**On Feyerabend’s “On the Responsibility of Scientists”**

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**Abstract:**

In this paper, we provide a critical overview of Feyerabend’s unpublished manuscript “On the Responsibility of Scientists.” Specifically, we locate the paper within Feyerabend’s corpus and show how it relates to his published remarks on topics such as expertise, democracy and science, opportunism, science funding, and the value of scientific knowledge. We also show how Feyerabend’s views anticipate and point novel directions for contemporary philosophical literature on values in science.

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**On the Responsibility of Scientists**

Scientists are supposed to be responsible for (a) running science properly and (b) taking care that dangerous results are kept from the rest of society.

As regards (a) it is often said that science is inherently self-critical and needs no external guidance. Any outside interference, it is said, will disturb the path of science and make it less efficient than it would otherwise be. It is admitted that self-criticism is not perfect (cf. recent examples of manufactured evidence) and that it cannot be expected to be perfect. But it is still affirmed that it provides better guidance than any alternative that might be imagined. Science, though fallible, knows best.

But in a democracy science is a part of larger units that are also self critical. Which self criticism should be preferred? The internal self criticism of science or the (for science external) self criticism of democratic institutions? In the case of a conflict – who knows better? Science or democracy?

The answer suggested by scientists, scientifically inclined philosophers and a large percentage of the general public is: difficult problems should be decided by experts and accepted by politicians and the population at large. But experts only rarely agree on difficult matters and many problems they are supposed to solve are outside their area of competence. In practice expert opinions on important social matters crystalize around two or three suggestions which may be connected with well defined political positions. Thus ‘science knows best’ turns into ‘our scientists know best’ and the problem is now how ‘our scientists’ can be made to prevail over ‘their scientists.’ This is a political not a scientific problem and it has been solved, long ago, though only on a small scale: in a trial by jury the experts give their judgment and then the jury (which consists mainly of laypeople) decides whose position is to be accepted.

There is another reason why ‘science knows best’ cannot guide our treatment of even purely scientific knowledge. Scientists like to pursue successful research programs; they follow the line of least resistance. After the discovery of the structure of DNA every young biologist tried to get into molecular biology. This is where the excitement was, where Nobel prizes, well paid academic positions and invitations to conferences in exotic countries could be obtained. Other parts of biology were neglected not because there was no truth in them (though some of the young turks soon talked as if they alone were capable of producing knowledge), but because their truth was either too remote, or too obvious to be examined. But scientific opportunism and the needs of society do not always coincide. Should a society finance a subject that attracts scientists because it is temporarily successful, or should it not rather encourage subjects closer in content and spirit to the burning questions of the time? The issue is often concealed by the assumption that a successful subject will eventually be capable of solving all problems. An enormous amount of money is now being spent on the attempt to map the human genome. One expects that health, social harmony etc. etc. will profit from the knowledge gained and that other, more ‘holistic’ and ‘subjective’ approaches (i.e., approaches dealing with how social change looks to the people who experience it) don’t deserve attention. Note that this attitude contains a political position in the guise of a scientific expectation: it is assumed that people who do not fit into certain surroundings have to be ‘improved’ by genetic tinkering. Note also that the expectation is far from reasonable: human life is much too complex to be resolved into a few traits. Note further, that any widespread popularity of the program will lead to a new way of looking at humans: what counts are the features that can be correlated to the elements found; the rest is irritating idiosyncrasy. ‘Science knows best’ here turns into plain tyranny. But tyranny has no place in a democratic society.

A further point is more abstract, but no less important. I argued that unpopular trends within the sciences may have to be encouraged because they may be closer in content as well as in spirit to important social problems and tendencies. I now add that unpopular trends are also needed to show the limits of the more popular scientific research programmes. Every research programme has successes and areas which seem to resist penetration. Considering such areas scientists supporting the programme are in the habit of saying: ‘the matter is difficult; we understand the basics; give us time and money and we will penetrate this area as well.’ Now an unpopular alternative may provide the needed understanding, but in its own very different terms and may thus show the expectation to be unreasonable (the search for further epicycles ceased to be reasonable after Copernicus).

