

Kitcher's Two Cultures

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A conception of natural science as primarily politics by other means animates the so-called “science wars.” Ideologists dispute over competing conceptions of scientific rationality or objectivity. On the one hand, debunkers maintain that the hard sciences endorse beliefs that only reflect—sometimes consciously, sometimes not—efforts to legitimate nonscientific interests (typically, ones connected to gaining or maintaining political or social advantage).¹ On the other hand, the “scientific faithful” portray science as providing an undistorted window on the natural world, a perspective almost magically disconnected from social influence. One must laud Philip Kitcher’s ambition in *Science, Truth, and Democracy* to find a via media between these extremes.

Kitcher’s analysis evolves through two broad divisions: “Part I: The Search for Truth,” and “Part II: The Claims of Democracy.” The two divisions fit in the following way. Part I defends scientific inquiry as a search for truth, while acknowledging that what truth is is no simple matter. In particular, the questions for which scientists seek (true) answers themselves reflect interests particular to the place and time of those asking the questions. In this regard, reflections in Part I lead to the conclusion that although science connects with a world not of human making, the pieces with which scientists attempt to connect strongly reflect social and cultural interests. This prepares readers for the discussion in Part II, which asks what constraints on research should be imposed. Not all knowledge is worth having. The constraints that emerge involve those which ideal deliberators would

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choose in seeking to navigate between a conception of science as an unfettered search for truth and a science that respects the interests and concerns of a democratic society (p. 200). Kitcher, in short, posits a culture dedicated to the unfettered search for truth as in conflict with and a threat to those supporting democratic values.

Yet some concepts resist stable theoretical elaborations. Examples here include science, truth, and democracy.² But public policy debates continue to invoke these seemingly weighty notions. And just here Kitcher promises some tangible payoff. "I present an ideal of 'well-ordered science,' intended to capture what inquiry is to aim at if it is to serve the collective good" (p. xii). However, nothing Kitcher says in either section ultimately make plausible or compelling the view that there exists a method for determining which truths to discourage scientists seeking. Kitcher unwittingly offers only a pseudo-palliative—the "tyranny of democracy"—to compensate for failures of contemporary science policy. What makes "well-ordered science" well-ordered or a science remains unknown and unknowable. Kitcher's suggestions on how to bridge the gap remains more a symptom of a cultural problem, not its cure.

A PHANTOM MENACE?

"What is truth?" said jesting Pilate, and would not stay for an answer. Pilate was in advance of his time. (Austin, 1970, p. 117)

Like moths to a flame, philosophers find themselves irresistibly drawn to the concept of truth, unable to emulate Pilate's higher wisdom. The fatal attraction the concept exerts rests on a very strong and (surely) widely shared intuition: "We believe that we cannot simply make up the world to our own specifications" (p. 11). From this intuition some conclude that truth names those representations that represent Reality.³

What signals correct representation? Kitcher follows Putnam in promulgating the view that "success is naturally taken to betoken truth" (p. 17). In other words, the "practice of inferring truth from success should be understood as one in which we support belief in the working posits of our theories" (p. 18). Kitcher's "fallibilist realism" (p. 19) takes it that the entities—sensed or unsensed—named in one of our best confirmed theories genuinely refer, while acknowledging that theories are subject to correction over time.

Consider Kitcher's example of navigating one's way about London using a subway map (p. 24). Because the map maps areas and items independent of human cognition, it enables a person to successfully make their way about. Those with inaccurate maps do not as a rule fare as well. The map's predictive success would be inexplicable if attributed to, for example, happenstance. Precision further validates confidence in the truth of representations (p. 28). Interests may help determine what wants representing, but interests do not dictate whether or how these representations fit that world. "Whether or not we value predictions and interventions of a particular type, we can recognize that a theory generates those predictions and interventions" (p. 30). Opinion does not determine whether a person arrives at her desired destination. "If my arguments are correct . . . the sciences sometimes deliver the truth about a world independent of human cognition, and they inform us about constituents of that world that are remote from human observation" (p. 28). Success signals fit between representation and world.⁴

But sociological critics of science would claim that extraneous interests determine as well what counts as accuracy or success.⁵ That is, sociologists of science typically differ from philosophers of science not on the issue of the success of representations, but the conditions under which a theory achieves its putative success. The claim that accuracy or success links to truth requires then that the measures of accuracy or success themselves have some independent plausibility.⁶

Kitcher attempts to create just this link by developing what he terms a "Galilean strategy." The core of this strategy resides in blurring distinctions which might function as "inferential obstacles" (my term) that is, challenges which seemingly block drawing a desired conclusion. According to Kitcher, Galileo's opponents raised such an inferential obstacle based on a celestial-terrestrial distinction, that is, between what one observes using a telescope on earth as opposed to what one observes celestially. Galileo, on Kitcher's telling, overcomes inferential skepticism regarding telescopic observations of matters celestial by shifting the burden of proof. If all grant that the telescope enhances naked eye observability in the terrestrial realm, what reason exists for saying it fails to offer the same enhancement for eyeing the celestial realm? (See especially pp. 20-21.) Known and accepted applications can be used to justify seemingly novel applications by arguing, in effect, that the extension to an additional realm involves no genuine novelty.

