

Feyerabend, experts, and dilettantes

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Abstract: Paul Feyerabend's 1970 article "Experts in a Free Society" tries to make the case that scientific experts can only be tolerated if they are *dilettantes*. He uses Galileo, Newton and Kepler as examples of great scientists whose writing is nothing like that of contemporary "experts", these latter being represented by the authors of the well-known book *Human Sexual Response*, Bill Masters and Virginia Johnson. He goes on to argue against the idea that the Scientific Revolution represented the triumph of empiricism.

I take issue with the way Feyerabend represents Galileo as implacably opposed to empiricism, with his supposition that good science requires a particular personality, and with the way in which he represents the work of Masters and Johnson.

Keywords: Feyerabend, Experts, Dilettantes and amateurs, Empiricism, Scientific Revolution.

Introduction

Paul Feyerabend is now best known for his book *Against Method* (1975), in which he tried to alter our perception of science and the lessons we should learn from it. On the basis of an historical case-study of Galileo, he argued that there is no such thing as scientific method, since great scientists are methodological opportunists who use any strategies they can in order to promote their preferred theories. He didn't talk much about scientific experts in that book, but in other work he published during that decade he had quite a lot to say about them, and what he said bears even more investigation nowadays, when we rely on experts for their views on climate-change, COVID-19, vaccination, etc.

Most of what Feyerabend said about experts fell somewhere between the sceptical and the scathing. So, for example, he took delight in pointing out that "obstinate and conceited" experts can be outsmarted by laypeople (notably, by lawyers), that experts often disagree with one another, that their judgements can legitimately be challenged by non-experts, that they can legitimately be overruled by political interference, and he was happy to refer to them as "know-it-alls".

Feyerabend also rarely said anything positive about the people he thought of as scientific experts. However, going somewhat against his popular reputa-

tion, he was capable of saying very positive things about science. How could this be? An answer lies in the paper I will discuss.

The context of Feyerabend's career

By the end of the 1960s, Paul Feyerabend's star had risen very high. He had made his mark with important papers published in the late 1950s, while he was still associated with the Popperian school, defending scientific realist approaches to scientific theory against positivist approaches. But in the early 1960s he had broken with Popper, publishing papers which were critical of the hypothetico-deductivist approach to methodology which was common to Popperians and to logical empiricists. Together with Thomas Kuhn, he had also been at the forefront of a new *historical* approach to philosophy of science, thus moving away from the logical approaches that were, again, common to Popper and the logical empiricists. His papers on Niels Bohr had been critical of Popper, and although he wasn't able to present his paper to the International Colloquium in the Philosophy of Science at Bedford College, London, in 1965,¹ he did present it to Popper's LSE seminar in 1967, and when it was published in one of the resulting volumes, "Consolations for the Specialist" (published in *Criticism and the Growth of Knowledge*, 1970) was very critical of Popper, and Kuhn, and Imre Lakatos. At the end of that paper, Feyerabend concluded that no proposed method for rationalizing science can be applied, that "what remains are aesthetic judgements, judgements of taste, and our own subjective wishes" (1970b/1981: 160), and further that "the choice between theories which are sufficiently general to provide us with a comprehensive world view [...] may become a matter of taste" (1970b/1981: 160). In that same year, in the original, essay version of *Against Method* (1970d) Feyerabend had argued that there is no scientific method.

He expanded on this in a shorter paper entitled "Experts in a Free Society" (1970c/1999), and brought it to bear on the issue of expert scientific authority, which clearly bothered him. What follows is a critical examination of some themes from this paper, although I'm not going to cover those parts of it that make Feyerabend's case "against method", since that has been covered extensively elsewhere.

"Experts in a Free Society" (1970)

"Experts in a Free Society" was published, not in an academic journal, but in a magazine. The magazine in question, *The Critic*, was produced by the

¹ For the full story, see Collodel *et al.* (2020: 402n).

Thomas More Association, a non-profit organisation based in Illinois. It's hard to find any information about this organisation or its magazine online, since there now exists a well-known (and very web-assertive) Thomas More *Society*, a conservative Roman Catholic public-interest law firm based in Chicago, which was founded much later. In 1962, though, the *Catholic Standard and Times* reported that "A magazine founded and edited by laymen has won for the second consecutive year the Catholic Press Association's general excellence award in competition among general interest magazines. The award went to *The Critic*, self-described as a 'magazine of Christian culture.' It is published six times a year by the Thomas More Association of Chicago" (The Catholic News Archive, online). *The Critic* was published from 1948 to 1981, and then from 1985 to 1990.

