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Better Red than Dead—Putting an End to the Social Irrelevance of Postwar Philosophy of Science

Don Howard

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Abstract This paper asks what is necessary in a theory of science adequate to the task of empowering philosophers of science to participate in public debate about science in a social context. It is argued that an adequate theory of science must be capable of theorizing the role of values and motives in science and that it must take seriously the irreducibly social nature of scientific knowledge.

1 Introduction: Reviving a Philosophy of Science in Society

There is no more interesting nor sobering chapter in the history of 20th-century philosophy of science than that which tells the story of the discipline's disengagement at mid-century from the social and political concerns that shaped its earlier years. In Europe, near the century's start, conservative Catholics like Pierre Duhem,[1] social democrats like Ernst Mach,[2] and revisionist Marxists like Otto Neurath[3] all understood that science was central to the modernist outlook then asserting its cultural authority and that a philosophy of science must, therefore, among other tasks, theorize the manner in which science is embedded in a social, cultural, and political context and the manner in which it contributes to the transformation of the world.

Agendas differed. Duhem wanted to secure a place for Catholicism in an ever more secular world by circumscribing the domain within which science could assert its authority and by demonstrating, historically, that the cultural authority of the Church is not necessarily

[1] Brenner (1990) and Martin (1991) are good sources on Duhem's work in context. For a more partisan view, see Jaki (1987). For more general background, see Brenner (2003).

[2] Blackmore (1972) is still the best general introduction to Mach's work in context.

[3] The literature on Neurath has blossomed in the last twenty years. Among the better sources are Cartwright et al. (1996), Nemeth and Stadler (1996), and Uebel (1992, 2000). For general background on logical empiricism, see Stadler (1997).

D. Howard (✉)
Philosophy, University of Notre Dame, 100 Malloy Hall, Notre Dame, IN 46556, USA
e-mail: dhoward1@nd.edu

inimical to the growth of science. Neurath, the socialist critic of the baleful influence of social, religious, economic, and political dogma, wanted a properly philosophized science to play the leading role in progressive social and political reform (Neurath 1930).

Theories differed, as well, especially concerning the place of "non-cognitive" factors in theory choice.[4] Neurath spoke for the Vienna Circle's "left wing" in drawing from an underdeterminationist and holist picture of theory choice the lesson that "auxiliary motives," including political agendas, play an inescapable role in science and so should, themselves, be subjects of empirical study and public debate. In Neurath's lexicon, a theory of science that denied the role of such auxiliary motives committed the sin of "pseudo-rationalism" (Neurath 1913). Hans Reichenbach spoke for a "right wing" that discounted the role of non-cognitive factors in the "context of justification," relegating their work to the "context of discovery," but held, nonetheless, in the typical Enlightenment trope, that the disinterested pursuit of scientific truth, for its own sake, would tend to promote human emancipation and the betterment of the human condition (Reichenbach 1938).[5] Duhem (1906) agreed with Neurath that theory choice is empirically underdetermined, but he mystified the workings of the non-empirical moment in theory choice under the heading of a "bon sens" that could be trusted to lead scientists, in the long run, to the "natural classification."

That a philosophy of science made an important difference in the political arena was proven once by the fact that Lenin devoted a whole book, *Materialism and Empirio-Criticism* (1909), to attacking as a disguised form of idealism—hence not materialism—the Machist variant of Marxism that was growing in popularity in Austria and Russia at the beginning of the 20th century. It was proven again by the fact that the book that earned the refugee Karl Popper his influential place in British philosophy of science at mid-century was not his as yet untranslated *Logik der Forschung* (1935) but his philosophical critique of Marxism, *The Open Society and Its Enemies* (1945).[6]

North American philosophers spoke a different idiom, but the question about the role that science would play in the modern world was much the same. Conservatives like the neo-Thomist Mortimer Adler asserted the value neutrality of science, or rather the impotence of science in the face of questions of value, for the purpose of denying science a leading role in culture, a role that might be filled, instead, by religion. Progressives like the pragmatist and evolutionary naturalist John Dewey argued that valuation played a crucial role in all cognizing, including the "organized intelligence" that we call science, and that science, in turn, had a role to play even in the selection of ends. Liberals like the scientific rationalists Morris Raphael Cohen and Ernest Nagel championed the Enlightenment view of disinterested science as the engine of material and, perhaps, also social progress.[7]

On both sides of the Atlantic, the arguments could become quite rarified, with philosophers doing what they are trained to do, examining the fine structure of scientific cognition, its origins, nature, and limits. Of particular import were questions about the manner in which and the extent to which reason and experience constrain the scientist's

[4] The scare quotation marks around "non-cognitive" are to remind the reader that casual use of the expression begs important philosophical questions by implying that values and motives are not integral to some presumably "pure" form of cognition. The ineliminable, often constructive role of values and motives in cognition was a point of great importance to philosophers like Neurath and John Dewey.

[5] For a recent reassessment of the discovery-justification distinction, see Schickore and Steinle (2006).

[6] The English version of *Logik der Forschung* appeared only in 1959 (Popper 1959), long after Popper had become the loudest voice in post-war British philosophy of science.

[7] For more on the North American background, see Reisch (2005), Howard (2003), and McCumber (2001). On Dewey, in particular, see Ryan (1995); on Cohen, see Hollinger (1975).

theorizing, questions about whether reason has non-empirical avenues of access to truth, and questions about reason's role in the moral life. But it was understood that, with even the most abstruse questions, matters of major cultural and political moment depended upon the answers. Thus, arguments for the integrity of a priori metaphysics were understood as being aimed, in some instances, at legitimating theological answers to social questions. And arguments for verificationism, the view that each, individual, primitive, scientific term can and should be given an exhaustive empirical definition, were understood as being aimed at insulating theory choice against the intrusion of non-empirical factors and thereby securing the independent intellectual authority of empirical science. Noteworthy is the fact that philosophers of science, qua philosophers, were alert to the social, cultural, and political implications of their work and saw it as a properly philosophical task to pursue those implications into the social and political arena.

And then it all changed. Hitler chased serious philosophy of science out of Europe in the 1930s. Émigré logical empiricists made new homes in North America. World War Two ended. After 1945, philosophers of science found themselves in a new intellectual setting, dominated by the Bomb, the Cold War, and the Red Scare. And by the end of the 1950s, thoughtful philosophical debate about the place of science in society had all but disappeared, replaced by a highly formalized philosophy of science pursued by a new generation of technically well trained young specialists whose inability to think carefully about science in context was disguised as disdain for irrelevant, non-technical questions.