Realists extend this strategy to argue for the reality of unobservables by claiming that “observability-under-current-limits” matters not; successful prediction does. “More exactly, we come to believe that people usually only manage to achieve systematic success in prediction when their views about the underlying entities are roughly right” (p. 22). Kitcher construes success as an observer-independent notion. For example, the truth of the assertion that someone arrives at a particular station does not require the presence of any observer (p. 28). But then given predictive success, Kitcher notes, why continue to discriminate against unobservables (p. 23)?⁷

The map metaphor also provides an intuitive connection with what to count as objective accuracy of representation. But the metaphor does not work well for purposes of tying accuracy to truth. For accuracy relates to interests in a way in which, presumably, truth does not. A subway map may accurately represent the number of stops between any two stations but not be scaled to distances between stations.⁸ Do such maps represent truly? Only, it seems, if one adds—*sotto voce*—that it represents truth for particular purposes. But truth so construed lacks the observer-independent quality Kitcher attributes to it.

Interestingly enough, Kitcher appreciates this complication and even adds to it. His complication cleverly builds on Arthur Danto's famous thought experiment of an *Ideal Chronicle*.⁹ An *Ideal Chronicle* is a record of everything that happens, recorded as it happens. Danto argues convincingly that even such an *Ideal Chronicle* will fail to capture all historically significant truths. For example, the *Chronicle* could record the bombing of Pearl Harbor on 7 December 1941. Yet its record for that date on that date, no matter how detailed or complete, would not contain the true statement that on this day the event occurs that draws the United States into World War II. Only subsequent events, of course, make this statement a correct description of what happens earlier. Happenings after the fact invariably alter the set of descriptively correct statements that apply to prior events. The past proves plastic, a kaleidoscopic collection of details whose patterning depends upon one's temporal focal point.

Kitcher ingeniously expands Danto's thought experiment to show that it challenges an analogous notion of ideal or objective understanding. Specifically, he objects to an *Ideal Science* “based on a single unified framework of laws,” one where the core idea is “that science supplies a structure that is a resource for ‘objective understanding,’

whatever our contingent interests" (pp. 71-72). Just as the *Ideal Chronicle* cannot possibly capture all truths because later happenings add new true descriptions of the past, likewise the hope of an ideal explanation (or ideal atlas) comes apart under pressure of shifts in interests. Human interests determine what calls for explanation. Moreover, interests shape as well the factors needed to explain.¹⁰ No atlas can be ideal from an atemporal standpoint because none can anticipate what future generations may wish maps to map.

Future happenings also undo hopes for an ideal causal explanatory account for at least two basic reasons. First, lacking any hierarchical reduction of all explanations to ones in a most basic science, differing schemes of explanation cannot necessarily be reconciled. In short, there exists no objectively determined ideal explanation. Second, not just any correct causal story explains.¹¹ The world does not dictate under which description of events explanation will be called for. Hence, an unbounded proliferation of explanatory schemes becomes possible as future events reshape the terms describing the past.

But the truth-connected doctrine of scientific inquiry Kitcher peddles in his opening pages sits uneasily on the shifting sands of explanation he sketches above. For the ironic payoff of his argument paralleling Danto's *Ideal Chronicle* turns out to be that we have no reason to suppose that theories or even chronicles ultimately converge, that is, produce some one final account of what there is. Later happenings provide new perspectives on the past, but nothing requires that perspectives prove consistent or stable.¹²

Kitcher may appear to the unwary to offer a defense of realism, but his defense proves to be vanishingly thin. Take a seemingly extreme statement of the sort of view which, I take it, Kitcher intends to oppose. "In principle, the decisions which produce the world are free and unconstrained, They could be made at random, each scientist choosing by the toss of a coin at each decision point what stance to adopt."¹³ But Kitcher actually offers nothing that directly opposes this view.

For example, Kitcher readily concedes that experimental evidence may be accommodated within competing theoretical frameworks. "Debate goes on, because there are many different ways of adjusting the background constraints, given the broader goal of achieving a systematic account of the composition of the pertinent substances" (p. 40) Indeed, he acknowledges, "the fact that we find it hard to generate a precise account of scientific evidence that will apply to such

complicated debates should no longer be surprising" (p. 40). He grants that no one decision necessarily decides the game.

