CHAPTER IO

Against Expertise A Lesson from Feyerabend's Science in a Free Society? Matthew J. Brown

10.1 Introduction

As my title suggests, I will examine a lesson from Feyerabend's controversial work, *Science in a Free Society*: that free societies should not invest scientific experts with special epistemic or social authority. At least, I will ask, should we take this claim from Feyerabend as a lesson? In particular, I see Feyerabend's argument about expert authority as a substantive challenge to a central commitment of many philosophers of science who reject the ideal of value-free science, a commitment to *the ineliminability of expert judgment*. An argument against this principle is articulated clearly and forcefully, if somewhat roughly, in both his *Science in a Free Society* (1978, hereafter *SFS*) and a related article, "How to Defend Society against Science," originally published in *Radical Philosophy* (1975c, hereafter *HDSS*).

Science in a Free Society, published over forty years ago, was Feyerabend's least well-received work, one even Feyerabend himself came to dislike. In his later years, he apparently wished that the publisher would cease reprinting the book.¹ There are a number of reasons this might have been. There is the problematic, even naive political philosophy of "the free society" contained in the book. The book defends a form of "relativism," a term Feyerabend tried to dissociate himself from in his later years. The third part of the book collects a rather testy set of responses to critics, which Feyerabend may have come to see as too salty even for his taste. And yet, major parts of the book's arguments were incorporated in the second and third editions of *Against Method*, Feyerabend's most well-known work, one that he continued to carefully rework throughout the middle and later periods of his career. The core ideas of *SFS* remained important to Feyerabend.

First, I provide some background for this discussion by looking at contemporary discussions of values in science and the role of expert

¹ The book remains in print to this day.

judgment in those discussions. Here, I lay out the importance and rationale behind the commitment to the ineliminability of expert judgment. Then, I briefly discuss Feyerabend's problematic political philosophy, in order to disentangle its commitments from what is potentially of value in his argument about the role of experts in society. Next, I lay out four theses about the relation between citizens and scientific experts that forms a major part of Feyerabend's arguments in *SFS* and *HDSS*. After briefly discussing a major caveat that Feyerabend makes about these claims in *SFS*, I evaluate Feyerabend's argument and its bearing on the question of the eliminability of expert judgment, ultimately arguing that Feyerabend presents a dilemma for the role of experts in society that we cannot avoid, though hopefully we can find a compromise position between its horns.

10.2 Background: Expert Judgment and Values in Science

Before we start looking at Feyerabend, I want to review some common commitments in the science and value literature which give contemporary relevance to Feyerabend's arguments. Although these commitments are common in the literature, for the sake of space, I will largely focus on arguments about *inductive risk* presented by Heather Douglas (2000, 2009). Douglas begins from the fact that ampliative claims are pervasive in scientific inquiry – claims that go beyond what is strictly implied by the evidential basis for those claims. Douglas argues that all such ampliative claims have a risk of error, and that the consequences of error can include social and ethical consequences. Because scientists have the ordinary responsibilities to consider the consequences of their actions, whenever scientists make empirical claims or inferences, they ought to make social or ethical value judgments to weigh the consequences of potential errors. You can run the same kind of argument with research questions, choice of language, or concepts with which to frame data or hypotheses, theory choice in the broad sense, or various other consequential contingencies in science (see Brown, 2020, Chapter 2). The conclusion of each argument is that scientists ought to exercise (non-epistemic) value judgment.

A common response to arguments like Douglas's is the *deferred decision response* – scientists need not make the risky empirical claims, but only pass on the relevant information to make those claims to decision-makers, who rightly make the relevant value judgments.² Or, alternatively, that

² Various versions of the response can be found in Jeffrey (1956); Mitchell (2004); Pielke Jr. (2007); Betz (2013); Edenhofer and Kowarsch (2015). The concept of the "deferred decision response" was

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scientists accept hypotheses for the purposes of scientific *belief* only, in such a way that is completely cut off from *action*. Scientists' proper work can thus remain value-free, and the burden of judgment is passed on to the relevant decision-makers. For example, Sandra Mitchell explicitly gives the deferred decision response to Douglas. First, she describes Carl Hempel's own approach to inductive risk. Hempel distinguishes between the inductive risk of hypothesis acceptance in science, which depends on purely epistemic values, and inductive risk in "practical contexts ... when the hypothesis is to form the basis of action or policy," where social or ethical values are appropriate. On this basis, she responds to Douglas thus:

Douglas justifies her 'expansion' of Hempel's argument from inductive risk into the domain of deciding which theory to accept as true by appealing to the authority of science in our society ... This conflation of the domains of belief and action confuses rather than clarifies the appropriate role of values in scientific practice. Indeed, to make public one's belief that a given hypothesis is true is an action, and in certain contexts a scientist might judge that stating what he or she is scientifically warranted to believe is politically inadvisable ... The values appropriate to generating *the belief* and the values appropriate to generating *the action* are different. (Mitchell 2004, pp. 250–251)

Mitchell goes on to argue that we must analytically separate the two functions, even when the same particular individuals might be involved in doing the science and making the policy decisions.

There are a number of problems with the deferred decision response. The deferred decision response usually focuses only on the final decision to accept or reject a hypothesis.³ But there is a regress of value-laden decisions throughout scientific inquiry, such as decisions about framing a hypothesis to test and about methodological choices. Also "the relevant information" that scientists might provide policymakers itself consists of further risky ampliative empirical claims, such as the characterization of evidence or attributing a probability to various hypotheses.⁴ The rigidly defined roles

first worked out collaboratively with my co-author Joyce Havstad, and she deserves the lion's share of the credit for it. See Havstad and Brown (2017).