Feyerabend prefaced this article with a slogan ("Jargon, jargon everywhere. And all of it doth stink"), a warning (that the paper will sound a little harsh, and unjust), and a "confession" (no doubt with the context of publication in mind) explaining that it was written "in a fit of anger and self-righteousness caused by what I thought were certain disastrous developments in the sciences". The key lies in his view that experts are the problem with science, rather than its lifeblood (or even a desirable feature of it at all):

I think very highly of science, but I think very little of experts, although experts form about 95% or more of science today. It is my belief that science was advanced, and is still being advanced by *dilettantes* and that experts are liable to bring it to a standstill (1970c/1999: 112).

This I take to be the *main* thesis of his paper. I'll give a brief exposition of some themes from "Experts in a Free Society", and then address some of the issues they raise.

Feyerabend's attitude to experts

Feyerabend begins, before explaining what experts *are*, by setting out his feelings about them. If, he argues, our attitude towards giving people the vote is as strict as it is today, then we should *not* give experts the vote, because of their immaturity. (In the US at that point, there was public discussion of whether eighteen-year-olds should be able to vote. The Twenty-sixth Amendment, lowering the voting age from twenty-one to eighteen, was ratified only in 1971). Rather, we must wait until they grow up, become mature and responsible, and this means becoming what Feyerabend calls a *dilettante*. (We will soon investigate what he means by this term, but for the moment we can understand it as meaning a non-specialist). In a *free* society, though (I presume that this is the contrast he's mak-

ing) experts will certainly have the vote; they will certainly be listened to as every other citizen will be listened to, but they will receive none of the special powers which they would so dearly love to possess. Laymen will look after their affairs and will make the decisions which must be made if we want to apply science to society. Laymen will control science – and no harm will come of it.

What, then, *are* experts? Experts are people who have decided to achieve excellence in a narrow field “at the expense of a balanced development” (1970c/1999: 113). They subject themselves to standards which restrict them in many ways, conduct their working lives in accordance with these standards, and divide their lives into separate domains. This has bad consequences: special subjects are voided of ingredients which make a human life beautiful and worth living; these ingredients are impoverished, too; emotions become crude and thoughtless, just as thoughts become cold and inhumane. Professionalism takes over.

Feyerabend gives us examples of this professionalism, but prefaces these by discussing some examples of science writing that *don't* exhibit it. His examples are drawn from Galileo, Kepler, and Newton. I will focus on Galileo and Newton, the two figures most important for what follows (as well as for *Against Method*, in Galileo's case, of course).

Galileo's acquaintance with the telescope

Feyerabend quotes two passages from *Sidereus Nuncius* (*The Starry Messenger*), written in 1610, the longer one being:

About ten months ago a report reached my ears that a certain Dutchman had constructed a spyglass by means of which visible objects, though very distant from the eye of the observer, were distinctly seen as if nearby. Of this truly remarkable effect several experiences were related, to which some persons gave credence while others denied them. A few days later the report was confirmed to me in a letter from a noble Frenchman at Paris, Jacques Badovere, which caused me to apply myself wholeheartedly to inquire into the means by which I might arrive at the invention of a similar instrument (Drake 1957, ed.: 28-29).

What we have here, Feyerabend explains, is “a personal story, a very charming personal story which slowly leads us to the discoveries, and these are reported in the same clear, concrete, colourful way” (1970c/1999: 114).

Newton's experimental preparation

Feyerabend is also impressed by a passage from the beginning of Isaac Newton's first paper on colours in which Newton (who, as we shall see, Feyerabend

usually has no time for), describes procuring a glass prism, preparing his chamber for the experiment, and viewing “the vivid and intense colours produced” when a beam of light hits the prism and is refracted onto the opposite wall:

[I]n the beginning of the year of 1666 [...] I procured me a triangular glass prism, to try therewith the celebrated phenomena of colours. And in order thereto having darkened my chamber and made a small hole in my window shuts, to let in a convenient quantity of the sun’s light, I placed my prism at his entrance, that it might be thereby refracted to the opposite wall. It was at first a pleasing divertissement to view the vivid and intense colours produced thereby; but after a while applying myself to consider them more circumspectly, I became surprised to see them in an *oblong* form... (“The New Theory about Light and Colours”, 1672, as reprinted in Thayer, 1953, ed.: 68).

Feyerabend sums up his three examples by asking us to remember that

all these reports are about cold, objective, “inhuman”, *inanimate* nature; they are about stars, prisms, lenses, the moon; and yet they are described in a most lively and fascinating manner, communicating to the reader an interest and an excitement which the discoverer felt when first venturing into strange new worlds (1970c/1999: 115).