But he attempts to dull the force of this concession by using the case of Lavoisier's triumph over phlogiston theory. For such cases establish, he maintains, that as "unsolved problems mount for one side, and as previously recognized difficulties for the other turn into successes, the scales begin to tip, and we appreciate how the evidence favors Lavoisier" (pp. 40-41). But this will hardly do for purposes of tying success or accuracy to truth in a way that opposes Pickering. For the example only establishes that Lavoisier had key decision points go his way. But how were those cases in turn decided?

If, that is, one imagines scientific controversies looking like a chart of some sports league playoffs, certain games—decision points—serve to eliminate some contenders and allow others to advance. But the sociological question concerns the terms under which such games get played. When were key fouls called or plays made? What could have gone differently without violating the terms for playing the game? Outcomes might easily change. Here Kitcher's own concessions make it unclear that the evaluative processes themselves provide anything like a necessary link between success and truth.

Now, in fact, Kitcher himself appears to recognize and effectively concede just this point. For he draws only extraordinarily modest conclusions from his favored example:

I conclude that neither the fact that major scientific controversies are protracted nor our inability to delineate a precise account of scientific evidence should undermine our confidence that the resolution of scientific debate on the basis of evidence is impossible. The ideal of objectivity need not be dismissed as a fond delusion. Hence there is no basis for believing that value judgments inevitably enter into our appraisal of which of a set of rival hypotheses (if any) is approximately correct. (P. 41)

Claims that the "ideal of objectivity need not be dismissed as a fond delusion," that the detracting influences on assessments of accuracy need not be "inevitable," fail to contravene Pickering's "science by coin toss" view.¹⁴

Kitcher never attempts to demarcate what makes a science a science. Rather, his strategy involves attempting to identify general markers of truth, and then arguing that many statements made in the natural sciences manifest these marks. But his arguments fail to

secure any necessary or interesting relation between accuracy and truth.

But even if Kitcher overstates his goal when he claims to tie accuracy to truth, perhaps he could be credited with a still interesting, if conceptually more modest, achievement. For although unable to tie assessments of accuracy to truth, he can link them, it would seem, to objective satisfaction of interests. Indeed, in the closing pages of his "Part I: The Search for Truth," Kitcher almost says as much. Having pooh-poohed the notion of objective understanding or fixed contexts of explanation, he contents himself with the claim that one can have objective intratheoretic markers of scientific significance (p. 78).

But odd things happen to the argument here once Kitcher attempts to elaborate this account of significance by what he terms a "significance graph." These graphs map lines of influence within a field of inquiry (p. 78). Aspects of scientific research inherit their significance from connection to ordinary concerns. In effect, the graphs graph activities within Kuhnian normal science.¹⁵

However, significance graphs also make plain, Kitcher acknowledges, the underlying contingencies of how a field develops. "Alternative choices made earlier would have led to a different development of the field, so that, in quite particular ways, the development of the sciences is thoroughly contingent" (p. 81).¹⁶ Without any hint of the deep irony his remarks betray, Kitcher concludes by declaring his map analogy "vindicated" (p. 82) by his subsequent account of significance graphs. But the map analogy was to tie theories to reality, to make plausible a strong link between a theory's predictive accuracy and its truth. The subway map of his example "is not *approximately* accurate. It is exact" (p. 58; see also p. 61). But 20 pages later, Kitcher announces that like

maps, scientific theories—or, better, significance graphs—reflect the concerns of the age. There is no ideal atlas, no "objective explanations" at which inquiry aims. Further, the challenges of the present, theoretical and practical, and even the world to be mapped and understood, are shaped by decisions made in the past. The trail of history lies over all. (P. 82)

He cannot, however, have it both ways. Appeals linking accuracy to truth suggest a stable target on which inquirers should converge. But historicizing interests permanently destabilizes the object of inquiry and removes reasons for thinking that inquiry can or should converge.

Now Kitcher could insist that no formal inconsistency exists between notions of maps as accurate and that of maps as products of their age. But simply claiming the notions consistent fails to bring his position into any interesting conflict with that maintained by his erstwhile opponents. Moreover, appeals to consistency here hardly constitute a defense of realism. Shockingly, Kitcher remarks that the weak-kneed conclusion cited above represents all that favors “the view of the scientific faithful” (p. 41). The faithful have reason to worry.

