that I do no such thing. I’m perfectly aware that those issues became prominent after Feyerabend’s death. But it would be naive to believe that there is no connection to be made here. Indeed, the infamous “science wars” of the ‘90s (Gross and Levitt 1994), pitting strongly postmodernist philosophers and sociologists on one hand against scientists and philosophers of science on the other, had a pretty direct connection with Feyerabend’s work, which was, predictably, ailed by the first group and condemned by the second one.

Finally, Shaw takes up what Feyerabend, Kidd, and myself have in common: a strong suspicion for what nowadays is referred to as “scientism,” the exponents of which are those
who Shaw labels “the cranks.” Citing Feyerabend, Shaw writes: “The crank usually is content with defending the point of view in its original, undeveloped, metaphysical form, and he is not prepared to test its usefulness in all those cases which seem to favor the opponent, or even admit that there exists a problem” (2017, 85). I have certainly encountered such types, both among scientists and among so-called skeptics. They are not serving the interests of science, critical thinking, or society. So Shaw is correct when he states that “it is clear that there is a commonality between Pigliucci, Kidd, and Feyerabend: their disdain for the cranks!” Indeed. But, contra, Feyerabend, I do not think that the way to do it is to take an anti-rationalist stance about the value of pseudoscience. It is both counterproductive (Feyerabend was famously labeled “the Salvador Dali of academic philosophy, and currently the worst enemy of science” by two physicists in the prestigious journal Nature: Theocharis and Psimopoulos 1987), and simply not the virtuous thing to do.

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References