- ³ The most nuanced version of the deferred-decision response appears in the "pragmatic-enlightened model" for science-based policy by Edenhofer and Kowarsch (2015). This version traces the role of value judgment through all of the decisions made throughout the process of scientific inquiry, setting up a "cartography" of pathways through the science that policymakers can decide how to navigate based on their value judgments. For critique of this version of the response, see Havstad and Brown (2017).
- ⁴ Richard Rudner already demonstrated this problem for the deferred decision response in Rudner (1953). Dan Steel refers to the uncertainty about probability estimates as "second-order uncertainty" (2016).

and oversight that the deferred decision response would require would likely *stifle* scientific progress. It is plausible that only the scientific experts have the *competency* – the knowledge, skills, and experience – necessary to adequately anticipate and weigh consequences of error. Finally, in practice, the role of scientific research and science advising are often *indistinguishable*.

These objections to the deferred decision response amount to a view that is a near consensus among those who reject the value-free ideal: the ineliminability of expert judgment. Simply put, experts must make value-laden scientific decisions with an eye to guiding policy- and decision-makers. I have defended this claim myself (Havstad and Brown 2017; Brown 2020), and it is important to the ways that those of us who have been defending the value-ladenness of science have been working. However, this position also raises problems – of accountability, trust, and legitimacy of experts in a democratic society (as many defenders of the value-free ideal point out, and as many advocates for values in science attempt to address; see Betz 2013; Bright 2018; Douglas 2005, 2009, Chapters 7–8). The ineliminability of expert judgment is the thesis that I want to use Feyerabend's argument to question.

10.3 Feyerabend's Problematic Political Philosophy

Before considering Feyerabend's arguments that, I think, call into question the ineliminability of expert judgment, it must be recognized from the outset that Feyerabend's theory of the "free society" in SFS is notoriously problematic. Among other things, it is committed to a kind of naive and simplistic libertarianism, problematic views about group identity and the nature of cultural traditions, and of course relativism. The untenability of Feyerabend's ideal of a "free society" has been so thoroughly discussed elsewhere that I do not need to recapitulate the problems (see, e.g., Munévar 1991; Brentano 1991; Kidd 2016a). However, much of SFS and HDSS are concerned not with defending this problematic approach, but with determining the role of science in this free society. Much of Feyerabend's account of science in society can be retained without adopting the problematic picture of a free society. In fact, few premises in the argument about science in society depend on the problematic aspects of Feyerabend's political philosophy.

10.4 Feyerabend on the Role of Science in a Free Society

Let us see, then, if we can understand Feyerabend's argument as one about the role of science and scientific experts in the kind of "free society" that is much more widely endorsed, one committed to basic liberal–democratic values and to political processes that both respect the rights of the citizens while being likely to deliver good outcomes on matters of public interest.

Even on this background, Feyerabend can make a case for an approach to scientific expertise that denies a special epistemic and cultural authority to the scientist. Feyerabend articulates and defends four increasingly radical claims about science and its place in society that, together, point toward a decentering of experts in society and a rejection of expert authority.

10.4.1 Citizens Can and Should Evaluate Expert Opinions

Feyerabend holds that citizens have the right to evaluate expert opinions for themselves, and that they ought to exercise that right. Here he does not simply mean that citizens have a right to believe what they will, in a purely private sense of their personal opinions. He means that every citizen has a right to evaluate, criticize, and reject in whole or in part expert claims for the purposes of public decision-making. He has three main arguments for this view.

First, citizens should be free to evaluate expert opinions for themselves for broadly Millian reasons, that freedom and plurality of opinion and open debate leads to better epistemic outcomes. Feyerabend adopts wholesale the arguments of John Stuart Mill's On Liberty and takes them, perhaps, further than Mill himself.⁵ Feyerabend sees a plurality of competing views in society, with the freedom to advocate for any belief and to adjudicate competing beliefs for oneself, as tending to increase the justification of those beliefs, the likelihood that true beliefs are found and adopted, and the very meaningfulness of those beliefs (SFS, p. 86). This putatively Millian argument is familiar to Feyerabend readers and much commented upon (Lloyd 1997; Staley 1999). Feyerabend uses the argument here to defend the process of citizens evaluating putative expert opinions for themselves, as being more likely to lead at least some citizens to adopt true beliefs, as well as their actually being justified in so adopting them and knowing what the beliefs actually mean. Feyerabend argues, with Mill, that this freedom of belief and expression is more likely than deference to others to lead to human progress.

⁵ See Jacobs (2003) for a critique of Feyerabend's interpretation of Mill.

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Second, Feyerabend argues that *even if* citizens having the last say over the claims of experts leads to worse *outcomes* for themselves or for society, it is nevertheless justified, as it would contribute to human freedom and to the development of a mature democratic citizenry – these goods outweighing the value of mere true belief or the risks of policymaking without the benefits of expert assessments. On the one hand, Feyerabend takes this as just definitional of democracy – in a democracy, the public must participate in fundamental decisions; they cannot be left to the experts. He simply asks us to be fully consistent on this point. Feyerabend also argues that the learning and maturation necessary for citizens to become wise or skillful public participants requires being that they be allowed to try and, perhaps, do poorly. According to Feyerabend, "[p]articipation of laymen in fundamental decisions is therefore required *even if it should lower the success rate of the decisions*" (SFS, p. 87).⁶ It is the kind of thing that distinguishes democracy from totalitarianism, which is free to focus on whatever is most "effective."