How and why this change occurred is the subject of George Reisch's masterful historical study, *How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic* (2005). It is a surprising story, in some ways a shameful story, and it should be a humbling one. One is surprised to learn that J. Edgar Hoover thought philosophers of science like Philipp Frank and Rudolf Carnap to be worthy targets of FBI investigation. One is shamed to learn of the philosophical profession's failures to defend colleagues who were victims of political persecution.[8] One should be humbled to learn the extent to which a philosophical picture of science as immune to political interference was, itself, a product of political influence on and within the community of philosophers.

The story that Reisch tells is also a tragedy. The tragedy is that postwar philosophy of science lost the will and the ability to theorize the social role of science. But questions about science's social role remained, and others stepped forward to answer them. We now have apologists for theologically debatable religious interests telling the public that intelligent design is "scientific."[9] We have apologists for corporate greed telling the public that predictions about global warming or species extinction are not based on "sound science." Lobbyists are consulted about the place of public input in science policy making.[10] Lawyers are consulted about resolving conflicts in the courtroom testimony of "expert" "scientific" witnesses.[11] Historians of science are consulted about the wisdom of

[8] Philosophers of science were not alone in failing to defend persecuted colleagues during the 1950s. On the response of the general philosophical community, see McCumber (2001), and for a still broader perspective, see Schrecker (1986). The situation of the sciences during the McCarthy period is the subject of Wang (1999).

[9] Pennock (2001) is a very helpful collection of views on the intelligent design controversy. On the role of the religious right in other controversies about science policy, see Kaplan (2004).

[10] For more on the politics of the "sound science" debate and the growing role of corporate lobbyists in science policy formation, see Mooney (2006).

[11] Golan (2004) is provides a comprehensive history of expert scientific testimony.

massive investments in experimental particle physics.[12] Sociologists of science are consulted about funding strategies for restoring American competitiveness in science and technology.[13] One might have thought that philosophers of science, who are supposed to know more than anyone else about how science works and how it is demarcated from other ways of knowing, would play prominent roles in all such discussions. But with precious few exceptions, professional philosophers of science choose not to participate and their help is not routinely solicited.[14]

That there is a role for the philosopher of science to play is proven by the appallingly naive conceptions of science invoked in many of these discussions. In more than one case, the level of public debate would be raised by someone's simply reminding the discussants of the lesson of Humean inductive uncertainty. Preponderance of evidence, not definitive proof, is the proper standard in assessing the health effects of secondary smoke or the effect of human activity in global climate change. In other settings more advanced philosophical tools are called for, as in figuring out how to assess the risks associated with burying nuclear waste at Yucca Mountain, where the "philosophical" questions concern topics ranging from the methodological constraints on modeling groundwater movement to the logic of statistical inferences concerning the health effects of radiation exposure.[15]

What is required for philosophers of science again to enter the arena of public debate about science is, partly, just a shot of moxey. As a discipline, we lost our nerve during the witch hunts of the McCarthy period and, but for a few individuals, we never regained it. We retreated into the safe haven of socially disengaged, technical philosophy of science and there, for the most part, we remain. Defending science against unreasoning dogma will always expose one to attack from those whose social and political agendas are, thereby, put at risk. Such is the price to be paid if one does not want to surrender the world to greed and bigotry, and giving in to bullies only encourages them. But more than just moral courage is needed.

Needed, as well, is a philosophical engagement with problems within the philosophy of science that are impediments to our theorizing the place of science in society. The safe haven of a socially disengaged philosophy of science is secured by philosophical doctrine. Such doctrine requires a philosophical critique. To that I now turn.[16]

2 Theorizing the Place of Motives and Values in Science

Science in a social context is science influenced by values, motives, social interests, and political agendas. Therefore, a necessary condition for substantive participation by philosophers of science in public debate about science in society is a theory of science that addresses forthrightly the role of values and motives in science, rather than just dismissing

[12] Heilbron (1987) gives several examples of historians of science providing expert testimony regarding science policy.

[13] See the recent program solicitation from the National Science Foundation, "Science of Science and Innovation Policy" (SciSIP, NSF 07-547), http://www.nsf.gov/funding/pgm_summ.jsp?pims_id=501084

[14] One noteworthy exception is Susan Haack's recent work on science in the courtroom, Haack (2003a, b).

[15] Schrader-Frechette (1993) is a rare and praiseworthy example of a philosopher's substantive intervention in such public controversy. See also Schrader-Frechette (2007).

[16] Some philosophers of science have been thinking and arguing about the social dimensions of science for a long time; Longino (2006) is a helpful survey. But the question at issue here is that of developing a theory of science that empowers effective intervention in public debate.

them as irrelevant to science. If, in principle, values, personal motives, social agendas, and other non-cognitive factors play no role in theory choice, then philosophers of science—qua philosophers of science—have nothing to say about the place of motives and values in science, beyond consigning them to the context of discovery and ceding responsibility for thinking about them to historians, sociologists, and psychologists. Right-wing Vienna Circle logical empiricists were not alone in developing just such a picture of science, driven partly by the worry that allowing values and motives a role in theory choice risked opening the door to dogma, prejudice, and other species of unreason. Much later work on confirmation theory has evinced the same concern, though the failings of strict verificationism have become clear. Even falsificationist critics of the Vienna Circle, like Popper, were enchanted by the idea of a theory choice algorithm.

Neurath, as noted, derided such disavowals of a role for "auxiliary motives" as pseudo-rationalism (Neurath 1913, 1935). For Neurath, it was a contingent fact about science that such auxiliary motives were virtually always in play, especially in the social and economic sciences that were his main professional concern. How could it be otherwise if science is done by real human beings living and working in real human communities? That it was likely to be so was also, for Neurath, a straightforward consequence of the basic logic of theory holism and empirical underdetermination.[17] In general, logic and experience do not suffice to pick out a clear winner among a set of competing theories, so the proponent of underdetermination argues. But choose among theories we must—the imperatives of life compel us—from which it follows that we do choose on the basis of factors other than experience and logic. Criteria like simplicity might diminish the degree of underdetermination a little or a lot, but defending the epistemic character of simplicity without committing the sin of metaphysics might be hard, and, anyway, whatever its allure in physics, simplicity helps little in social theory. There remains a gap that is filled, in fact, according to Neurath, by habit, bias, dogma, or political agenda. Better, then, to make such motives the subject empirical investigation, make them topics of public debate, and honestly advertise their role in one's own theorizings. A science that is honest about the role of auxiliary motives is a more, not less, objective science.

It is no accident that, nearly a century later, questions about underdetermination still come to the fore when the issue of science and values is raised. If there is a theory choice algorithm, then there is no underdetermination and no role for non-cognitive factors in theory choice. If underdetermination is a fact about theory choice, then factors other than logic and experience must play a role in theory choice. The alternatives are clear, if the answer is not.