On to point (b) – the duty of scientists to consider the social (ecological, military) consequences of their research. Many theoreticians reply by distinguishing between knowledge and its application and by asserting that knowledge is inherently good. Let us take it for granted, for the moment, that knowledge is inherently good. Does it follow that it should be produced, no matter what? It does not. Assume that a piece of knowledge just discovered fits neatly into a powerful and well financed military programme designed to conquer the world and subject it to a reign of terror. It is the last element that is still missing. I would say that a scientist who made the discovery is too close to the battleline to disregard its practical applications. So, a scientist has to bear two things in mind; (1) where is the battleline? (2) what should I do if I am so close to it that my discovery will be immediately used (this question now arises for some computer scientists working on Starwars). The reply ‘if I don’t publish, someone else will’ is childish. Number one, because there is always a chance that some element of my discovery will be overlooked by others and, number two: why should I be in such a hurry to lead others into disaster? Note that here again one cannot trust the self criticism and self policing of the community of scientists. Murder is subject to the self criticism of family, school, church and other institutions. But murder does occur. We have a police force to prevent it. Similarly we need a corrective force to police scientists, who, after all, are a competitive and greedy lot, despite all the alleged self-criticism that occurs in their ranks.

But theoretical knowledge is not at all as benevolent as generally assumed. Take classical mechanics. It is a very useful discipline. It is also very beautiful. But now assume that we regard it as a true account of the world and not merely as a (useful and beautiful) model of it that helps us in some circumstances but hinders us in others. Taking this step we regard the world as a mechanical system consisting in parts, without inherent purpose. An attitude of reverence towards nature no longer makes any sense. Continuing now into sociobiology and molecular biology we are also encouraged to take a similarly unsentimental attitude towards humans. Personal relations may remain what they were (though it is unlikely that they will) but they are now entirely private, lacking in substance to influence important political matters. Tendencies such as these already exist, they inspire pedagogic as well as military programmes and they were foreseen by A. Huxley in his Brave New World. I conclude that ‘knowledge in itself’, far from being inherently good is one of the most dangerous things in the world. It can aid us; but, interpreted in the wrong way, it can also turn the entire world into a monster.

**On Feyerabend’s “On the Responsibility of Scientists”**

 From early in Feyerabend’s corpus, he recognized the importance of ethical decisions in scientific epistemology (see e.g., Feyerabend 1961/1981, 71). In his mature and later corpus, the political dimensions of science become a much more prominent theme. He wrote extensively on expertise, science funding, and the relationship(s) between science and society. “On the Responsibility of Scientists” (henceforth, ORS), recently discovered in the Feyerabend archive at the University of Konstanz, expresses one of Feyerabend’s most direct encounters with these topics. ORS not only reverberates with themes from Feyerabend’s published corpus, but provides sharper formulations of key claims and novel considerations, arguments, and examples. The manuscript was undated, making precise dating difficult. The fact that it was produced on a dot-matrix printer and the reference to “Starwars” suggest that the earliest dating would be the mid-to-late 1980s. The reference to the “enormous amount of money” spent on mapping the human genome suggests that a likely dating would be the early 1990s, as the Human Genome Project began in 1990.[[1]](#footnote-1) Here, we situate ORS within his broader corpus and contemporary literature on science and values.

**1.** **Continuities with Feyerabend’s Publications**

While Feyerabend’s earlier works are peppered with acknowledgments of the ethical and political dimensions of epistemology (e.g., Feyerabend 1961/1981, 71; 1963, 319-320), we see much more sustained treatments of several topics on the politics of science in his mature and later writings (Tsou 2025). Here, we cover the most prominent ones to contextualize ORS.

**1.1 Expertise**

Feyerabend’s first detailed treatment of expertise comes in “Experts in a Free Society” (Feyerabend 1970/1999). Here, he argues for an epistemic egalitarianism, where experts have no special authority, and that expertise is often exaggerated and entrenched through needless technical idioms. In “How to Defend Society Against Science,” he further claims that “there must be a *formal separation between state and science*… Science may influence society but only to the extent to which any political or other pressure group is permitted to influence society” (Feyerabend 1975/1999, 187). Within *Science in a Free Society*, he clarifies that “[e]xpert opinion will of course be taken into consideration, but experts will not have the last word. The last word is the decision of democratically constituted committees, and in these committees laymen have the upper hand” (Feyerabend 1978, 87). This continues his arguments against expertise, calling most experts “prejudiced” and “untrustworthy” (88). Despite using the term ‘expert’, it isn’t even clear that Feyerabend thinks that such a coherent entity exists.

Feyerabend later distanced himself from *Science in a Free Society* (Preston 2020). In an interview, he claims that experts are a necessary evil since “the droppings of science are all over the place.” In a letter to Gonzalo Munévar, he writes:

I agree with you that citizens must be made acquainted with the sciences not because the sciences are so excellent but because science plays a large role and they will not be able to act as responsible citizens unless they know this important institution well: if a country is invaded by locusts it will be of advantage if everybody learns about locusts but it would be most foolish now to say that locusts are the only animal in existence.[[2]](#footnote-2)

This specifies Feyerabend’s stance towards expertise. Rather than denouncing scientific expertise altogether, Feyerabend reluctantly accepted their existence and began to assign role-responsibilities. Specifically, in ORS, he claims that experts must consider their social situatedness and, if they are close to a “battleline,” they should consider the moral consequences of their research and act accordingly.