However, Feyerabend also thinks that motivated "laymen," in fact, can be competent enough to make good decisions regarding scientific information: "science is not beyond the natural shrewdness of the human race" (SFS, p. 98). Scientists, of course, are mere human beings; no miraculous feat transforms a human into a scientist. Among Feyerabend's favorite evidence for this point is the way that a skilled litigator can digest and critically analyze expert testimony on the fly, and thus expose weaknesses and uncertainties. Without specialized training, (suitably dedicated) non-experts can evaluate scientific information competently for themselves. The ability of historians and philosophers of science to analyze scientific publications and archival records, to break down the decisions behind them, and to critically assess them, is further evidence in Feyerabend's favor.

10.4.2 Citizens Can and Should Supervise Science

Feyerabend goes one step further: not only should citizens decide for themselves whether to believe and how to use scientific information; they should regularly subject science to careful scrutiny. He even goes so far as to suggest that citizens should "supervise" science.

⁶ There is reason to object to the use of the term "laymen" or "laity" as distinct from experts precisely because it sets experts up as clergy-like authorities. But science is not a church, and scientists are not authorities ordained by a higher power. I suspect Feyerabend uses this term knowingly and tongue-in -cheek. I will typically use "citizen" or "non-expert" instead of "laymen," except when paraphrasing Feyerabend's own use of the latter.

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Laymen can and must supervise Science . . . it would not only be foolish but downright irresponsible to accept the judgment of scientists and physicians without further examination. If the matter is important, either to a small group or to society as a whole, then this judgment must be subjected to the most painstaking scrutiny. (SFS, p. 96)

According to Feyerabend, we should elect committees of non-experts to regularly subject scientists and their work to review before it is put to social use. He specifically mentions reviewing evidence for theories before they are taught, reviewing the safety of nuclear power plants, and reviewing the efficacy of scientific medicine against alternatives.

One of the major reasons that Feyerabend sees the need for citizen supervision is his view that scientists themselves are often prejudiced and untrustworthy:

Expert Opinion [is] often Prejudiced, Untrustworthy, and in Need of Outside Control . . . scientists quite often just don't know what they are talking about. They have strong opinions, they know some standard arguments for these opinions, they may even know some results outside the particular field in which they are doing research but most of the time they depend, and have to depend (because of specialization), on gossip and rumours. (SFS, pp. 88–89)

The argument here is a bit complicated. One might balk at the claim that scientists "don't know what they are talking about." But in this context, it is clear that Feyerabend is worried about the rather common phenomenon of scientific experts speaking authoritatively outside of their (narrow) area of expertise. Feyerabend spends a lengthy section of SFS criticizing "Objections to Astrology: A Statement by 186 Leading Scientists," a brief statement published in The Humanist magazine, September/October 1975 issue (see also Kidd 2016b). He does this not because he cares to defend astrology, but to show the irresponsible attempt of this band of scientists to assert their authority. He shows that many of the scientists knew nothing about astrology and did not have expertise directly relevant to the issue. Feyerabend summarizes, "[t]hey neither know the subject they attack, astrology, nor those parts of their own science that undermine their attack" (SFS, p. 92). He demonstrates that by the lights of mainstream science, some of the claims from the statement were baldly false, while others are irrelevant.⁷ These are in themselves good reasons to insist on some increased scrutiny, to check on

⁷ Feyerabend was in good company, here. Astronomer, science popularizer, and pseudoscience critic Carl Sagan similarly objected to the content of the statement in a letter to the editor of *The Humanist*.

whether experts are advising within or outside of their area of expertise. But should they also be supervised within their area of expertise?

One reason to think so comes from a common view about the nature of (scientific) knowledge in philosophies of science like Feyerabend's, Kuhn's, and Lakatos's. On this view, science contains some necessarily presumptive, dogmatic element that both makes scientific progress possible but also contingent (different presumptions would have led to progress in a different direction). As Feyerabend puts it,

Such ideas are not simply errors. They are necessary for research: progress in one direction cannot be achieved without blocking progress in another. But research in that 'other' direction may reveal that the 'progress' achieved so far is but a chimera. It may seriously undermine the authority of the field as a whole. Thus science needs both the *narrowmindedness* that puts obstacles in the path of an unchained curiosity and the *ignorance* that either disregards the obstacles, or is incapable of perceiving them. (SFS, p. 89)

That functional "ignorance" can be provided by non-expert control of science. But this way of putting things (" . . . science needs . . . ") is inapt. For Feyerabend's argument in *Against Method*, such a claim is on point: dilettantes, amateurs, and heterodox scientists have much to offer *science* because they can pursue paths closed off by scientific orthodoxy, and thus promote the pluralistic growth of knowledge. But the appropriate question in the line of argument Feyerabend is pursuing in Part 2 of *SFS* is not, "What does science need?" but "What does society need?" The nascent point that Feyerabend is making is rather that, precisely because scientific orthodoxy may block the development of knowledge in precisely the direction that some citizens may desire, while producing progress in directions that might turn out to be chimerical, we should not just take the scientists' word for it. We must supervise their work to determine whether this is the case.