It might help to distinguish the question of near-term underdetermination from the question of long-term underdetermination, or, as Philip Kitcher puts it, between transient and permanent underdetermination (Kitcher 2001, 30). W. V. O. Quine famously defended the extreme thesis that underdetermination would remain even in the infinite long run of inquiry and even if we were possessed of the infinity of all possible evidence (whatever that means), one of Quine's reasons being that, like Neurath, he thought that underdetermination goes all the way down to the level of the observation sentences (Quine 1951, 1960). For Quine and Neurath there is no fixed, neutral, observational basis for science.

There might be no way to settle the argument about permanent, long-term underdetermination. The absence of a proof for permanent underdetermination is not proof of the absence of permanent underdetermination, nor is an induction from the commonplace

[17] Ben-Menahem (2006) is provides a helpful, critical appraisal of arguments concerning underdetermination and conventionalism, more generally.

phenomenon of new experimental results breaking theoretical ties, which is countered by an equally weak induction from the commonplace phenomenon of the proliferation of alternative, at least transiently empirically equivalent theories on the cutting edge of science.[18] But settling the question about permanent underdetermination might actually be irrelevant to the question about science and values, and not just because we're all going to be dead in the infinite long run.

Debates about long-term underdetermination are irrelevant to the question about science and values because science is done in the here and now, and examples of underdetermination are everywhere to be found in the sciences that make the most difference to human well being.[19] In another hundred years we'll surely know a lot more about how the global climate works. But we have to act now, which means that we have to make policy in a setting where the evidence for and against different climate models is not definitive. That is the human condition. As Neurath pointed out, one lost in a deep, dense wood is sure to die if one abides by canons of reason that counsel one's waiting for evidence concerning the surest way out. One's only chance of survival is to pick a direction, any direction, and walk resolutely on that heading (Neurath 1913).

What advice can the philosopher of science offer the lost wanderer? Do more research, to be sure, when and as one can. And should compelling new evidence suggest that one is headed in the wrong direction, a course correction might well be indicated. But otherwise, what does the philosopher of science have to say? Policymakers are not helped by being told that definitive evidence might someday be available. The world can wait a long time to decide whether "bumps" in the data from the D Zero detector at Fermilab pass the five-sigma test and so confirm the existence of the Higgs boson (Overbye 2007). We cannot wait as long to decide whether ocean temperature fluctuations are consistent with historical, decadal patterns of variation or, instead, signify human-induced perturbations that will produce a steady intensification, on average, of hurricanes and typhoons.[20] It is an evasion

[18] Kitcher's dismissal of the problem of permanent underdetermination (Kitcher 2001, 31–38) shares with other such (for example, Friedman 2001) the straw-man strategy of taking the thesis to assert that anything goes, that any of one's favored doctrines can be defended, come what may by way of future evidence, and that there are no rational grounds upon which to distinguish alternative, empirically equivalent theories. Some who use underdetermination arguments to impugn the objectivity of science might argue thusly, but sober, science-friendly proponents of strong underdetermination theses like Duhem, Neurath, and Quine do not. One is tempted to say to Kitcher, "pick on somebody your own size." Consider the Quinean version of the thesis. Quine emphasizes asymmetries in the "web of belief," such as relative resistance to revision varying with distance from the center of the web (we don't give up our logic easily), that provide the scientist with *rational reasons* for preferring one option to another. On this view, no principle is ever permanently immune to revision (why should it be?), but what drives the scientist's choices is not just whim or prejudice. Nor does Quine assert that anything goes. What he asserts is that a cherished thesis can always be retained, *as long as one is willing to make sufficiently drastic changes elsewhere in the web of belief*. The problem for the anything-goes relativist is that the price one has to pay to maintain one's pet principle is usually prohibitively high, as with denying what others see as obvious evidence. No proof from self-evident first principles will convict the persistent dogmatist of error in such cases, but it is one of several diseases peculiar to philosophers to think that only such proof suffices to secure the integrity of science. Why, in a contingent world, should one not be content with contingent safeguards for the integrity of science?

[19] Yet another factor complicating theory choice in the here and now is that the underdetermination often results not because two competing theories are empirically equivalent but because what one might term the "phenomenal domains" of the theories don't coincide. One theory might have implications with respect to questions about which another theory is mute. Judgments about which are the important questions to address represent another moment of theory choice in which auxiliary motives might play a big role.

[20] For contrasting views on the question see Pielke et al. (2005) and Mann and Emanuel (2006).

of responsibility if, in such circumstances, the philosopher of science says that the only important question is the question about permanent underdetermination.[21]

Another way the philosopher of science evades responsibility is by arguing that cases like global climate modeling exemplify not underdetermination but mere inductive uncertainty (which is, in essence, what Kitcher means by "transient" underdetermination). From one point of view, the difference between these two ways of characterizing evidential insufficiency might seem slight. Weighing policies based on two rival climate models, the policy maker seems to face much the same challenge whether there is a factual difference as yet not revealed by the evidence or there is no factual difference capable of revelation, perhaps because the evidence, itself, is indefinitely contestable, there being no neutral, observational basis. But the difference is, still, an important one, for identifying the insufficiency as inductive uncertainty encourages the hope of near-term empirical resolution of the dispute and draws attention away from non-empirical aspects of the policy maker's predicament, whereas viewing the insufficiency as an instance of underdetermination draws attention toward the auxiliary motives, the values, the agendas, interests, and biases that so complicate the policy maker's predicament.

By pointing to the role of auxiliary motives in science, Neurath meant to suggest that there is further work for the philosopher of science to do in understanding how auxiliary motives work. What kind of work? Consider some examples. Thomas Kuhn makes the point that the education (indoctrination? socialization?) of young scientists usually includes teaching them to advert to conventional models and problem solutions, not, or not necessarily, because the evidence favors those models but because mundane, work-a-day science proceeds only if there is a broad consensus about fundamentals (Kuhn 1962). Good, normal science more or less requires this kind of group-think and deference to custom. The social consequences are usually benign. But, in situations where solving pressing human problems requires radical innovations, the philosopher of science should be able to help us to distinguish between benign and harmful inertial effects of intellectual custom and to distinguish the crackpot from the scientist who is thinking "outside the box."[22]

An example of a different kind is provided by Helen Longino's analysis of the not-always-conscious gendering of work on behavioral neuroendocrinology (Longino 1990, 112–128). Here the potential social cost is greater if the effects of the auxiliary motives go unrecognized. Exposing those motives to view involves little more than showing that there is no conclusive evidence for the hypothesis that gender role behavior is influenced by pre-natal exposure to sex hormones (Doell and Longino 1988), the sort of work that philosophers of science do all the time. But the point is that the question was far less likely to have been raised in the first place (and was, in fact, not raised) by philosophers and endocrinologists in thrall to the pseudo-rationalist ideal of a science immune to bias. Philosophers (and scientists) not in thrall to that ideal are also well trained in precisely the kinds of analytic skills necessary for figuring out how far the evidence carries us and where other forces take over.