 In ORS, Feyerabend further claims thats expert disagreements should be resolved by *political* procedures. Expanding on his published remarks (see Shaw 2021a), Feyerabend analyzes the politics of bandwagon effects. Feyerabend is most likely drawing from Robert Merton’s (Merton 1968) work on the Matthew effect. But research by others, especially Joan Fujimura (1987, 1988), was being conducted around the same time as ORS and speaks more directly to Feyerabend’s example of molecular biology. According to Fujimura, bandwagon effects emerge despite highly ambiguous and uncertain epistemic beginnings. Crucially, they form over career opportunities such as chances to use “hot” new technologies (Fujimura 1988, 270). It remains unclear whether Feyerabend was aware of Fujimura’s work, however it would not be surprising given that she completed her PhD at UC Berkeley in 1986 (Fujimura 1986), when Feyerabend was splitting his time between Berkeley and ETH Zurich. Recent literature has vindicated the position that bandwagon effects have such epistemically shaky beginnings, so Feyerabend’s analysis remains pertinent (Bol et al. 2018).

 This complements his analysis of Traditional Chinese Medicine (TCM), where TCM faded in clinical practice and universities by a conscious effort of Marxist ‘materialists’ who wanted to imitate ‘Western medicine’ (Feyerabend 1975, 50-51; 1978, 102-103). Mao forced TCM back into practice which led to many epistemic and social fruits (Shaw 2021b).[[3]](#footnote-3) While Feyerabend condemns Mao’s tactics, he praises the push towards pluralism. Here, Feyerabend recognizes the social and political dynamics of pluralism within science and the necessity of ‘outside’ pressures for promoting pluralism. This remains relevant, as recent studies suggest that increasing epistemic freedom does not increase diversity (Whitley et al. 2018).

Feyerabend’s contention that resolutions amongst experts are political is intriguing. Oftentimes, debates about ‘who counts’ as an expert involves partitioning ontological domains according to domains of expertise. Feyerabend rejects this as a coherent strategy. Expertise assumes that particular domains can be understood by exclusive uses of theories and methods within a particular discipline. This is false, given Feyerabend’s open-ended pluralism: knowledge requires engagement with a wide range of theories and methods, so specialization inherently involves a truncated perspective. This is why ‘experts’ do not significantly advance science (Feyerabend 1970/1999, 112) or have knowledge in Feyerabend’s sense; by missing out on the forest, they are also missing out on the trees. Rather, Feyerabend contends that expert disagreement resolutions are political, where the framing of problems determines what counts as expertise, a point in keeping with more recent literature on this topic (Irwin 2001; Douglas 2021).

**1.2 Democratization of Science**

The suggestion of using ‘science courts’ is also defended in *Science in a Free Society*. He writes,

That the errors of specialists can be discovered by ordinary people provided they are prepared to ‘do some hard work’ is the basic assumption of any trial by jury. The law demands that experts be cross-examined and that their testimony be subjected to the judgment of a jury. In making this demand it assumes that experts are human after all, that they make mistakes, even right in the centre of their speciality… [and] that their expertise is not as inaccessible as they often insinuate. And it also assumes that a layman can acquire the knowledge necessary for understanding their procedures and finding their mistakes. This assumption is confirmed in trial after trial. Conceited and intimidating scholars… are tripped up by a lawyer who has the talent to look through the most impressive piece of jargon and to expose the uncertainty, indefiniteness, the monumental ignorance behind the most dazzling display of omniscience: *science is not beyond the shrewdness of the human race*. I suggest that this shrewdness be applied to all important social matters which are now in the hands of experts (Feyerabend 1978, 97-98).