Even more narrowly construed, within their proper area of expertise, and according to the paradigm or research tradition adopted, Feyerabend raises concerns about the trustworthiness of scientists, and insists that they must be supervised by citizens. Even in this area, Feyerabend thinks that scientists can hide "uncertainty, indefiniteness, the monumental ignorance" behind jargon and assertions of epistemic authority (SFS, p. 98). It may not be too much of an exaggeration to say that scientists regularly oversell the strength of their knowledge to society, in ways that hurt their credibility. For instance, Jim Brown and Jacob Stegenga have argued forcefully, the trustworthiness of contemporary medical science is in grave doubt (J. R. Brown 2002, 2016;

J. R. Brown 2008a, 2008b; Stegenga 2018). The human and social sciences are beset by replication crises (Smaldino and McElreath 2016). Arguments have been made from within science that most published findings are in fact false (Ioannidis 2005). And even where the technical quality of the research is good, if science is value-laden, and scientists incorporate their personal values into science, this may give reason for certain groups to distrust those results, if they disagree with said values. Without any established democratic means for influencing scientists' value judgments, this issue is pressing.

10.4.3 Science Is Just Another Ideology or Interest Group

Feyerabend argues that we should see scientists as purveyors of just another ideology, that is, as a group with a specific perspective and a characteristic set of beliefs reflecting that perspective (see Selinger 2003, 360–361). This can be seen, for instance, in the dogmatic form of science education:

Scientific 'facts' are taught at a very early age and in the very same manner in which religious 'facts' were taught only a century ago. There is no attempt to waken the critical abilities of the pupil so that he may be able to see things in perspective. At the universities the situation is even worse, for indoctrination is here carried out in a much more systematic manner. (HDSS, p. 4)

This can be contrasted with Kuhn's view of science education. While Kuhn agrees that science education is rigid and dogmatic, he argues that this is necessary for providing scientists with the working commitments that make progress possible (see Kuhn 1963). Feyerabend sees this as evidence rather that scientists favor their own lore and wish to use their special place in society to push it as universal truth on unsuspecting young minds.

Feyerabend taught at the University of California, Berkeley during the desegregation of public education in the United States and the attendant increase in non-white enrollments at Berkeley, an experience that had a profound effect on his philosophical outlook (Kidd 2013, pp. 408ff; 2016a, p. 124). He came to see his educational role as essentially oppressive, pushing "reflections of the conceit of a small group who had succeeded in enslaving everyone else with their ideas," and to find the very idea revolting (SFS, pp. 118–119). The problem was that the ideology of the privileged remained centered:

But equality, racial equality included, then did not mean equality of traditions; it meant equality of access to one particular tradition – the tradition of the White Man. (SFS, p. 76) On Feyerabend's view, "the tradition of the White Man" was centered because of the power of white supremacy, and not because of the undeniable superiority of their results or method:

... the comparative excellence of science has been anything but established ... Science does not excel because of its method for there is no method; and it does not excel because of its results: we know what science does, we have not the faintest idea whether other traditions could not do much better. So, we must find out. (SFS p. 106; cf. HDSS, p. 5)

Because science is just another ideology, and scientists are just an interest group promoting said ideology, historically through the exercise of colonial, patriarchal, and white supremacist power, there is no justification in giving science greater authority than any other tradition. Hence, citizens will have to make up their own mind about whether to accept scientific conclusions, and where science might have significant impact on society, it must be supervised by citizens. The value-ladenness of science strengthens this point, as there exist no established means for scientists to incorporate any but their own personal values into their scientific decisions.

Feyerabend's position here is supported by a philosopher of a very different temperament, the great American philosopher of science, education, and democracy, John Dewey:

A class of experts is inevitably so removed from common interests as to become a class with private interests and private knowledge ... No government by experts in which the masses do not have the chance to inform the experts as to their needs can be anything but an oligarchy managed in the interests of the few (Dewey 1927, pp. 364–365)

Dewey rejected the technocratic role of scientific experts in governance, all the while insisting on the importance of science. For Dewey, however, the participation of the public in the process was paramount. Any opportunity to exercise authority, uninformed and uncontrolled by the public, amounts to autocratic rule by private interests.

10.4.4 Science Should Be Separated from the State

Another consequence which Feyerabend takes from the claim that science is a culturally specific ideology or a particular interest group is that it should be treated with a certain distance by the state. Indeed, Feyerabend claims that there should be formal separation between science and the state, modeled on the formal separation between church and state that is characteristic of many liberal democracies:

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The most important consequence is that there must be a formal separation between state and science just as there is now a formal separation between state and church. Science may influence society but only to the extent to which any political or other pressure group is permitted to influence society. Scientists may be consulted on important projects but the final judgement must be left to the democratically elected consulting bodies. (HDSS, p. 6)

This may seem to conflict with Feyerabend's second claim, that society should supervise or monitor science. This points to two ways of interpreting the claim that citizens should supervise science. One meaning of "supervise" is control; a manager who supervises an employee's work is controlling the work of that employee. This generates a tension for Feyerabend: if science is a private activity, separate from the state, then non-scientist citizen control of that activity seems to violate a host of liberal-democratic rights such as freedom of association and freedom of opinion. Given Feyerabend's insistence on the rights of individuals and groups throughout *SFS*, this seems at odds with the overall argument.