[21] The project on "Wissenschaftstheorie im Anwendungsbereich" ("Philosophy of Science in the Domain of Application") at Bielefeld University is a commendable if rare example of philosophers of science seeking to address the distinctive characteristics of applied science. Representative of the work emerging from this project is Carrier (2004).

[22] It was Albert Einstein who wrote, about the benefit of studying history and philosophy of science, "A knowledge of the historic and philosophical background gives that kind of independence from prejudices of his generation from which most scientists are suffering. This independence created by philosophical insight is—in my opinion—the mark of distinction between a mere artisan or specialist and a real seeker after truth" (Einstein to Robert Thornton, December 7, 1944, as quoted in Howard (2005), 34).

If, in addition, Neurath is right about the underdetermination going all the way down to the observation sentences, then attention to the work done by auxiliary motives conduces also to a healthy skepticism about evidence itself. More often than not, we can trust the institutions of science to generate evidence that is well vetted and reliable. Too often, however, and, again, especially in areas touching vital issues of human well-being, that is not so. How many harmful drugs have been brought to market after trials that supposedly proved their safety and effectiveness? Should we trust as evidence studies published only in journals funded by drug manufacturers and reviewed mainly by scientists on the payrolls of those same corporations? Surely not. And what about the problem of missing evidence? Do we trust our meat supply to be free of bovine spongiform encephalitis ("mad cow" disease) if we don't track and test every slaughtered cow? Choosing not to test is a social and economic choice.

To examples of the kinds just discussed, many will reply that they simply prove the point about the importance of eliminating from science all but what the evidence warrants. No one disputes the old empiricist ideal of letting the evidence do all of the work that it can, and everyone prefers evidence to prejudice. But the real challenge arises in those all-too-common cases of real, if transient, underdetermination in settings where action is required. How do we proceed when the evidence equally warrants importantly different theories and models? Can the philosopher of science help?

There are many ways in which the philosopher of science can help. If the philosopher of science respected as philosophical the perspectives afforded by history, sociology, and psychology, then the philosopher could bring to bear the lessons of history, sociology, and psychology to understand the forces moving individual scientists and whole communities this way and that. Surely history is relevant to an understanding of why neo-classical economists commonly commend market-based mechanisms when the evidence, such as it is, does not really settle the question of how best to remunerate low-skill service workers or how best to provide accessible, high-quality health care to the poor.[23] Surely sociology is relevant to an understanding of why there is so little dissent from neo-classical orthodoxy on such issues. Is it beyond the purview of the philosopher to intervene with suggestions that, in view of what the history and the sociology teach us, different solutions might be proffered with at least equal warrant?

If the philosopher of science respected as relevant to science the perspectives afforded by moral and political theory (which means regarding moral discourse as involving more than just grunts and groans), then the philosopher might also press the conversation in the direction of considerations of justice and fairness. If health care for the poor is the issue, empirical studies might not yet settle the question of whether a privately administered system or a government run system is most conducive to overall health and well being. More empirical studies might point us one way rather than another. But, in the meantime, is it not relevant and reasonable to ask whether one system is more just? Do we as philosophers not have many theoretical resources upon which to draw in pursuing the question of justice?[24] Is there not a distinctive role for the philosopher of science to play in

[23] See Mirowski (1989) for a critical discussion of the scientific pretensions of neo-classical economics.

[24] Fortunately, philosophers of science who think questions of justice relevant to a socialized theory of science have more resources upon which to draw than just the neo-liberalism of John Rawls (1971), which is Kitcher's main resource (Kitcher 2001). Those making policy in a contingent, real, social world pervaded by interests of many kinds are little helped by an ideal model of rational calculators operating behind the veil of ignorance in the original position, a model that Neurath probably would have seen as just another example of pseudo-rationalism. One needs tools for theorizing interest. In this one respect, at least, Marx has the advantage over Rawls.

sorting, connecting, and balancing questions of evidence and questions of morality? That the two kinds of questions can be deeply interconnected was a point made, famously, by Richard Rudner in (1953), just before the tide turned against such work on the integration of science and values. Rudner argued that, for example, value judgments might contribute to fixing a threshold for hypothesis acceptance or rejection within the framework of inductive logic, as when the moral cost of error drives a higher acceptance threshold for judgments of product safety (Rudner 1953).[25] Reason, or reasonableness, in settings such as this, is not modeled by inductive logic alone.

Asking what is reasonable points to yet another kind of intervention for which philosophers of science are well suited. Some might think that conflicts over values, motives, agendas, and the like are fated to be non-rational clashes of will or struggles for power, as sometimes they are. But they need not always be such in the settings where empirically underdetermined science is a basis for policy. In other settings, such as international relations, family counseling, and labor-management negotiations, we like to think that reasonable people can discuss their differences in respectful, fair, and open ways, and that, while they might not always resolve those differences, they can often find a workable compromise or a modus vivendi. Elaborate procedures have been established for facilitating and constraining such conversations. Conflict resolution has received a lot of theoretical attention, much of it from philosophers who work on topics like game theory. There is an interesting, partly philosophical literature, on modalities of public participation in policy making, ranging from citizen science courts to government science advisory bodies.[26] Is not all of this relevant, as well, in the context of transient underdetermination?

Questions about underdetermination are important because they highlight the arena in which motives and values play a role in theory choice, hence, the arena (shall we acquiesce in calling it the context of justification?) in which those in thrall to pseudo-rationalism worry most (if often wrongly) about the threat to reason from the intrusion of non-empirical factors in science. But there are other arenas in which philosophers of science should play a more prominent role in theorizing the place of science in society. One of those arenas is the setting of research agendas. The naive faith that all important scientific truth will eventually emerge from a free market in scientific research is just that, naive. Resources are limited; decisions have to be made. Shall we build the superconducting super-collider (SSC) or shall we decode the human genome? Here (in the context of discovery?) the role of motives and values is indisputable. But one wants to think that reason has its place in the way we make these decisions. What role for the philosopher of science?

Consider the debate about the SSC. One central issue was the more or less purely philosophical question of reduction and emergence. The SSC was championed by the particle physics community as the instrument that would make possible the definitive experimental test of the "standard model" in particle physics. Opponents of the SSC within the solid state community argued that, because it was inherently "reductionistic," the particle physics paradigm was not the right route to progress in fundamental physics. Better to follow the route charted by solid state physics, which studies emergent structures, and is less capital intensive. Instead of spending megabucks on one, big, particle physics experiment, spend the same money on lots of research in solid state and get a much bigger

[25] Rudner's argument was, just as famously, attacked in Levi (1960).