This is a social conception of expertise, where decisions emerge from structured dialogues. The evidence Feyerabend cites for optimism comes from histories of lawyers exposing scientific experts in ways that are accessible to laypeople. The analogy of a courtroom further implies that the goal is not ‘truth’, a value Feyerabend was skeptical of, but actionable decisions in accordance with value-laden conventions (Haack 2017). Some scholars working in a Feyerabendian vein have expanded on this analogy in greater depth with more up-to-date considerations (Pamuk 2021).[[4]](#footnote-4)

**1.3:** **Opportunism**

Opportunism, for Feyerabend, is a willingness to exploit or adapt to new situations with the hopes of perpetuating one’s own agenda. “In many ways a good scientist has to be like a politician who possesses an intuitive grasp of the objective situation and of the mood of his audience, and who has to make the best of both if he wants to get his views across” (Feyerabend 1970/1999, 123). Opportunism is connected to the epistemic freedoms permitted in *Against Method*, where Feyerabend claims that “I am convinced that Mankind, and even Science, will profit from everybody doing his own thing” (Feyerabend 1975, 215).[[5]](#footnote-5)

Feyerabend’s remark in ORS, though brief, suggests a novel perspective on opportunism. Here, he notes that scientific opportunism can conflict with the needs of society and that the needs of society should trump scientific opportunism. While Feyerabend was aware of the ethical contours of opportunism from the beginning, in many passages he praises opportunism while discussing *scientific* progress. While the textual evidence on this point seems indecisive, ORS reflects either an abandonment or refinement of his position that opportunism is beneficial. This, coupled with his denouncements of scientific imperialism (Feyerabend 1987), suggests that Feyerabend’s focus on opportunism in the context of politics of science involves not only a change in topics but a novel perspective. He claims that “[s]cientific method is a part of a general theory of man. It receives its rules from this theory and is built up in accordance with our ideas of a worthwhile human existence” (Feyerabend 1981, 67). This, as Tsou (2025) argues, is a part of a shift of Feyerabend’s thought, although this interpretative point remains contentious.

**1.4:** **Funding and Pluralism**

Feyerabend raises the question of whether we should finance scientific opportunism or research that is “closer in content and spirit” to the “burning problems” of the time (cf. Shaw 2022). This is a false dichotomy, a point Feyerabend recognized in his Trieste lectures(Feyerabend, 2011, 43-44). The deeper point is that Feyerabend rejects the freedom of scientists to do what they see fit with public funds. Policy-wise, Feyerabend supported the Baumann amendment that proposed additional Congressional oversight of budgetary decisions at the National Science Foundation (Feyerabend 1976, fn. 2 390). In ORS, he claims that implicit political expectations of research programs play important roles in determining their value. ‘Genetic tinkering’ as a solution to life’s problems, for example, should be discussed democratically. Thus scientists are often not the best estimators of the social value of their own research, making them even less trustworthy to control the fate of their own pursuits.

Feyerabend further argues that all research programs are limited, and often a divergent yet complementary pluralism is necessary (Oliveira 2021). We also see a glimpse of the anti-reductionism prominent in his later works when he says that “life is far too complex to be reduced to a few traits.” This provides an ontological argument for pluralism. This much is unsurprising for Feyerabend aficionados. What is puzzling in ORS is Feyerabend’s contention that successful alternatives can make some expectations ‘unreasonable.’ This seems inconsistent with Feyerabend’s recurrent pronouncements that inquiry is unpredictable, and seemingly defeated research programs can make comebacks (Feyerabend 1975, 189; 1987, 263; 1999, 138). Specifically, he repeatedly claims that it is ‘reasonable’ to pursue research regardless of the current epistemic state of affairs, making it unclear why the presence of successful alternatives should make some pursuits ‘unreasonable.’ Feyerabend may have been unaware of this tension, leaving him conflicted about the implications of pluralism for science funding policy.

**1.5:** **The Value of Knowledge**

Finally, Feyerabend claims that knowledge, rather than being intrinsically good, can be one of the most “dangerous things in the world.” This is implied in print. For example, he writes

It is of course *not* true that we *have* to follow the truth. Human life is guided by many ideas. Truth is one of them. Freedom and mental independence are others. If Truth, as conceived by some ideologists, conflicts with freedom we have a *choice*… My criticism of modern science is that it inhibits freedom of thought. If the reason is that it has found the truth and now follows it then I would say that there are better things than first finding, then following such a monster (Feyerabend 1975/1999, 183).

Here, Feyerabend states that there are personal reasons for resisting scientific worldviews even if they are ‘true’. In ORS, Feyerabend is more straightforward in highlighting the cultural dangers of theoretical knowledge. Most discussions about the ethics of science focus on its practice or implementation in technological artifacts (Kline 1995). While Feyerabend was aware of these dimensions, the *existential* dimension of scientific knowledge was especially important for him. His primary value is intellectual freedom (which coincides with happiness) (see e.g., Feyerabend 1970, 209), which involves making conscious choices about how to live and what to believe. This forms the basis of “Aristotle’s Principle,” where “*real is what plays a central role in the kind of life we identify with*” (Feyerabend 1999, 201). Since science is one tradition amongst many, there is nothing that compels anyone to accept its metaphysics over alternatives. The increasing cultural influence of science deeply troubled Feyerabend, and, in ORS, he puts the hefty responsibility of managing this influence on scientists.