Another meaning of "supervise" is to monitor and evaluate without interfering. This seems more consistent with Feyerabend's project. Here, citizens are charged with closely scrutinizing what science does, insofar as they may want to let science influence society on particular points. This is compatible with formal separation of science and state. In a liberal democracy with strong separation of church and state, religious citizens can vote, religious leaders are consulted on public decisions, and sometimes even religious organizations can be contracted to fulfill state functions, such as providing homeless shelters. But in the latter two cases, it is up to citizens or democratically elected representatives to independently supervise by evaluating the claims of religious leaders and the functioning of religious organizations. Similarly, if science is to be thought of as a private activity, but allowed to influence society, it must be monitored and independently evaluated.

10.4.5 Summary of the Argument

Feyerabend has argued as follows. Scientists often speak outside of the areas of their expertise with the same confidence and assertion of authority as within their area of expertise. They do this even when they have not properly looked into the question at hand. Even within their area of expertise, scientists tend to be close-minded and arrogant. The very nature of scientific knowledge requires a narrow-mindedness that makes it a problematic resource for society at large. In effect, science is an ideology or an interest group, with its own articles of faith, its own specific aims and

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values that may be at odds with those of citizens. What's more, we cannot overlook these issues on the basis of the superior results that science has delivered, because there has never been a fair and even playing field from which to adjudicate such superiority. Science should thus not be granted any special epistemic authority, and citizens will have to make up their minds for themselves whether to believe the deliverances of science. Science should be formally separated from the state, not given a special role in policy or education. That does not mean science cannot or will not be consulted; only that it does in the way any interest group operates in a democratic society. Whenever science may influence or impact society, it should be monitored and evaluated by non-experts, to ensure its influence on society is beneficial and legitimate.

10.4.6 The Maturity Caveat

Feyerabend occasionally qualifies the sorts of claims made above as applying to a society of "mature people," and he quotes John Stuart Mill who argues that his views on pluralism and free exchange of ideas "is meant to apply only to human beings in the maturity of their faculties" (SFS, 29n). Near the end of his main argument, he makes the following major caveat:

The separation of state and science (rationalism) which is an essential part of this general separation of state and traditions cannot be introduced by a single political act and it should not be introduced in this way: many people have not yet reached the maturity necessary for living in a free society ... The maturity I am speaking about is not an intellectual virtue, it is a sensitivity that can only be acquired by frequent contacts with different points of view. It can't be taught in schools ... But it can be acquired by participating in citizens initiatives. This is why the *slow* process, the *slow* erosion of the authority of science and of other pushy institutions that is produced by these initiatives is to be preferred to more radical measures: citizen initiatives are the best and only school for free citizens we now have. (SFS, p. 107)

This can easily be seen as taking the sails out of the radical claims canvassed earlier. We thought Feyerabend was recommending that citizens make up their own minds and that science be formally separated from the state, but he is really recommending neither. Should science thus retain its epistemic authority and special place with respect to the state?

Feyerabend is not entirely clear here, but there are reasons not to read this caveat as complete reversal but rather as a plea for gradualism and bottom-up, rather than top-down change. He emphasizes here the importance of informal education and the encounter with other points of view, some of which will be "non-Western" or non-scientific perspectives. Also, in a passage partially quoted earlier, Feyerabend tells us that the relevant kind of maturity is not learned in schools, but "by *active participation* in decisions that are still to be made" (SFS, p. 87). Cultivating maturity means allowing citizens to decide for themselves, as quoted above, "even if it should lower the success rate of the decisions." However, current citizens should not all at once be thrust in to having decide everything for themselves. They should gradually do so through growth of participation in "citizens initiatives."

We live in a world where many gullible citizens are liable to be hornswoggled by industry-driven merchants of doubt into believing that climate science is a hoax; by QAnon conspiracists into believing that Donald Trump is working to secretly save the world from the secret Satanic pedophiles who secretly run it, and by nutrition gurus who hold the secret to the miracle of health but choose to release it as a multi-level marketing scheme rather than through scientific or even mainstream business channels. In that context, Feyerabend's argument might seem a little bit irresponsible. In this context, the maturity caveat is crucial. Feyerabend's four claims represent ideals to work toward, not immediate policy proposals. Still, whether we *should* work toward them depends, in part, on how well his argument against expert authority and against the ineliminability of expert judgment fare.

10.5 Whither the Eliminability of Expert Judgment?

Considering Feyerabend's argument, should we give up on or modify our commitment to the ineliminability of expert judgment? Can we allow citizens or policymakers to decide for themselves? If we answer yes to these questions, then the arguments against the deferred decision defense of value-free science puts us in a difficult position. On the one hand, we might work to reform science such that the separability between science and policy analytically made by Mitchell can become a practical reality. If, as Feyerabend argues, we tend to grossly underestimate the competence of non-scientists to evaluate technical scientific information, then this may be the best path forward. This would allow, perhaps, for a modicum of authority to remain with science, rendered suitably value-free by being walled off from social relevance. On the other hand, if science is unavoidably value-laden, this raises significant concerns about the democratic legitimacy of science's epistemic authority. In turn, this potentially bolsters Feyerabend's claim that science is just another ideology and scientists just another interest group in society. In that case, whether or not citizens are really competent to evaluate science, they have no choice; they cannot take what scientists claim for granted.

In this section, I will explore some ideas that complement Feyerabend's approach, as well as some potential objections, in ways that I think sharpen the problem. On their basis, I will stake out two possible positions on the role of experts in society and whether we should continue to insist on the authority and eliminability of expert judgments.