[26] Peterson (1984) is a classic source. For more recent overviews, see Chopyak and Levesque (2002) and Douglas (2005). For examples of philosophers of science tentatively exploring connections between game theory and norms, see Bicchieri et al. (1997).

bang for the buck.[27] Philosophers of science had for a long time being arguing about reductionism, but philosophers of science were silent when these issues were debated in Congress and elsewhere, even though they were better situated than others to make the point that the lack of conceptual sophistication in the physicists' argument about reduction and emergence was a clue to the fact that that argument was just a smokescreen veiling a clash over money. Some now think that the death of the SSC set back progress in fundamental physics by decades. It's not clear that that's true, but it is clear that the leading role in experimental particle physics has been ceded to European researchers at CERN. It is ironic that similar questions about reduction and emergence arose again in connection with the human genome project and other developments in molecular genetics. This time philosophers of science played a more prominent role, but it must be admitted that those philosophical interventions have had less of an effect in steering the debate than one might wish.[28]

Questions about research agendas appear to fall entirely within the context of discovery. But that is not always the case. Questions of theory choice are, typically, questions about choice among a set of competing theories, if only between a hypothesis and its corresponding null hypothesis. Even if evidence alone suffices to pick a winner, that means only that the chosen theory is better than its rivals. Were there different rivals, the evidence might point to a different winner. It follows that the motives and values driving the selection of research agendas can also, in this way, affect judgments of warrant.[29] For example, it took a surprisingly long time for the U.S. Food and Drug Administration (FDA) and the National Institutes of Health (NIH) to realize that findings of a drug's safety and effectiveness might not be trustworthy if differences in response based on gender, race, and age are not routinely studied.[30] And some think that lack of progress in environmentally benign solar and wind electrical power generation is less a result of an objective scientific judgment of comparative cost and efficiency and more a result of our having chosen for social and economic reasons not to invest in the research and development necessary to increase the efficiency and bring down the cost of genuinely "green" energy sources.[31] Does anyone think that we make a narrowly scientific decision when we choose, instead, to spend heavily on research on biofuels that do nothing to help with global warming but do a lot to enrich big agribusiness?

A theory of science adequate to the task of understanding the work that motives and values do in science requires our acknowledging the philosophical importance of transient underdetermination of theories by evidence and of the manner in which questions of theory choice are deeply interwoven with questions about the setting of research agendas. Pretending that motives and values play no role in science or that their effects are negligible in the infinite long run of inquiry are merely ways of ignoring genuine problems about science in context. Equally unhelpful is the view that the philosopher's task is limited to articulating idealized theories of science whose only relationship to real science is to serve as a yardstick by which to measure our failure to achieve the ideal. We need a

[27] Some aspects of the reductionism and emergence controversy and the debate between solid state and particle physicists are discussed in Howard (2007).

[28] Kitcher (1996) is an impressive, and rare, example of a philosopher of science seriously engaging the revolution in applied molecular genetics.

[29] Okruhlik (1994) makes this point very persuasively.

[30] Only in 1994 was it mandated by NIH that women and minorities be represented in clinical trials. For overviews, see Caldwell et al. (2004), McMurdo et al. (2005), and Merkatz (1998).

[31] Herzog et al. (2001) provides a helpful review of the funding for research on renewable energy sources.

theory of science that applies to science as it lives in a contingent, complicated, and contested real human world, a world where bias, greed, prejudice, and political agendas affect all human practice, including the practice of science.[32] But still more is required.

3 Theorizing the Social Nature of Science

A major impediment to substantive philosophical engagement with questions about motives and values in science is our continuing attachment to an epistemology that assumes, usually without argument, that the locus of scientific knowledge is the mind of the individual scientist. It is a trivial truth that scientists are individuals, and it is also a trivial truth that, in some sense, the individual scientist knows something about the world. From these two trivial truths it does not follow, however, that the most important epistemological questions about science concern relationships between individual mind and world.

Notice how the individualist bias in epistemology affects one's thinking about the place of values and motives in science. If what matters is only or mainly individual scientists, individual epistemic agents or subjects, then the only values and motives that could be relevant are those of individuals. Straightaway, we find ourselves thinking that the only possible questions are those about the "subjective" values and motives of the individual, and already just our philosophical vocabulary has us thinking that merely "subjective" values and motives have nothing to do with "objective" scientific knowledge. End of argument. Yes, individual scientists sometimes wrongly let themselves be influenced by such subjective motives and values, but that's not a problem for science, or the philosophy of science, because it's just a matter of the moral or psychological failings of those individuals. Blame the weak-willed or naughty scientist, not science itself.

What's wrong with the individualist epistemology of science should be easy to see. That there is an important social dimension to scientific knowledge might already be suggested by the fact that scientists (and philosophers) have invested so much effort in the design of systems of peer review and the independent replication of experimental results. Do we accept Boyle's law if we cannot get Dutch and French vacuum pumps to behave like the pumps in Boyle's own laboratory?[33] No. Do we accept the inheritance of acquired characteristics if researchers other than Kammerer cannot produce heritable nuptial pads in midwife toads?[34] No. We all agree that, in principle, one scientist's producing a result does not suffice to warrant the community's acceptance of the result. But the defender of individualism in epistemology might reply that peer review and the requirement of reproducibility are for the purpose of eliminating or minimizing precisely the individual bias or error that is acknowledged on the individualist model.

That more is involved than just the elimination of individual bias is implied, however, by the utterly commonplace use of expressions like "community acceptance." For *me* to know that I'm wearing my hat, it usually suffices that I, alone, just looked in the mirror (though a contrary report from just one trustworthy friend can be enough to occasion doubt). For *us* to know (and that's our default way of speaking) that the earth is round, that benzene has a ring structure, or that space-time is curved requires the collaborative efforts

[32] One takes heart from recent growth of interest in value questions among philosophers of science. See, for example, Machamer and Wolters (2004), Kincaid et al. (2007), and Carrier et al. (2007).

[33] Early failures to replicate Boyle's results are an important issue in Shapin and Schaffer (1985).

[34] A helpful, sober appraisal of this famously controversial episode can be found in Gliboff (2006).

of, in some cases, hundreds of individuals acting within structured scientific communities. In a derivative sense, I might be said to know, myself, that benzene has a ring structure, but a well-nigh necessary condition for my *knowing* that—in the only sense relevant in science—is that what I "know" has been accepted by a whole community of researchers after multiple experimental tests and critical debate in the journals.[35] That I know that space-time is curved is, by itself, actually quite irrelevant to this claim's having the status of scientific knowledge. What is important is, instead, its relationship to the relevant community of inquiry.