Professional science writing: Masters and Johnson

We then get Feyerabend’s two examples of *bad* science writing, or at least bad writing: the well-known book *Human Sexual Response*, by William Masters and Virginia Johnson, and a pamphlet containing the report of an *ad hoc* committee examining allegations of police brutality on the Berkeley campus during 1968-1969. The latter would have been of personal interest to Feyerabend, of course, since he had a role in the student unrest. But I can’t find any copy of it (online or elsewhere), so will discuss the former.

Human Sexual Response was published in 1966, and had become something of a *cause célèbre*. Since the book is, as Feyerabend puts it, “about men rather than about stones or prisms”, one would expect a beginning even more lively and interesting than that of Galileo, Kepler, or Newton. But if we turn to it we find things like this...

“In view of the pervicacious gonadal urge in human beings, it is not a little curious that science develops its sole timidity about the pivotal point of the physiology of sex. Perhaps this avoidance...” and so on. This is no longer human speech. This is the language of the expert (1970c/1999: 115).²

² The horrible English word “pervicacious” means obstinate or stubborn. The “gonads” are the primary reproductive organs: the testes in the male, and the ovaries in the female.

I won't regale you with the other things Feyerabend quotes, but simply relate his observations. First, the *subject*, the human person, has left the picture. Second, relatively simple human matters are put in a more wordy and more complicated way than those matters demand. Third, "irrelevant technical terms intrude and fill the sentences with antediluvian barks, grunts, squeaks, belches" (1970c/1999: 115). The effect is that

[a] wall is erected between the writer and his readers not because of some lack of knowledge, not because one does not know who the reader is, but in order to make utterances conform to some curious professional ideal of objectivity. And this ugly and inarticulate idiom turns up everywhere and takes over the function of the most simple and straightforward description (1970c/1999: 115).

Feyerabend's complaints don't stop there. Instead of saying that when masturbating, a woman will stop only when she gets tired, Masters and Johnson say: "usually physical exhaustion alone terminates such an active masturbatory session" (1966: 65). And instead of saying that the man should *ask* the woman (whom Feyerabend himself calls "the female") what she wants or does not want rather than try to guess it on his own, Masters and Johnson say: "The male will be infinitely more effective if he encourages vocalisation on her part" (1966: 66). We shouldn't pretend that this is a matter of their wanting to be more precise (their expression "*infinitely* more effective" precludes that), and neither should we excuse it by claiming that they want to address their fellow professionals rather than the general public and so have to use a special lingo in order to make themselves understood: "[W]e are not dealing with the structure of organs, or with special physiological processes which might have a special name in medicine, but with an ordinary affair such as *asking*" (1970c/1999: 116). Feyerabend concludes:

Galileo and Newton could do without a special lingo although the physics of their time was highly specialised and contained many technical terms. They could do without a special lingo because they wanted to start afresh. Masters and Johnson find themselves in the same position, but they cannot speak straight any more, their linguistic talents and sensibilities have been distorted to such an extent that one asks oneself whether they will ever be able to write normal English again (1970c/1999: 116).

And after his discussion of the Berkeley pamphlet Feyerabend concludes that in these both texts:

a single and rather impoverished idiom takes over all functions and is used under all circumstances. Does one want to insist that the thought that hides behind this ugly and inhuman exterior (emphasis upon abstract processes such as "communication" instead of living people) has remained nimble and humane? (1970c/1999: 117).

Free men (dilettantes) vs. slaves, and the power of experts

According to Feyerabend, it was already realised by Aristotle that being an expert was a *predicament*, and not a matter of pride (1970c/1999: 117). He summarises Aristotle's picture of "the free man" as a man having a sense of balance and perspective, being well-informed in different aspects of human culture, having emotions, and having concrete experience of what goes on outside the limits of his own subject. And Feyerabend's way of putting this is: you can be a free man, you can achieve and yet retain the dignity, the appearance, the speech of a free man only if you are a *dilettante* (1970c/1999: 117-118).

Feyerabend asks us to remember "to what extent the academic profession makes slaves of its members" (1970c/1999: 118). He finds support for this in the sociologist Robert Merton, who had recently characterised science as "a system of institutionalised vigilance, involving competitive co-operation" (1969: 220). Such a tough-minded or "realistic" picture of contemporary science, then, means that "wandering minstrels who try to bewitch the onlookers by praising the beauties of science, the joys of discovery, the essentially human character of the search for knowledge and truth [...] are singing about a time that has long gone by [i.e., the heroic time of the 'Scientific Revolution']" (1970c/1999: 118). (I imagine that Feyerabend had all sorts of people in mind here, including Popper and Lakatos).