10.5.1 Scientific Judgment: Transparent or Opaque?

Ian James Kidd (2016b) provides an interesting objection to Feyerabend's account of the illegitimate exercise of authority by scientists, as exemplified in the astrology case, based on Michael Polanyi's response to the case of Immanuel Velikovsky (also briefly commented on by Feyerabend, SFS 91n). Velikovsky published a book, *Worlds in Collision*, in 1950. In it, he laid out and defended a radical theory that the solar system had undergone major changes during historical times, forming the basis for many stories of ancient mythology and religion, which themselves were a source of putative evidence for Velikovsky's theory. There was an immediate and severe response from the mainstream scientific community, which attempted to censor and dismiss the book as pseudoscience. There, the scientific community showed the same sort of inappropriate dismissiveness (speaking outside of their expertise, without even really investigating the case). Polanyi argues, however, that rather than showing a lack of integrity or an illegitimate appeal to authority, their dismissiveness is entirely appropriate. Polanyi is well known for demonstrating the importance of the *tacit* dimension of scientific knowledge; the skills and implicit assumptions that make science possible. As Kidd summarized, "[w]hat seemed, to non-scientists, to be reactionary dogmatism was, in fact, a spontaneous evaluation both generated and justified by a tacit sense of plausibility. Polanyi concluded that since that sense is historically informed, collectively supported, and a product of practice and discipline, those scientists were right to trust it" (2016b, p. 476). Otherwise, they would be wasting time and resources. This points strongly toward the ineliminability of expert judgment.

A similar argument in favor of the ineliminability of expert judgment can be extracted from some ideas of Bruno Latour. Latour emphasizes not so much the dimension of tacit knowledge as the material means of science and

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their rhetorical function. In Science in Action (1987), Latour asks us to consider what is necessary to continue to dispute a scientific claim; that is, he asks us to think about the nature of arguments and counter-arguments in science. Arguments in science differ from arguments in the humanities. Arguments in the humanities are transparent – you have the textual evidence, scholarly references, the arguments, and that's it. All you need is a library card, cleverness, and motivation in order to craft a counterargument. Some aspects of the technical literature in science is, of course, like this as well – there are references to follow and logical and mathematical arguments that can be criticized. But, arguments in science are also different. Scientists construct phenomena in laboratories, which they turn into inscriptions (tables, charts, graphs, and figures), which play a special role in scientific arguments. They cannot be disputed in the way that a logical argument or an interpretation can be disputed. One may be able to visit the lab and find a flaw in the inscription device (the laboratory equipment). But, if no such flaw can be found and agreed upon, there is one further strategy available to disputants: build a laboratory of one's own and generate different results.

In the most general case in science, you cannot have a complete counterargument without a *counter-laboratory*. This is the bread and butter of scientific argument; as Latour says, "[t]his is why all laboratories are *counter-laboratories*" (1987, p. 79). What's more, "[t]he dissenters cannot do less than the authors. ... So the dissenters do not simply have to get a laboratory; they have to get a *better* laboratory" (p. 79). This restricts effective dissent toward putative scientific facts to a very specific group with the skills and resources to operate with the proper material, technological means. Thus, while the main action of science takes place in the discursive "agonistic field" of the published literature, the anatomy of the scientific laboratory. Thus, even non-scientists who are very committed, resourceful, and mature may not be able to decide for themselves without a laboratory of their own, without becoming a scientific expert in their own right.

These arguments suggest bases for scientific expertise outside the explicit (formal or informal) communications of scientists in tacit knowledge and the material means of knowledge-production; bases that cannot be completely supervised by non-experts. Ian James Kidd (2016b) offers the beginnings of a Feyerabendian response to this line of argument. As Kidd points out, the mere existence of these tacit dimensions of science does not, on its own, suffice to guarantee the public acceptance of the authority of science. If the public cannot effectively control science democratically as a result, they have to decide whether or not to trust science. For

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Feyerabend, "[c]entral to his conception of the social authority of science is . . . the claim that scientists ought to conduct themselves with integrity" (Kidd 2016b, 14). Their behavior in the astrology and Velikovsky cases is reason to think that science has not yet earned that trust and authority.

10.5.2 Authority versus Autonomy of Science

Another challenge to Feyerabend's argument comes from recent discussions in the literature on science and values about the relationship between the authority and autonomy of science (Douglas 2009; Brown 2013). As Heather Douglas argues:

On the basis of the value-free nature of science, one could argue for the general authoritativeness of its claims. But [given that science is not value-free] an autonomous *and* authoritative science is intolerable. For if the values that drive inquiry, either in the selection and framing of research or in the setting of burdens of proof, are inimical to the society in which the science exists, the surrounding society is forced to accept the science and its claims, with no recourse. A fully autonomous and authoritative science is too powerful, with no attendant responsibility, or so I shall argue. Critics of science attacked the most obvious aspect of this issue first: science's authority. Yet science is stunningly successful at producing accounts of the world. Critiques of science's general authority in the face of its obvious importance seem absurd. The issue that requires serious examination and reevaluation is not the authority of science, but its autonomy. (Douglas 2009, pp. 7–8)

Douglas here points out an important tension between autonomy and authority. If we think about institutions with social authority, they are or ought to be answerable to society. On the other hand, private individuals and organizations plausibly have a right to a great degree of autonomy, but they have no social authority beyond the right, which everyone has, to freely have their say and their vote. To allow a person or institution to have both would be a legitimization of autocracy.