Feyerabend uses the example of the Copernican revolution in print). Here, he defends the Church’s censoring of Galileo’s promotion of a realistic interpretation of the Copernican model. This defense is partially empirical, but the more important motivation was protecting citizens. Since Copernicanism contradicted Scripture, and it was assumed that Scripture as interpreted by the Roman Catholic Church “adumbrate[d] a correct account of a well-rounded and sanctified life” (Feyerabend 1975/1988, 135), the Church had “the right social intention, viz. to protect people from the machinations of specialists” (137). Feyerabend goes further in “Galileo and the Tyranny of Truth”:

Thus the church was not only on the right track when measuring reality by human concerns but it was considerably more rational than some modern scientists and philosophers who draw a sharp distinction between facts and values and then take it for granted that the only way of arriving at facts and, therefore, reality is to accept the values of science (Feyerabend 1987, 253).

He then argues that there are contemporary analogues with laws, norms, and institutions that control the ways science influences society. For example, public policies that allow evolution to be taught as a fact to schoolchildren, while the account of Genesis is taught merely as something some people believe, impact how science influences society. This ethical-existential dimension of scientific knowledge is central to Feyerabend’s vision of free and happy people and is somewhat independent from action-oriented considerations of predict-and-control applications of science. Knowledge is not intrinsically good because it increases ‘power’ or ‘understanding.’ Because it can change who we are and how we understand ourselves, we should consider such issues with our eyes wide open, rather than falling prey to the cultural hegemony of science.

**2. Feyerabend, Science, and Values**

We have already seen some of the ways in which Feyerabend’s essay anticipates subsequent trends in the literature on science and values. Let us consider in more detail how this literature provides resources for elaborating on his concerns. His essay focuses on two main issues: “(a) running science properly and (b) taking care that dangerous results are kept from the rest of society.” Regarding (a), he offers three reasons for questioning the attitude that “science knows best” and that it should be policed solely by its own experts. First, he notes that experts commonly disagree, so one cannot merely “trust science”; instead, one has to decide which experts to trust. Second, the research projects that are most appealing from a narrowly scientific perspective may not be those that are most beneficial for society. Third, seemingly unpromising lines of scientific investigation can provide promising critiques of dominant scientific research programs. These points foreshadow a great deal of subsequent work on values and science that serves to extend Feyerabend’s thinking.

**2.1: Expert Disagreement**

Building on Feyerabend’s point that experts commonly disagree, subsequent scholarship has highlighted a wide array of ways in which scientific reasoning can legitimately incorporate both epistemic and non-epistemic values, thereby generating reasonable opportunities for experts to disagree.[[6]](#footnote-6) For example, experts can adopt different value-laden background assumptions (Longino 1990), or adopt different standards of evidence for drawing conclusions (Douglas 2009; Elliott and Richards 2017), or accept different aims of inquiry (Elliott and McKaughan 2014; Intemann 2015). Feyerabend is right to point out that this potential for reasonable disagreement in science generates important challenges for society. If the scientific community is left to its own devices, it raises the specter of technocracy: the values of the scientific community will receive precedence over the values of other members of society (Betz 2013).

Scholars working on science and values have proposed various solutions to this difficulty. One solution is to develop strategies for keeping scientific reasoning free of value influences.[[7]](#footnote-7) For example, one might insist that scientists should hedge their claims so that they draw only those conclusions that everyone could agree on (Betz 2013; 2017). Unfortunately, it is doubtful that this strategy could be implemented effectively (Elliott 2022; Havstad and Brown 2017). An alternative solution that has received increasing attention is to develop a “political” philosophy of science (see e.g., Kitcher 2001; Lusk 2021; Schroeder 2021; Thoma 2024). Broadly speaking, the goal of this effort is to develop a legitimate political approach for identifying the appropriate values to be brought into the practice of science. This effort appears to align well with Feyerabend’s goal of incorporating societal influences into science so that experts are not left to their own devices. And although much work is still needed to develop a theoretically sophisticated political philosophy of science, a variety of efforts are already underway to incorporate greater input from non-specialists in scientific practice. For example, a great deal of recent literature in the field of science, technology, and society has shown how non-specialists can highlight problematic assumptions in scientific work and help to improve it (e.g., Epstein 1996; Powell and Powell 2011; Wynne 1989), thereby vindicating Feyerabend’s contention that “the errors of specialists can be discovered by ordinary people” (see the quotation from *Science in a Free Society* provided above in Section 1.2). Indeed, the explosion of recent work on community-based participatory research and citizen science is a testament to the ability of “ordinary people” to contribute productively to science (see e.g., Corburn 2005; Elliott and Rosenberg 2019). By the early 1990s, when ORS may have been written, this confidence in the epistemic prowess of “ordinary people” was beginning to take off, especially via works Feyerabend cites repeatedly, such as Meehan (1984).