Feyerabend sums up: experts today are excellent, useful, irreplaceable, but mostly nasty, competitive, ungenerous slaves, slaves both in mentality, speech, and in social position (1970c/1999: 118). But these inarticulate and slavish expert minds now have power over large parts of society. What arguments lie behind this claim for experts to have such power? (1970c/1999: 119). Feyerabend proposes to investigate, and he does so by pivoting to discuss the character of the Scientific Revolution.

The Scientific Revolution, Galileo, and empiricism

Many people, he claims, still believe that the Scientific Revolution of the 16th and 17th centuries was the result of a "radical" empiricism and think that it occurred only because one man decided to eliminate views not in agreement with observation and reasonable generalisations therefrom. But the Scientific Revolution *wasn't* the result of a radical empiricism. Galileo was suspicious of experience, since experience played a role in the magical tradition which he despised. Alchemy, too, and the art of discovering witches, were firmly empirical. Galileo was suspicious about "[s]uch magical and enthusiastic types of empiricism which exclude thought from a large area of knowledge" (1970c/1999:

120). He even rejected the more sober empiricism of Aristotle (which Feyerabend takes to be the most promising kind).

Feyerabend agrees that the Scientific Revolution was the heroic time of science, but he insists this is because the best scientists were the best *dilettantes*. In this scenario, expert knowledge is produced, but it is produced *by* dilettantes. By “dilettantes”, Feyerabend seems to have meant *non*-specialists, thinkers who did not limit their thought to any single area.

Evaluation of Feyerabend's attitudes and claims

How, then, should we evaluate these attitudes and claims on view in “Experts in a Free Society”?

Feyerabend wasn't really interested in explaining what experts *are*. One might sympathise with his admiration for “Renaissance men”, i.e., polymaths (and I do mean *men* – women hardly figure in Feyerabend's works). But narrowness of excellence will make “experts” of almost every significant contributor to any intellectual field (including Feyerabend himself). At that point one has to ask: do the consequences that Feyerabend supposed really follow? Perhaps Feyerabend's critique is best thought of as a blast at a certain *kind* of expert prominent in the cold war US context: the (CIA-funded) technocrat, keen on presenting humans as machines. That kind of expert is less fashionable or perhaps just more sophisticated nowadays, even in the US, and so one might wonder whether experts *have* grown up, become mature and responsible. Certainly I think one can't just take Feyerabend's case against them and assume that it applies with full force to our current context.

Feyerabend's (early) Galileo

It's true that what Feyerabend quotes from *Sidereus Nuncius* is “a personal story, a very charming personal story which slowly leads us to the discoveries, and these are reported in the same clear, concrete, colourful way” (1970c/1999: 114). But Galileo was in a very particular and unusual context: first, his text is dedicated to a particular person, his patron, the Grand Duke of Tuscany. That demanded something of a personal manner. Second, he obviously felt the need to explain to his reader (and his patron) how he came to know about this remarkable and controversial new instrument, and how one might construct a similar instrument. Patronage wasn't untypical, of course, but the *combined* context is untypical enough that it's unfair to demand that personal stories should accompany or preface all scientific communications, or even to suppose that they could do so.

What about the way in which Galileo writes? Lively and personal writing impresses Feyerabend, of course, and he himself almost always wrote in that way, but it seems to be a pretty superficial criterion for scientific merit. It's true that Galileo and Newton could do without a special lingo in *preambles* to their physical works, but the physics of their time was highly specialised and contained many technical terms. The extent to which they wanted to "start afresh", as Feyerabend supposes, is contestable. They didn't eschew the existing technical terms in mechanics, optics, etc. Feyerabend is impressed by scientists who write in ordinary language. And surely there's lots of bad writing in science texts. But it would be wrong to issue a general prohibition on jargon (i.e., technical terms) in science. Feyerabend's anti-jargon slogan would apply to *all* science, since all sciences (and all scientists, including the ones Feyerabend revered) use *some* technical terms.

Galileo against empiricism?

What about Feyerabend's insistence that Galileo rejected empiricism? There's clearly *something* right about this, but also, something clearly wrong.

No-one reading *Sidereus Nuncius* today could fail to think of it as empiricist in a *broad* sense. Its whole *raison d'être* is to relate several series of observations of heavenly bodies that Galileo claims to have made with his "spyglass" (telescope). So what can Feyerabend have meant (here) when he claimed that Galileo wasn't an empiricist?

To see this, we have to turn to the contexts in which each of them was writing. *Galileo's* context was characterised partly by the empiricism of the Aristotelian tradition but also (Feyerabend thinks) by a kind of "empiricism" associated with magic, alchemy, and witchcraft.