That scientific knowledge lives in communities of inquirers, not in the heads of individual scientists, is clearer still if one asks about any reasonably comprehensive achievement of modern science, "In whose head does it live?" No one individual knows everything that there is to know about current relativistic cosmology, or superconductivity, or estuarine ecology, or polymer chemistry, or the cultural anthropology of the Pacific Northwest, yet all of these belong to the store of current scientific knowledge. What about experiment? No one individual is master of all of the many different bodies of expertise that go into designing, building, running, and interpreting the results from the soon-to-be completed Large Hadron Collider. The typical paper in experimental particle physics carries scores of names and would carry many more if everyone responsible for all of the technical infrastructure were credited.[36]

Viewing scientific knowledge as the achievement of communities, not individuals, changes some of the epistemological questions we ask, but not others. One question that simply disappears concerns the putative, subjective, phenomenal basis of scientific knowledge, too long a weak point in traditional empiricism. The shift of perspective from the individual to the community should suffice to make irrelevant all questions about theory's grounding in individual sense experience.[37] Whether you and I see the same shade of pink when we look at the litmus paper is of no more than minor, practical, significance, likewise the observational astronomer's "personal equation." We have to be alert to individual perceptual differences, but the only philosophically relevant issue in this vicinity concerns the way in which the community (there it is, again) controls for individual difference. Perfectly legitimate questions remain about theory's relation to world, theory now regarded as abstract propositional or semantic structure, or theory's relation to what the community accepts as the relevant observation sentences, conceived not as reports of individual, subjective experience but as statements of experimental findings.

Some new epistemological questions emerge from within a social perspective on scientific knowledge, foremost among them being questions about the epistemic significance of social and institutional structures. That there are epistemological implications of the

[35] That we so easily advert to the view that "knowing" is the relevant epistemic modality in science, as opposed to something like "provisional acceptance" or "taking as pursuitworthy," might be yet another legacy of the individualist bias in epistemology. After all, in the strictest sense, scientists do not *know* that space-time is curved, while there is general agreement within the relevant communities that this view, a consequence of general relativity, is the best one available at present. For more on the variety of epistemic modalities at play in science, see McKaughan (2007).

[36] Hardwig (1985) emphasizes the point about the large numbers of people involved in particle physics experiments. See Baird (2004) for an interesting discussion of the way in which, starting in the mid-20th century, the knowledge involved in the design, construction, and operation of scientific instrumentation tends to be black-boxed.

[37] No surprise, therefore, that Neurath (1932) contested the phenomenalism advocated by the Vienna Circle's right wing in the context of the protocol-sentence debate during the early 1930s. For an analysis of the debate, see Zhai (1990).

social and institutional structures within which scientific knowledge is generated and tested should occasion no surprise. After all, everyone agrees that scientific claims that have been independently duplicated and vetted by peer review are, for those reasons, more reliable. Questions about the reliability of scientific claims are epistemological questions. Moreover, if, from an individualist epistemological point of view, it makes a difference how the mind works, then, from a social epistemological point of view, it should make a difference how scientific communities work. But we all know that assertions of the epistemological significance of social structure will be resisted. Whence the prejudice that the behavior of ideas in the mind is part of the epistemologist's brief whereas the behavior of scientists in the laboratory is not?

One source of the anti-social prejudice in epistemology is surely the Cartesian illusion of immediate, first-person access to mental content, an illusion that engenders the further illusion that when we reflect upon the introspected contents of our own minds we are doing something different in kind from bad psychology. His frustration with this enduring philosophers' fantasy was one motivation for Wilhelm Wundt's leading the experimental revolution in psychology at the end of the 19th century, a revolution that drew lots of bright young researchers out of stale philosophy departments and into laboratories.[38] If the subject is social structure, not mental structure, there can be no illusion about privileged, first-person access and no illusion about the prospect of such an epistemology's being an a priori science. An intellectually responsible social epistemology will have to be an empirical science, hence a form of what is termed "naturalistic" epistemology.

This is not the place to rehash old arguments about epistemological naturalism.[39] Suffice it to note that when Neurath turned his attention to the non-empirical factors in theory choice he chose to speak in the psychological language of "motives" and urged the study of auxiliary motives as part of an empirical science of science. Recall also Quine's sensible reply to critics of naturalism who held that it is viciously circular to pretend to justify empirical science by doing empirical science. Quine said that if one abjures the goal of justification, then vice becomes virtue, the circularity becoming commendable theoretical closure (Quine 1969). Not often enough noted is that forswearing the goal of justification does not mean giving up on the self-critical reform of scientific practice based on empirical studies of scientific practice. If kinesiology can improve my backstroke, epistemology can improve our scientific practice.

What would a critical, social epistemology of science involve? It would involve empirical studies of community structures and practices in science. It would involve the formulation and testing of models for improved scientific practice. For example, whether blind refereeing produces better science than does refereeing in which author and referee are known to one another is an empirical question, assuming broad agreement on what "better" means in science. It is, likewise, an empirical question whether government funding or corporate funding of pharmacological research produces a wider variety of safer, more effective, and less expensive drugs. Is a hierarchical or an egalitarian laboratory organization more effective? Is publication through for-profit journals better for science than "publication" though free, pre-print servers? Those are empirical questions. And it is an empirical question whether allowing citizen challenges to government funded scientific research produces sound science.

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38 Boring (1929) is the classic history of the development of experimental psychology; see also Rieber et al. (1980). For recent philosophical and sociological perspectives, see Hatfield (1990) and Kusch (1995).

39 Two helpful sources are Shimony and Nails (1987) and Kornblith (1994).

A critical, social epistemology of science might also involve analyses of major failures in science, after the model of failure analysis in engineering. What went wrong in the Vioxx case? Bad science was done in this case. We want to know why? Was it mainly errors in data analysis? Was it mainly errors in the design of clinical studies? Was it mainly the result of individual scientists' and their bosses' not behaving as professional ethics demands? Or was it mainly the result of weaknesses in structures of authority within the Merck corporation or structures of oversight in the FDA?[40] One suspects that the philosophical literature on confirmation theory will be of little help in answering most of these questions.

Of course the idea of social epistemology or a critical, social epistemology of science is not new. Such projects were launched already in the 1920s and 1930s in the work of Karl Mannheim (1929), Ludwig Fleck (1935), and Edgar Zilsel (1926; also de Santillana and Zilsel 1941). One should include the sociology of science of Robert K. Merton (1938b) in this early history, his introduction of the idea of the social norms of science (1938a, 1942; see also Douglas 2002) being especially pertinent. The development of strong program sociology of scientific knowledge in the 1970s bespeaks a similar impulse, even if it has been, in some of its manifestations, less respectful than its predecessors of science's own, unreflected pretensions to objectivity and readier to see not just the social structures of science but also the content of scientific theories as being affected by social context (see, for example, Bloor 1976; Woolgar 1988). More recently, still, some within the analytic epistemology community have begun to explore the social dimensions of knowledge (see, for example, Schmitt 1994; Goldman 1999), and there is a growing interest among philosophers of science, as well, in social epistemology (see, for example, Longino 1990, 2002; Solomon 2001; Kusch 2002). There has even been, for more than 20 years, a journal called *Social Epistemology*.