Feyerabend specifically suggests that something akin to a science court may be warranted as a way to incorporate public input on science, and although he may be idealizing the process to some extent (see Selinger 2003), this idea has received more recent uptake and defense. For example, building on earlier proposals that Arthur Kantrowitz developed in the 1960s and 1970s (see Kantrowitz 1967; 1976). Justin Biddle (2013) has proposed a system of Adversarial Proceedings for the Evaluation of Pharmaceuticals (APEP). Biddle suggests that two groups of advocates could present opposing perspectives on debated issues related to pharmaceutical research, and a group of judges could adjudicate between them. Although Biddle (following Kantrowitz) proposes that the judges would be scientists, he notes that the APEP could be implemented in the context of a public forum to facilitate public input on the proceedings. He also notes that one could include community members as judges in some cases (see also Shrader-Frechette 1985). Others have proposed that a science court or a similar deliberative body involving scientists could be accompanied by a consensus conference or a citizen jury involving lay people to incorporate even more voices from non-specialists (see e.g., Elliott 2011, 128).

**2.2: Scientific Opportunism**

 Feyerabend’s second reason for resisting the notion that scientists can police themselves is that the research projects that are most appealing from a scientific perspective are not always those that are best suited for addressing social issues from a “holistic” perspective. This point also resonates extremely well with subsequent themes from the literature on science and values. Perhaps this theme is exemplified best in the work of Hugh Lacey (1999, 2001), who has emphasized that scientists can adopt different “research strategies” that serve different social values. For Lacey, research strategies specify what kinds of data to collect and what sorts of hypotheses and theories to propose. He frequently uses the example of agricultural research, where he points out that the dominant research strategy in agriculture is to focus on manipulating the genetics of seeds in an effort to produce crops that generate maximum yields under ideal conditions of irrigation, fertilizer, and pesticide use. Aligned with Feyerabend on this matter, Lacey (2001) notes that multicultural science better addresses socially significant phenomena (e.g., crop production). This approach draws attention to values from different cultures, and consequently, to alternatives from “traditional knowledge” (with “agroecological strategies”) that differ from those of the mainstream agricultural strategy, led by a “materialist strategy” (231-2). Quoting Feyerabend, Lacey (2001) notes that different values in modern agriculture (like turning seeds into commodities) and traditional knowledge (such as human well-being and sustainability) could be fruitful through cooperation. Lacey calls it a “fruitful strategy” that has “clear affinities with several of Feyerabend’s themes” (232) and argues that research strategies that initially appear to be in tension can both (potentially) be fruitful if we “adopt the one that may produce understanding of significance for the cultural, moral and social values one holds” (232-3). The idea is that as scientific and traditional knowledge interacts, “such proliferation and multiplicity serve to gain access to possibilities that are important for ‘humanitarian’ and ‘democratic’ reasons, but which are otherwise inaccessible” (232-3). According to Lacey, the dominant agricultural strategy tends to serve the values of capital and the market, whereas alternative strategies might serve a broader array of ethical and social values (see e.g., Lacey 2017).[[8]](#footnote-8)

Another example of how dominant scientific approaches can fail to serve social values comes from chemical risk assessment. In the 1970s and 1980s, regulatory agencies around the world enshrined risk assessment as their primary tool for regulating toxic chemicals. Risk assessment is a highly technical endeavor that requires extensive data inputs and complicated scientific analysis. However, this technical complexity can easily disguise a wide array of value-laden judgments that play unavoidable roles in the risk-assessment process and that can be covertly steered to serve particular interests, such as those of the chemical industry (see e.g., Elliott 2016; Shrader-Frechette 1991). Moreover, risk assessment is typically a very time-consuming endeavor that requires a great deal of effort to complete (Cranor 1993; Elliott and McKaughan 2014). Therefore, if regulatory agencies allow chemicals to be placed on the market without requiring proof of their safety and then require that risk assessments be performed in order to pull them from the market, this seemingly beneficial and technically impressive scientific endeavor can actually become a significant barrier to protecting the public from chemical pollution. As William Boyd puts it in an article for the *Harvard Law Review*, “risk assessment has operated first and foremost as a political technology intended to discipline the way agencies make facts and produce knowledge about harms–all as part of a broader distributional struggle over which harms will be imposed on which groups of people. At a basic level… one could explain the rise of quantitative risk assessment largely as a triumph of class interests” (2024, 243). This perspective on risk assessment aligns very well with Feyerabend’s contention that dominant scientific approaches are not neutral but can instead shape society in ways that serve particular interests, values, or worldviews.