Feyerabend's context was of course one in which forms of empiricism had dominated Anglo-American philosophy generally, and philosophy of science in particular. But the forms against which Feyerabend was reacting were drawn both from recent philosophy and from past philosophies of science. One of the latter was *Newton's* empiricism, which Feyerabend railed against in an article published in the same year as "Experts in a Free Society", namely, "Classical Empiricism" (Feyerabend 1970a/1981). Among the others were the kinds of sense-datum empiricism popular among some of the founders of analytic philosophy (Russell, Moore), *then* among the logical positivists, and *then* among figures who tried to combine these approaches (such as A.J. Ayer). No doubt some of these could be characterised as "radical" empiricisms.

When Feyerabend insisted that Galileo rejected empiricism, he seems to have had both "radical" and "naïve" empiricism in mind. Feyerabend is on

strong ground when understood as claiming that Galileo wasn't a "*radical* empiricist". His methodology doesn't seem to have featured the kind of restrictive "rules" that characterised *Newton's* empiricism (and obviously he wasn't an empiricist as conceived by the tradition of British empiricist *philosophy*, or a sense-datum empiricist). But the idea that the Scientific Revolution was the result of *such* an empiricism is far-fetched, and also difficult to pin on anyone. When Feyerabend speaks of the myth that one man initiated the Scientific Revolution by eliminating views not in agreement with observation he *may* have been thinking of Stillman Drake, whose edition of Galileo's essays he was working from, and who did present Galileo as an empiricist (see, e.g., Drake 1957, ed.: 8, 18). But "*radical*" empiricism is a red herring.

So is "naïve" empiricism, assuming that this is the view that we have to take the unaided deliverances of sense-experience as the only court of scientific appeal. Again I suspect that *no-one* in science or its philosophy would have endorsed an empiricism of *that* kind (not even figures such as Francis Bacon and Ernst Mach, who are sometimes thought of in this way).

When we come to Feyerabend's claim that Galileo rejected the empiricisms of Aristotle and of the magicians, matters are more complicated.

As I just said, Galileo would have rejected the idea that *unaided* human sense-experience provides the sole evidence-basis of science. He did not, however, *reject* the appeals that Aristotelians made to sense-experience. Rather, he answered them. And, as Maurice Finocchiaro has recently pointed out, in a certain respect Galileo used Aristotle's own empiricism against Aristotle, in arguing that "it is more in accordance with Aristotle to philosophize by saying 'the heavens are changeable because so the senses show me' than if you say 'the heavens are unchangeable because theorizing so persuaded Aristotle'" (Finocchiaro 2019: 129-130, quoting Galileo 1997: 104).³

The Witchfinder General's "experience"

What about the magicians, alchemists and witch-hunters to whom Feyerabend adverted, and who did appeal to "experience" (and "experiment")? Was it the kind of experience to which Galileo was appealing, that is, human sense experience (as aided by his new instrument, the telescope)? Let's see. Which of Feyerabend's examples should we consider? The one that's most fun, of course.

In *The Discovery of Witches* (1647), credited to Witchfinder General Matthew Hopkins, the question is put: "From whence proceeded [the Witchfinder's] skill? Was it from his profound learning, or from much reading of

³ For a more balanced view of whether Galileo was an empiricist see, e.g., Hesse (1967).

learned authors concerning that subject?”. And the answer given there, which Feyerabend quotes, is: “From neither or both, *but from experience*, which though it be meanly esteemed of, is yet the surest and safest way to judge by” (1970c/1999: 120, Feyerabend’s emphasis). But this clearly refers to what we might call “hands-on” or “lived” experience, not *perceptual* experience. And this is amply confirmed when we read on, since the questioner asks “Where was this experience gained? and why was it gained by him and not by others?”, to which the reply begins “The Discoverer never travelled far for it, but in March 1644 he had some seven or eight of that horrible sect of Witches living in the Towne where he lived, a Towne in Essex called Maningtree, with divers other adjacent Witches of other towns...”. (The text’s four other mentions of “experience” also clearly refer to lived experience). In claiming that the discovery of witches was “firmly empirical”, insinuating that *philosophical* empiricists would have to acquiesce in it, and supposing that this was the same kind of experience to which Galileo was appealing, Feyerabend is cheating.

Appeals to sense-experience in Sidereus Nuncius

There’s plenty of evidence of Galileo’s own (more sophisticated) empiricism (or empirical stance, we might say) in *Sidereus Nuncius*. That text begins:

Great indeed are the things which in this brief treatise I propose for observation and consideration by all students of nature. I say great, because of the excellence of the subject itself, the entirely unexpected and novel character of these things, and finally *because of the instrument by means of which they have been revealed to our senses*. Surely it is a great thing to increase the numerous host of fixed stars previously visible to the unaided vision (Drake 1957, ed.: 27, emphasis added).