One welcomes the growth of interest in the social dimensions of scientific knowledge and the breadth of perspectives that have emerged, even as one notes, with regret, that these essays in the direction of the social are still somewhat peripheral to mainstream work in epistemology and philosophy of science. One notes as well that one has yet to see in these new literatures the kind of deep merging of empirical sociological and philosophical perspectives on the institutional-epistemic structures of science that will be needed in a critical, social epistemology of science that can aspire to being an effective voice in the actual reform of scientific practice and social practice affecting science. Such was, perhaps, the ambition of the founding editor of the journal, *Social Epistemology* (see Fuller 1988),[41] and such is the ambition of some new work (see Biddle 2006). But the field has yet to mature.

The above given examples of questions to be addressed by a critical, social epistemology of science do make one point very clearly, and that is the way in which the social perspective more easily accommodates our interest in motives and values in science than does individualist epistemology. It was Plato, in *The Republic*, who taught us that justice was more easily studied in the city-state than in the soul. In a similar way, it is in the

[40] See Biddle (2007) for a compelling argument that the important failures were institutional.

[41] Fuller's unfortunate intervention on behalf of the defendants, hence on behalf of requiring the teaching of intelligent design in public schools, in the Katzmiller v. Dover case of 2005 (Tammy Kitzmiller, et al. v. Dover Area School District, et al. 400 F. Supp. 2d 707 (M.D. Pa. 2005)) demonstrates only too well how, from the point of view of serious defenders of the integrity of science, social critique can devolve into what one dogged defender dubs the "New Cynicism" (Haack 2007, 21). Fuller's statement in the Kitzmiller v. Dover case, "Rebuttal of Dover Expert Reports," a copy of which can be found at http://www2.ncse-web.org/kvd/experts/fuller.pdf, makes instructive reading.

functioning of scientific communities and their relationships to the larger social whole that we see more vividly the work of values and motives that promote or hinder good science.

4 Why Do We Hesitate?

Important first steps toward crafting a critical, social epistemology of science were being taken before and during the Second World War in the work of people like Fleck, Zilsel, and Merton. When the old socialist Philipp Frank tried to reconstitute the institutions of the Vienna Circle in North America after the Second World War, he insisted that such critical, social perspectives on science be a central focus of the new Institute for the Unity of Science. Frank also agreed with Neurath that, because theory choice is empirically underdetermined, social and political considerations can and do play an important role in science (see Frank 1953, 1957). But as Reisch makes clear, by the early 1950s the climate was not right for a philosophy of science that seriously engaged questions of value in science and asserted the philosophical relevance of empirical studies of the social structures of science. Such a conception of the philosophy of science was being forced into retreat by the very different vision of the field promoted in books like Reichenbach's *The Rise of Scientific Philosophy* (1951), a vision that emphasized formal analysis and consigned empirical questions about the history, sociology, and psychology of science to the allegedly philosophically irrelevant context of discovery. And the political mood of the United States in the 1950s was not hospitable to philosophical projects with the pronounced left-liberal valence of Frank's program. A socially disengaged, purely formal, "professional" philosophy of science—the classic neo-positivism that we associate with Reichenbach, Carnap, Nagel, and their students—was more likely to thrive in the America of Joe McCarthy and "the end of ideology" (Bell 1960).

It's more than a little puzzling that in the dramatically different political climate of the United States after the Vietnam War, and in the dramatically different intellectual context that emerged after assaults on neo-positivist orthodoxy by Norwood Russell Hanson (1958), Stephen Toulmin (1953, 1961), Kuhn (1962), Paul Feyerabend (1975, 1978), and others, the idea of reviving a critical, social epistemology of science has yet found so little following. One would prefer not to think that it's because social perspectives on science reappeared so prominently in the mid-1970s and early 1980s in the work of feminist philosophers of science, though it is no secret that the mainstream philosophy of science community was slow to extend a welcome to feminist science theory. One would also prefer not to think that socially engaged philosophy of science is avoided today because of fear of political persecution and job loss, a threat that was all too real and effective in stifling dissent and heterodoxy in the 1950s.[42] The academy of the 1980s and beyond is a riot of heterodoxies and radicalisms. And in areas of the academy outside of philosophy of science, social perspectives on science—too often science unfriendly social perspectives—have been well received, as by historians, sociologists, and students of continental critical theory in philosophy.

[42] How to interpret the longer-term implications of Ward Churchill's recent firing by the University of Colorado is not clear. One is distressed, however, by the alacrity with which charges of plagiarism and other academic misconduct, the subject of old rumors, were investigated and made the legal basis for his dismissal only after his ill-considered remarks about the 9/11 attacks. For a recent, somewhat comprehensive report, see the story about the firing in the July 24, 2007 issue of the *Rocky Mountain News*: http://www.rockymountainnews.com/drmn/local/article/0,1299,DRMN_15_5642650,00.html

More likely explanations for the slow embrace of social perspectives on science must be sought elsewhere. One is suggested by the just mentioned popularity of social perspectives on science in departments of history and sociology, for social views of science in those academic settings have so often taken what is perceived—often most unfairly—as a science unfriendly form. The controversial Sokal hoax (Sokal 1996; Sokal and Bricmont 1998) and Gross and Levitt's cranky and querulous *Higher Superstition: The Academic Left and Its Quarrels with Science* (1994) are two more extreme expressions of the perceived threat to science that many have thought to lurk in social approaches to science theory. But it's surely an overreaction and a bad bit of philosophizing to argue that the only way to defend the integrity of science is to deny a role for values and motives in science anywhere outside of the context of discovery. And if irrational science bashing has been too prominent a theme in socialized science theory outside of the philosophy of science, then philosophy of science as a discipline bears some of the blame, for had an interest in the social dimensions of science been allowed to flourish within mainstream philosophy of science in the postwar period, if interests in the social dimensions of science could have been pursued in the tough-minded, critical environment of mainstream philosophy of science, then the more extreme kinds of nuttiness found in some corners of late-20th century science studies might not have survived.