Douglas presumes that the authority of science is not contestable, primarily on the basis of the record of its successes. If science is successful in this way, it has earned its authority. This combined with the argument that science cannot be value-free leads to the ineliminability of expert judgment and the reduction of the autonomy of science. As we have seen, Feyerabend disputes both the claim of success and the inference from success to authority. His challenge at least identifies a gap in Douglas's reasoning on this point. If authority and autonomy present

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the tension Douglas identifies, we appear to have the option to limit the authority *or* the autonomy of science.

On the other hand, Douglas presents an alternative option for those who share Feyerabend's concerns about the legitimacy of the social authority of science, but who are not ready to wholesale deny that authority. That authority could, potentially, be legitimized by curtailing the autonomy of science, requiring that it be guided by or answerable to the public. Call the position that Feyerabend advocates and Douglas rejects, "*epistemic anarchism*." This position allows the functioning of society to be left alone, but radically reconfigures society's relationship to science. Call the alternative position, which preserves the authority of science but curtails its autonomy, "*strong accountability*." This more or less retains the current dependence of society on science, or even strengthens it, but requires radical reconfiguration of the functioning of science itself. In this approach, "democratic control of science" has an even more robust meaning than in Feyerabend's consideration of that idea.

Robert Paul Wolff shares Douglas's concern about the incompatibility of authority and autonomy, though he is concerned with slightly different senses of "autonomy" and "authority" in the context of political philosophy rather than philosophy of science. Wolff is concerned about whether the moral autonomy of the individual is compatible with the legitimate authority of the state (Wolff 1970). He argues that they are not in fact compatible. He, too, considers a dilemma:

Either we must embrace philosophical anarchism and treat all governments as non-legitimate bodies whose commands must be judged and evaluated in each instance before they are obeyed; or else, we must give up as quixotic the pursuit of autonomy in the political realm and submit ourselves (by an implicit promise) to whatever form of government appears most just and beneficent at the moment. (Wolff 1970, p. 71)

Wolff argues, on explicitly Kantian grounds, that giving up autonomy is out of the question. He thus defends *philosophical anarchism*, which denies that there is any authority over one's conduct, besides the law of morality. And, as with Kant, the moral law is something we can only give ourselves, not something that is imposed on us from outside. Feyerabend is an anarchist in precisely the same sense as Wolff, but concerning epistemic rather than ethical-political matters. He, too, is deeply committed to the autonomy of the individual. He can be taken to insist that citizens judge and evaluate knowledge claims in each instance before accepting them, rather than accepting them on some external authority.

There are thus two kinds of anarchism in Feyerabend's body of work. The one, explicitly named and frequently discussed, is known as *epistemological anarchism* (Feyerabend 1993). This thesis concerns the authority of scientific methodologies. Feyerabend argues that no single methodology, as binding, can promote the progress of science. Thus, the search by philosophers of science for a single, binding methodology is not only hopeless but also harmful. The epistemological anarchist instead recommends pluralism, contextualism, flexibility, opportunism, and creativity with respect to methodology. The broader view, which Feyerabend misleadingly termed *relativism*, is philosophical anarchism as concerned knowledge claims, which I will thus call *epistemic anarchism*. Epistemic anarchism denies any special authority over your belief or acceptance of claims, over and above what you can judge for yourself using ordinary epistemic norms, as given to yourself in the Kantian sense.

Epistemic anarchism entails the strongly curtailed authority for scientific experts that Feyerabend recommends; they will be listened to, but only as any special interest group. Epistemic anarchism does not in itself deny the value of the division of epistemic labor. What it does is deny the right of specialists within such a division to demand deference to their claims. Nor is epistemic anarchism incompatible with one's accepting some claims of scientific consensus (just as Wolff's philosophical anarchism is compatible with following some laws, if they accord with the moral law within you). Informed deliberators might decide to take that consensus as good reason to accept such claims, in some contexts, though they would be unwise to adopt this as a universal policy, as unexamined presuppositions are also sometimes the object of consensus.

Strong accountability legitimates scientific authority at the cost of its autonomy. The values incorporated into science, from the choice of research questions to the setting of burdens of proof, will need to be responsive and answerable to the public. How this could be implemented is unclear. Will scientists be able to make such value judgments on their own? Will norms of transparency be sufficient to guarantee accountability? Will new institutional structures for citizen consultation and oversight be necessary? These are the sort of questions those committed to scientific authority and the ineliminability of expert judgment need to ask.

Against Expertise

10.6 Separation or Control?

Earlier, I identified an apparent tension in Feyerabend's thinking: should we have separation of science and state or control of science by citizens? I hastily argued that one could avoid contradiction by interpreting "control" as monitoring or evaluating rather than guiding. But perhaps it is wiser to sit with the tension. This tension reflects the other tension identified between autonomy and authority. Feyerabend was not focused on the tension in exactly the way that Douglas highlights, but the two are closely related.

Based on Feyerabend's arguments and the further considerations discussed here, I see a dilemma for the role of experts in society: On the one hand, we could strongly curtail the *authority* of scientific experts, ensure the separation of science and the state, and develop a more engaged, more mature democratic citizenry capable of adequately judging scientific results and appropriating or ignoring them as suits their needs. I have called this position *epistemic anarchism*, an extension to epistemic matters of philosophical anarchism. On the other hand, we could strongly curtail the *autonomy* of scientific practice, increase citizen oversight over or control of science, and make a radical shift of the focus of science toward the public interest. I have called this position *strong accountability*.