In this way one may learn *with all the certainty of sense evidence* that the moon is not robed in a smooth and polished surface, but is in fact rough and uneven... (Drake 1957, ed.: 28, emphasis added).

Again, it seems to me a matter of no small importance to have ended the dispute about the Milky Way *by making its nature manifest to the very senses as well as to the intellect* (Drake 1957, ed.: 28, emphasis added).

All these facts were discovered and observed by me not many days ago with the aid of a spyglass which I devised (Drake 1957, ed.: 28).

I have observed the nature and the material of the Milky Way. With the aid of the spyglass this has been scrutinized so directly and with such ocular certainty that all

the disputes which have vexed philosophers through so many ages have been resolved, and we are at last free from wordy debates about it (Drake 1957, ed.: 49).⁴

Of course, what Feyerabend was opposing *was* an empiricist myth, a myth according to which the Scientific Revolution occurred simply because people started observing rather than speculating. (Perhaps that's what he meant by calling it "*radical*" empiricism). No doubt this myth featured in the popular consciousness, and perhaps also in certain popularisations of the history of science. But if Feyerabend thought that it had been promulgated by serious works of philosophy, or history of science, he should at least have told us which ones.

Feyerabend and the Masters of Sex

Feyerabend's take on Masters and Johnson is equally bizarre. He misrepresents both what they write, and why they wrote the way they did. Thomas Maier's book *Masters of Sex*, which was subsequently turned into a TV series of the same name, supplies ample evidence that Masters and Johnson were not at all the kind of people Feyerabend imagined. His objections to their work completely fail to recognise its importance, cultural as well as scientific.

What Feyerabend quotes from *Human Sexual Response* (Masters *et al.* 1966: V – the passage I quoted above) is a strange way to begin the *preface* to a book, it's true. Convention demands that prefaces be reader-friendly. But what he quotes are not the words of Masters and Johnson, but of Robert L. Dickinson and Henry H. Pierson, from their article "The Average Sex Life of American Women", published in the *Journal of the American Medical Association* back in 1925 (what Feyerabend quotes is from page 1113).

One might well wonder whether Masters and Johnson themselves wrote like Dickinson and Pierson. Well, despite their book having gone on to be, as Feyerabend himself notes, a best-seller (1970c/1999: 115; Maier 2009: 174, 295, 360), it's true that they did so. Although they kept their distance from the "dry clinical language" of Alfred Kinsey's prior research (Maier 2009: 97), their book was "designed primarily for doctors and residents in need of basic training" (Maier 2009: 151), and published as a medical text. Masters was more inclined to jargon than Johnson (Maier 2009: 106), and wrote in what Maier calls an "obstipated medicalese" (2009: 123). Consequently, their book is full of medical terms ("ovulatory-mucus plug", "ovulatory-type mucoid material", "cervical os", "patulous", "nulligravid cervix", "the parous individual" – all from Masters *et al.* 1966: 115), to the extent that it required a glossary (Maier 2009: 158).

⁴ Note that two of these passages credit us with *sense-certainty* (although this isn't the philosophers' sense of "certainty").

However, despite his acquaintance with their context, Feyerabend fails to register their need to put their case in “language as ‘non-inflammatory’ as possible” (Maier 2009: 157). In the USA at that time, Masters and Johnson just could not have written about sex in ordinary and non-technical terms and had their case taken seriously, or indeed as anything other than pornographic. As Maier says:

Human Sexual Response transformed the public discourse about sex in America, opening a new era of candidness never seen before in the media. Although ridiculed for their turgid prose, Masters and Johnson relied on medical terms and clinical descriptions that didn’t offend readers. They stayed away from vulgar phrases that would have invited censorship (2009: 174).

Or, as Masters himself put it: “You must remember that in publishing this book we were concerned primarily with acceptance – that is the reason that it wasn’t in English to start with” (Maier 2009: 174).

As for his comments on their character (their thinking not being “nimble and humane”), Feyerabend clearly knew nothing about Bill Masters and Virginia Johnson.⁵ He can’t be faulted for not knowing about their sexual relationship (Maier 2009: Ch. 14 onwards). But he didn’t know that neither of them was trained in psychology or psychiatry (Maier 2009: 179). I guess that, pre-internet, we didn’t always have time to research the people whose books we were writing about. But let’s bear in mind that *he* was happy to accuse contemporary scientists of knowing nothing about the research *they* denigrated (into astrology, Chinese medicine, etc.). He was just attacking a target that he himself had stereotyped, and using a double-standard in doing so.