A clue to another explanation for the philosopher's slow embrace of a critical, social epistemology of science and continuing suspicion about assertions of a role for motives and values in science is to be found in the curious inertia seemingly still evinced by some core dogma from right-wing logical empiricism and post-war neo-positivism. In the years after publication of Kuhn's *The Structure of Scientific Revolutions*, no plank in the platform of neo-positivism went unchallenged. Demarcation, confirmation, explanation, reduction, unification, meaning, reference, and truth, itself, all took a beating. Anti-foundationalisms of every variety had their day. And yet, more than one veteran of the science wars when asked, today, what we learned through all of that will describe a scene—unrecognizable to some of us—in which the fortress walls are battered and scorched but the fortress, itself, still stands. The point is not that cozy notions of truth and objectivity have been demolished. Far from it; the argument is ongoing. The point is that too many of the front-line reports misrepresent the casualty counts. For example, there are more than a few serious, prominent philosophers of science who adduce good reasons, of different kinds, for thinking that one should be wary of uninflected talk of the "truth" of scientific theories. One need not go all the way to the radical social constructivist fringe to find skepticism about "truth" talk. One finds it right in the philosophy of science mainstream, as in the work of Bas van Fraassen (1980) or Quine (1960). Yet one too often gets the impression that such skeptics have been driven from the field.

One example of this tendency in the recent literature is the "simple realism" defended by Kitcher in *Science, Truth, and Democracy* (2001).[43] The essays collected by Noretta Koertge in *A House Built on Sand: Exposing Postmodernist Myths about Science* (1998)

[43] See also Kitcher (1993). The quibbler will say that realism is not a neo-positivist dogma. In fact, the relationship of realism to neo-positivism is more complicated than folklore suggests. Here, just three points out of a much-needed longer analysis: (1) Reichenbach, the teacher of Hilary Putnam (who made famous the "no-miracles" argument for realism), was, himself, a realist of sorts. (2) Thoughtful readers of Carl Hempel's "The Theoretician's Dilemma" (1958) all realized that the in-principle eliminabiltiy of theoretical terms promised by the Craig elimination theorem wouldn't pose a dilemma for the anti-realist. (3) The call for the elimination of metaphysics from science does not entail anti-realism, and the dispensibility of theoretical terms, especially via individual, eliminative definitions, can be viewed as proof of possession of cognitive meaning, not lack of it.

afford still more examples.[44] Perhaps the most telling examples, however, arise in forensic settings, especially when philosophers of science testify as expert witnesses in trials concerning creationism and intelligent design. Philosophers of science defending Darwin often make their case on the basis of criteria of demarcation between science and non-science. The appeal of such a courtroom strategy is obvious. Show that creationism or intelligent design is not science and the case is closed. Judges like such clean criteria.[45] So it's no surprise that the strategy has worked.

But the problem with the demarcationist strategy was shown already more than 20 years ago, when, after the Arkansas case,[46] Larry Laudan (1982) argued that each of the criteria of demarcation adduced by Michael Ruse (1982) in his expert testimony is violated by some well-accepted scientific theory and that many of the same criteria are satisfied by "creation science." Those familiar with the debates about demarcation in the philosophy of science literature were not surprised by the ease with which Laudan disassembled Ruse's reasoning. We knew that the project of articulating necessary and sufficient conditions for distinguishing science from non-science had encountered serious obstacles.[47] Why, then, is a doctrine whose vulnerabilities are well known among philosophers of science presented to the public as representing settled wisdom among philosophers of science? Is it a case of the Platonic noble lie, the philosophers' esoteric wisdom not being allowed to be shared with the hoi polloi? Is it that we think judges, juries, and the public incapable of understanding subtleties and shades of gray? Do we take more license when the barbarians are at the gate?

We confront here an important issue. If philosophers of science are to play a role in public debate about science—in the courtroom, in Congressional hearings, on advisory panels, as corporate consultants—then we have to find a way to represent what we have learned about science fully, honestly, with all appropriate detail and complexity. Respect for the public requires this, as does respect for the accomplishments of our discipline. We all wish we could say to the champions of intelligent design, here are three clear conditions that science must satisfy, intelligent design satisfies none of them, therefore intelligent design is not science. We all wish we could say to apologists for a hydrocarbon fuels economy that the case for global warming's being caused by human burning of fossil fuels has been conclusively proven. We all wish we could say that empirical science is a straight and easy road to the truth about nature. But wishing won't make it so.

5 Conclusion: Better Red than Dead

For contingent, historical reasons, the socially engaged theories of science promoted decades ago by Neurath, Dewey, and Frank came bundled with a leftist political agenda. That "scientific" Marxists, socialists of other kinds, and liberals who look to science as an

[44] But see also the interesting later collection, Koertge (2005), where the temperature is a bit lower.

[45] See the opinion of Judge John E. Jones III in the Kitzmiller v. Dover case, a copy of which can be found here: http://www2.ncseweb.org/kvd/all_legal/2005-12-20_Kitzmiller_decision.pdf

[46] McLean v. Arkansas Board of Education, 529 F. Supp. 1255, 1258–1264 (ED Ark. 1982).

[47] This difficulty and the associated difficulty of specifying necessary and sufficient conditions for cognitive meaningfulness were old news by the 1980s; see, for example, Hempel (1950). Robert Pennock's expert statement in the Kitzmiller v. Dover case, while arguing that intelligent design is not science, handles the demarcation question with more subtlety and nuance than one found in Ruse's testimony in the Arkansas case. A copy of Pennock's statement can be found here: http://www2.ncseweb.org/kvd/experts/pennock.pdf

engine of progressive social change need a philosophy of science is obvious. The right might be less likely to think it needs or to proffer unblinkered theories of agenda driven science, if, as Neurath, Marx, and others have thought, the rationalization of class and other interests is abetted by disguising partisan interest as objective truth in service to universal good. (There is, however, a species of right-wing anti-foundationalism whose point is to undermine or limit the cultural authority of science as a threat to class or other interests, leading to a form of right-wing science bashing.) Socially engaged science theory might, therefore, almost always appear in shades of pink or red. To those unsettled by such a prospect the best counsel might be the converse of an old slogan, "Better red than dead."

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Author Biography

Don Howard is a Professor in the Department of Philosophy and the Program in History and Philosophy of Science at the University of Notre Dame. He holds a B.Sc. in physical sciences from Michigan State University and both an M.A. and a Ph.D. in philosophy from Boston University. His special interests include the history and philosophical foundations of physics and the history of the philosophy of science. Recent publications include: *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, co-edited with Martin Carrier and Janet Kourany (University of Pittsburgh Press, forthcoming); "'Let me briefly indicate why I do not find this standpoint natural.' Einstein, General Relativity, and the Contingent A Priori," in *Synthesis and the Growth of Knowledge: Examining Michael Friedman's Approach to the History of Philosophy and Science*, Michael Dickson and Mary Domski, eds. (Open Court, forthcoming); "Einstein and the Philosophy of Science," in the *Cambridge Companion to Einstein*, Michel Janssen and Christoph Lehner, eds. (Cambridge University Press, forthcoming); and "Albert Einstein as a Philosopher of Science," *Physics Today* (2005).