Each position has its benefits and drawbacks. Epistemic anarchism ensures that public values are respected by placing the moment of judgment in the public's hands or the hands of their representatives. There is no concern that private values might influence science and thus illegitimately affect our decision-making, as all scientific advice will be evaluated by nonexperts. And, it will have equal standing in public decision-making with other local, situated, indigenous, and alternative knowledges.⁸ This approach fits better with a participatory democratic ideal, and it avoids the problem of illegitimate technocracy and paternalism. On the side of science, it preserves academic freedom and the right to free inquiry. It allows science to be value-neutral, if not precisely value-free. Scientists can ignore the social impact of their work without being socially irresponsible, as science would be a purely private activity.

There are, however, significant potential drawbacks of epistemic anarchism. In the political realm, history seems to show that in the absence of legitimate authority, illegitimate coercion tends to arise. Functional anarchist organizations or societies are few indeed. Inefficiencies and bad

⁸ With apologies to Shari Clough.

decisions are likely, as ordinary individuals are prone to mistakes, (perhaps) more so than experts. Epistemic anarchism diminishes the traditional role of science as public reason. Though arguably this role has already eroded, exploding it entirely could promote further social fragmentation and disagreement. Epistemic anarchism places a heavy burden on individual citizens for education and judgment, burdens that they may not be willing to bear.

Strong accountability, by contrast, retains the intuitive authority given to scientific experts and the scientific process. It better fits with structures of representative democracy, which devolve many aspects of governance onto representatives, bureaucracies, and experts. It preserves the role of science as public reason. Perhaps its greatest benefit is that it reorients science from private interests to public-interest science. The problems of private-interest science are many, including corruption and bias (Krimsky 2003). The promise of public, mission-driven science, by contrast, is great, and its strong track record has often been underestimated (Sarewitz 2016). On the other hand, strong accountability requires that we create new norms, structures, or institutions for consultation, authorization, and regulation of science. It requires that we overcome existing institutional tensions between science and politics. And it places a significant burden on scientists to be aware of, represent, and be answerable to public values and interests. Scientists may not be willing to bear such burdens.

10.7 Conclusion: Collective Inquiry as a Third Way?

Science is unavoidably value-laden. The role of values in science cannot be deferred onto politicians, bureaucrats, or the public, but must involve the scientific experts themselves. Expert judgment is an ineliminable part of the process. So the mainstream argument for values in science goes. But Paul Feyerabend gives us reason to question the authority of scientific experts. This line of questioning leads us to a tension between the authority and the autonomy of science, and thus to a tension between two of Feyerabend's proposals: should science and the state be separate, or should there be democratic control of science? These tensions point to a dilemma in the relationship between science and society: give up on the authority of science and, with Feyerabend, become epistemic anarchists or give up on the autonomy of science and create a regime of strong accountability. My hope is that the dilemma is a false one, and we can develop a third way between these two approaches, one that retains many of the benefits and

avoids many of the drawbacks of epistemic anarchism and strong accountability. I do not have space to outline this alternative in detail, but I will gesture toward what such a view might look like.

The middle way I have in mind combines the emphases on citizen participation, the importance of science, and the value of the division of epistemic labor. It requires us to reconceive democracy along participative–democratic lines as a kind of *collective inquiry*, an idea central to the work of John Dewey (Dewey 1927; Bohman 1999). Recognizing the importance of the division of labor and the need for scientific specialists, it requires us to reconceive science-informed policy as a kind of interdisciplinary collaboration on inquiry into a shared problem. The collaboration might include scientific experts, policy-making experts, public and stakeholder representatives, and those with local, non-expert knowledge. This middle way, which I call *democracy as inquiry*, draws on various approaches that ask us to break down the traditional conceptions of science and the public and reconfigure them in ways that re-sort their traditional tasks (Dewey 1927; Bohman 1999; Latour 2004; M.B. Brown 2009, M.J. Brown 2013).

Expert authority and autonomy would both exist, with limitations, under democracy as inquiry. This approach recognizes the special authority of scientists *within* their specific area of expertise, according to the division of epistemic labor. However, this authority is not absolute, but rather requires situationally specific evaluation and renegotiation of relevance and standing in each social and policy context in which scientific results or advice are being considered. In other words, society will adopt the same trust-but-verify attitude that cautious scientists take toward each other's work. Research of primarily specialist interest, and technical decisions made within the process of basic research, would remain fairly autonomous. The limits on the social authority of such research would make this autonomy reasonably tolerable. Policymaking inquiry, in contrast, must be appropriately, publicly authorized, accountable, representative, and so on (see M.B. Brown 2009). Here, there must be checks in place to make sure that publicly authorized values guide inquiry. Since policy inquiry would not take past results for granted, but rather renegotiate their standing and relevance in the course of the new context of inquiry, the relative autonomy of basic research should not be a problem; it will have no immediate authority in the new inquiry. Expert judgment is ineliminable, but only one contribution to a larger process.

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This is a hasty sketch of a complicated view, and I do not hope to have convinced you of its viability. But, it is one example of a way forward that might avoid some of the problems with rejecting authority or autonomy wholesale and limn the middle way between epistemic anarchism and strong accountability.

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