Experts, dilettantes, and slaves

I have no quarrel with what Feyerabend quotes from Aristotle’s discussion of the “free man” (*Politics*, Book VIII, section 2). Indeed I think it may be an important ideal, more widely applicable than Aristotle would have thought.

However, I do want to challenge Feyerabend’s idea that one can be an Aristotelian “free man” only if one is a *dilettante*. This is completely wrong. Aristotle’s ideal is similar to what north Americans call a “well-rounded” person, *not* a “dilettante”, and Aristotle would *not* have accepted the equation of free men and dilettantes, at least not using “dilettante” in its ordinary Italian or English senses. In Italian it means amateur. But Galileo was not an *amateur* scientist!

⁵ By the time he was writing, they were working at an independent not-for-profit research institution they had founded in St. Louis in 1964, originally called the Reproductive Biology Research Foundation.

He'd studied mathematics at the University of Pisa, and obtained professorial positions in Pisa and then Padua, before moving to Florence as Philosopher and Chief Mathematician to the Grand Duke of Tuscany. No-one, at that time, could have been less of an amateur.

In the English sense a dilettante is a person who has a *shallow* interest in some area of culture or knowledge. So the relevant contrast is shallowness vs. depth, not narrowness vs. breadth. An expert, that is, someone who has a *deep* knowledge of something, can quite well be the kind of person who also has *broad* interests and competences, i.e., a "free man" in Aristotle's sense. The two are orthogonal.

On a more positive note, perhaps what Feyerabend had in mind was that being a dilettante involves having a kind of naïveté (or openness) towards the knowledge acquired in the field, leading the researcher – however a well-known specialist they might be – to scrutinize established knowledge without taking it for granted.⁶ On such an understanding of the term, a specialist would approach the subject as if he or she was a dilettante, raising questions and concerns that might seem naïve and provocative for other researchers, but that are capable of unveiling new paths of inquiry. As we will see in a moment, this is a better fit with the kind of scientists Feyerabend most admired.

As for Feyerabend's claim that the academic profession makes slaves of its members, though, here we should just recall that Aristotle thinks *all* paid employments, even at the University of California, Berkeley, "absorb and degrade the mind" (quoted in Feyerabend 1970c/1999: 118). Aristotle provides no basis for what one might *like* to see here, that is, a critique of the way some people are treated within and on the fringes of academia (some people, but *not* others: even academics who don't have Paul Feyerabend's astronomical salary are still well-treated relative to other kinds of employees).

Galileo as a dilettante?

Neither Galileo's sophisticated empiricism, that is, the combination of realism with empiricism that his view exhibits, nor his not being a naïve or radical empiricist, means that he was a dilettante in any ordinary sense, for sure.

Was he, though, a dilettante in Feyerabend's sense, that is, an Aristotelian "free man"? (I'm not trying to make a joke at the expense of the Catholic Church here). Was he a well-rounded individual? As far as we can tell from biographies, yes. And surely that helped him to *write* well. But this is only a part of what made him a great scientist, and of what made his contributions to science great ones.

⁶ For this suggestion I am grateful to an anonymous referee.

Certainly the ranks of great scientists feature their share of narrowly-focussed single-minded individuals, alongside plenty of more well-rounded ones. Nothing can be said against the list of Feyerabend's most prominent scientific heroes (which would include Aristotle, Galileo, Ernst Mach, Ludwig Boltzmann, Albert Einstein, Max Born, and Niels Bohr). Or against his list of scientific villains, which would no doubt feature Isaac Newton (Feyerabend calls him "The awful Newton, who more than anyone else is responsible for the plague of professionalism from which we suffer today"), and Max Planck. (It would also have included a host of minor figures – in fact, this category included almost *all* scientists). But (leaving aside issues of nationalism, I hope) here we really *do* have a *subjective* preference, and not a good basis for evaluating whose science was good or who was a great scientist.

Rather than Feyerabend's personality monism (everyone, or at least every expert, should be an Aristotelian "free man"), I think we ought to prefer a *pluralism* of personality-types, combined with a social mechanism that ensures that no one type of personality has the sole say in public-facing matters.

The advancement of science

But there is a deeper issue: what are we to make of Feyerabend's picture of how science advances? He assures us that science was advanced, and is still being advanced by dilettantes, and that experts are liable to bring it to a standstill. But what criterion could he give for science being either "advanced" or "at a standstill"? I strongly suspect that the answer is: none! As long as there are experts for each alternative theory in the "ocean of alternatives", his conception pretty much precludes it.