Harkening back to the discussion above about the role of non-specialists in responding to expert disagreement, one way to respond to the dangers of scientific opportunism is to create collaborations between scientists and community members who can influence the direction of research (Douglas 2005). Indeed, as noted previously, scientific approaches that allow community members to participate in and guide research projects (e.g., community-based participatory research, participatory action research, or citizen science) are currently very popular (Cavalier and Kennedy 2016; Elliott and Rosenberg 2019). These collaborative approaches can be seen as a direct reaction to the worry that Feyerabend expressed so well in his essay: when scientists give precedence to their own preferred investigations, they tend to dismiss “approaches dealing with how social change looks to the people who experience it.” By collaborating with those who have “on the ground” experiences of problems, scientists are able to take more holistic approaches that evaluate and address those problems more effectively, their values. There is now growing recognition across many areas of science that the best solutions to social problems often stem not from “flashy,” high-profile research projects but rather from local, problem-oriented studies that incorporate community engagement (Toomey 2024). Community engagement is of course not a panacea; it can run into difficulties as well (Bauer et al. 2021; Repo and Matschoss 2019). However, providing greater training and guidelines for scientists and strategic educational initiatives for community members can hopefully lead to more effective, strategic collaborations (Oliveira 2024).

**2.3: Scientific Dissent**

 Feyerabend’s third reason for rejecting the view that “science knows best” is that new lines of investigation (even when they seem unpromising) can help to critique dominant scientific research programs. This observation accords very well with themes from the contemporary literature on scientific dissent. Prominent philosophers of science like Helen Longino have argued that dissenting voices “must be cultivated” (Longino 2002, 132) to promote objectivity in science. Longino, like Feyerabend, is especially eager to promote diversity that in turn facilitates “transformative criticism” of the fundamental norms or beliefs of particular communities (see also Solomon 2001). The neuroscientist Stuart Firestein (2012) has similarly called for dominant scientific approaches to be subjected to critique. Nevertheless, the philosophical literature on dissent is motivated by the recognition that unfettered dissent can also cause major social challenges. Even Feyerabend might be more hesitant about promoting dissent when faced with the contemporary misinformation campaigns about topics like climate change, COVID, and the effects of industrial pollution (see e.g., Michaels 2008; O’Connor and Weatherall 2019; Oreskes and Conway 2010; Shaw 2021a). In an effort to navigate these tensions, some scholars have proposed criteria for distinguishing legitimate forms of dissent from illegitimate forms of dissent (e.g., Biddle and Leuschner 2015; Miller 2021). Nevertheless, it is not clear that these criteria are successful (de Melo-Martín and Intemann 2018). Moving forward, we clearly need to be exploring creative ways to navigate scientific dissent in a responsible fashion (Douglas 2023; Elliott 2023), and it is possible that this literature could help to alleviate some of the tensions in Feyerabend’s own work on scientific pluralism and dissent.

**2.4: Social Consequences of Scientific Knowledge**

The second main point in Feyerabend’s essay is that scientists need to take responsibility for the impacts of their work on society. This claim takes on extraordinary significance in the wake of the global COVID pandemic, which may have been generated by a lab leak (Resnik 2024; Thacker 2021). And even if the COVID pandemic was not caused by errors in laboratory safety, there is increasing recognition that the scientific community needs to exert greater care when researching pathogens of pandemic potential (PPPs; Resnik 2024). Along the same lines as Feyerabend, Heather Douglas motivated her pioneering work on science and values by insisting that scientists have ethical responsibilities not to cause reckless or negligent harm to others (Douglas 2003; Douglas 2009). Therefore, given that scientists can cause significant harm to others when they make false positive or false negative errors, Douglas insisted that they have ethical responsibilities to take those impacts into account when setting their standards of evidence (Douglas 2009; Douglas 2021). Moreover, like Feyerabend, she rejected a sharp distinction between “knowledge and its application,” or between “basic” and “applied” research (Douglas 2009; Douglas and Branch 2024). Thus, she made the same move of denying that any part of science could be completely insulated from social responsibilities.