His perspective on this is worth comparing with that of his friend and contemporary Thomas Kuhn. It was Kuhn who popularised the notion of scientific *revolutions* (revolutions, *plural*, that is, relatively swift and wholesale revisions of the basic assumptions of a given scientific field) in his famous book *The Structure of Scientific Revolutions* (1962).

Kuhn would insist that most science is not *and cannot possibly be* a matter of revolutionary thinking. For Kuhn, most science takes place under the auspices of a "paradigm", a set of deep assumptions about the domain in question, the sort of assumptions which science students learn when they are being educated into the field, and which cannot be the subject of constant questioning. Science *advances*, though, during these periods of "normal science", just as surely as it does in "scientific revolutions". The kind of change in question is cumulative, rather than revolutionary, but it is just as real. And this kind of science, normal science, is the home playing-field of the expert.

I suspect that Kuhn would also insist that those who bring about scientific change don't do so because they are dilettantes. For Kuhn, scientific revolutionaries are, as it were, scientific conservatives who are *at the end of their rope*. In no way are they trying to be revolutionary – his perspective would scorn efforts to deliberately “revolutionise” scientific thinking. Those who initiate scientific revolutions, for Kuhn, are scientific experts who are trying to *defend* their existing paradigm, a paradigm which by then is in a serious state of crisis, by reinterpreting or re-jigging it in various ways. If that results in a scientific revolution, that is very much the *unintended* effect of their non-revolutionary activities.

Kuhn's picture may not be right. Maybe it fits only some of those who initiated scientific revolutions (like Charles Darwin, or Planck, who Feyerabend thought of as a dogmatist), and not others (such as Einstein). In addition, though, we must ask Feyerabend: why, when it comes to expert opinion, should we think that it is only the “*advancement*” of science that matters? Even if some particular science had stagnated, a situation which would appal Feyerabend, might it still not give the best answers to questions about the domain in question? Feyerabend didn't much care about the periods of stability between scientific revolutions, and most of his scientific heroes were people who presaged or brought about such revolutions. But why, when it comes to knowledge and the expert advice that should be based upon it, should we think that only *change* counts? Might not good expert advice be based upon scientific theorising which is (at that time) stable? In fact, isn't that what we generally expect, and *want*? Revolutionary science may feature experts, but one might well hesitate to take the advice of experts in such turbulent times.

Conclusions

“Experts in a Free Society” is lots of fun, but unreliable. Feyerabend was mainly concerned with the way scientists *write* (rather than the conclusions they come to, for example), and he was *generally* good at characterising that. But his claims on behalf of Galileo and Newton, who come away well, are exaggerated: each of them *did*, once Prefaces were done with, use technical language (physicists always do). That Galileo and Newton were (in certain respects) “starting afresh” should come as no surprise – but not every scientist can be expected to do so, and it would be a disaster if they did do so.

Feyerabend did a disservice to Masters and Johnson. He failed to find out about the two people in question, and ignored the context and the (mostly positive) effects of *Human Sexual Response*. Perhaps his critique is best thought of as a blast against a certain *kind* of expert, the kind then being produced in the US context. His vitriol about experts is understandable in

that context, but not generalisable (and Masters and Johnson were not experts of *that* kind).

Feyerabend was concerned with a popular myth about the Scientific Revolution, but he failed to show that this process had nothing to do with empiricism, since the kinds of empiricism he critiqued are *too philosophical*. He didn't successfully show that Galileo would have opposed the (now commonsensical) kind of empiricism that *did* play a role in the Scientific Revolution, and he ignored the things in Galileo's text which point to him having *not* rejected empiricism. His appeal to witch-hunters, magicians, alchemists, etc. was fanciful since, even if Galileo was suspicious of them, they weren't empiricists of the relevant kind.

Aristotle's "free man" may be a worthy ideal, but it is *not* to be identified with the dilettante (or the opportunist). Aristotle would not have accepted that equation, and none of Feyerabend's hero-scientists were dilettantes. Expertise is *deep* knowledge, and that's perfectly compatible with having a *wide* range of interests, as well as with the ability to see things in fresh and unprejudiced ways.

Finally, Feyerabend was preoccupied with how science advances, and that is always a one-sided preoccupation. His claims that science was advanced, and is still being advanced, by dilettantes, and that experts are liable to bring it to a standstill are problematic, since he offers (and may be *able* to offer) no criterion for science being either "advanced" or "at a standstill". In fact, by the time of "Experts in a Free Society", he had deprived himself of any way of saying how or whether science advances.

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