As discussed above, Feyerabend also claims that knowledge is not as benevolent as generally assumed and that scientific knowledge can change our self-conceptions in ways that can be highly damaging. Philip Kitcher explored this same claim in detail in his influential book, *Science, Truth, and Democracy* (Kitcher 2001). Kitcher illustrated his concerns by discussing the challenges that Victorians faced as they grappled with the work of Darwin. For example, he cites the case of George John Romanes, who experienced “anguish” because he was drawn to two claims: “first that the evidence of the life and earth sciences refutes the doctrines of Christianity (and other major world religions), and second that without the consolations of religious belief life is shrunken and meaningless” (150-151). Kitcher noted that one could respond to cases like this by insisting that people should be able to handle these sorts of “subversive truths.” However, he challenged those who took this line to consider how personally and socially harmful some kinds of scientific discoveries could be. For example, imagine if we discovered that people’s apparent love for each other was actually caused by unconscious and manipulative factors, or that all our actions are merely consequences of simple physiological processes, or that we can have successful relationships with each other only if we accept myths about ourselves (151-152). After analyzing a variety of arguments in favor of the notion that it is best to go ahead and pursue subversive truths even when they have the potential to harm society, Kitcher came to a conclusion very much in line with the views of Feyerabend:[[9]](#footnote-9)

I have attempted a systematic survey of all the possibilities for showing that “truth is better than much profit” and have come up empty. Indeed, what is most striking is the fact that, when articulated, the possible lines of defense are so unpromising (166).

**3. Conclusion**

 Feyerabend’s thought deserves an important place in the literature on science and values. His ideas not only anticipate many contemporary themes but also provide promising directions for further reflection. His views on the roles of experts, public participation, dissent and pluralism in science, and the value of knowledge were radical and provocative, and they continue to reverberate in much of the contemporary literature on science, politics, and values. Hugh Lacey, one of the pioneering figures in science and values, argues (pers. comm.) that Feyerabend’s work played a major role in changing the atmosphere within the philosophy of science so that scholarship related to values could find a home in the discipline. Further research connecting Feyerabend’s political philosophy of science to current research on science and values seems like an extremely fruitful avenue for future exploration.

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1. Thanks to an anonymous reviewer for helping us narrow down this date. [↑](#footnote-ref-1)
2. From the Feyerabend archive at the University Konstanz, PF 17-4-1. While it is undated, Munévar estimates, with good reasons, that it was written in mid-1979 (personal communication) [↑](#footnote-ref-2)
3. History is currently repeating itself in Iran, where local traditional medical practices are being enforced against the ‘Westernization’ of medicine. [↑](#footnote-ref-3)
4. To be clear, while influenced by Feyerabend in various ways, Pamuk’s view on science courts was developed independently of Feyerabend’s (personal communication). [↑](#footnote-ref-4)
5. The claim that scientists should be left alone remains in the 2nd and subsequent editions of *Against Method*. This is confusing because the sentence was edited (see Feyerabend 1975/1988, 165), so the continuity is likely not an editorial oversight, but still might be editorial carelessness. [↑](#footnote-ref-5)
6. Many figures in the science and values literature question whether there are clear distinctions between epistemic and non-epistemic values (see e.g., Longino 1996; Rooney 2017). If one were to reject this distinction, it would merely strengthen the argument that experts can reasonably disagree. [↑](#footnote-ref-6)
7. The literature has generally focused on excluding non-epistemic values from scientific reasoning while regarding epistemic value influences as unproblematic. Nevertheless, it is worth considering the possibility that epistemic value influences still raise concerns about technocracy. If experts can reasonably disagree about which epistemic values are most compelling as well as how to weigh and interpret them, and if those decisions in turn influence scientists’ acceptance of claims that impact society, then the public could reasonably worry about those epistemic value influences. Why allow experts sole control over debatable decisions that impact the public? [↑](#footnote-ref-7)
8. In personal communication on 11/23/24, Lacey noted that even though he does not recollect directly obtaining any of his core ideas from Feyerabend, the affinities between their views is surely not entirely accidental, given that he read everything he could find written by Feyerabend and included Feyerabend’s work “in virtually every course” he gave between 1965 and 2003. [↑](#footnote-ref-8)
9. This parallel is, most likely, uncoincidental: “it seems to me that many of [Feyerabend’s] insights about the roles of the sciences in society have been unjustly neglected by philosophers” (Kitcher 2011a, fn. 2 122). For Kitcher’s comparison between his political philosophy of science and Feyerabend’s, see Kitcher (2011b). [↑](#footnote-ref-9)