Sociological challenges often seek to debunk science as a new theology, as a surrogate source for certainties and truths. But this apparent relativizing of science to more parochial interests constitutes only a phantom menace, a threat more imagined than real.¹⁷ For relativism so construed becomes a matter of viewing scientific reasoning as a study of the “natural rationality” of social groups or cultures.¹⁸ It does for debates about scientific rationality what Kitcher does for debates about the notion of reality, that is, turns it into a quibble over just when and how we use a particular term. Genuinely confronting the claims of the sociologists requires showing that heretofore contingent notions now possess special timeless status. But Kitcher’s own account precludes this conclusion. But why, then, continue to theologize? Fault Kitcher here not so much for failing as for trying.

DEMOCRACY STRIKES BACK

Physicists have known sin; and this is a knowledge which they cannot lose. (Oppenheimer 1955)

Kitcher’s account in Part I ultimately privileges the notion of interests over that of truth. This paves the way for the claims made in “Part II: The Claims of Democracy.” Oppenheimer’s remark reminds us of how in manifold ways during World War II and through the period of the cold war the natural sciences became increasingly dependent on and complicit with government funding and policies.¹⁹ The question becomes how to manage potentially competing interests—particularly between those scientists may find significant and those of concern to people paying the bills—in pursuit of scientifically significant information. Truth remains the goal of Kitcher’s science, but interests shape significance, and significance points to which truths to seek.

Kitcher labels any dissonance between how scientific inquiry proceeds in democratic societies and the basic ideals of such societies as “the thesis that *science is not well-ordered*” (p. 108). His chapter 8 outlines how, on his account, science and society fall out of moral order. Other chapters in this section explore the conditions for having a science consonant with democratic values.

Kitcher nowhere attempts to define what constitutes science or even, for that matter, to indicate what sorts of statements can be true. Rather, he takes precision and prediction to be measures of at least (natural) scientific truth. But different domains of inquiry involve different levels of precision. History and physics may both be perfectly good forms of empirical scientific inquiry, but each has its own requirements for specifying relevant information. Many disciplines with clear normative agendas lay claim to the title of science. For example, there exists an extensive literature on character development in children that, although explicitly evaluative, has found its way into the curricula of many U.S. elementary schools. An underlying assumption appears to be that we not only know what constitutes “good character” but also have procedures for producing it. Consequently, a concern arises regarding Kitcher’s work just because the scope of science, and so candidates for well-ordering, remains essentially unspecifiable.

What argues for restraints on research? Which research ought to be restrained? Regarding the first question, Kitcher offers what he characterizes as a “Millian argument against freedom of inquiry” based chiefly on the premise that “certain types of research would be likely to undermine a more fundamental freedom” (p. 95). Mill, perhaps philosophy’s most eloquent and compelling advocate of freedom of speech and action, predicates his defense of these freedoms on their indispensability to an even more fundamentally important freedom—allowing people to make informed decisions about their own good and to pursue that good in their own way. These freedoms permit individuals to define who and what they are at the most fundamental level. To preempt someone’s right to those make such decisions lessens them as a human being.

But in the same sentence in which Mill articulates this ideal of a human being, he gives voice as well to the fundamental constraint on free speech and action. “The only freedom which deserves the name is that of pursuing our own good in our own way, so long as we do not attempt to deprive others of theirs or impede their efforts to obtain

it."²⁰ But just which of our actions threaten "to produce evil to someone else"?²¹ Here debate rages, and has since the initial publication of *On Liberty*. Although Kitcher does not place the classic face on the debate as he rehearses it, it presents this visage nonetheless. In this regard, one test for Kitcher's proposed constraints on inquiry would be just how well his account clarifies the vexed distinction between self- and other-regarding actions, that is, between those actions that concern only an individual and those which might others might legitimately restrain because of how it impacts them.²²

Indeed, as I read Kitcher's proposed distinction between science that is and is not "well ordered," it reproduces just the question of how to divide self-regarding actions from the rest:

We aren't used to thinking about the ways in which our attempts to achieve knowledge and the track records of their successes and failures, impinge on people's values and interests. I suggest that this is because of a tension between the science that is practiced by democratic societies and the underlying ideals of those societies. I formulate this as the thesis that science *is not well ordered*. (P. 108)

The variant of the classic Millian problem Kitcher reproduces poses scientists, in their research aspect, as individuals engaged in what they (the scientists) consider self-regarding actions. The case for limiting these individual actions rests on their impact on others.

Which brings us to the second question above: Which research ought to be restrained? Kitcher formulates a version of Rawls's famous maximin principle for choice under uncertainty as a test for distinguishing whether a research act falls into the self-regarding or other-regarding category.²³ The principle counsels the choice of that option with the least worst outcome. So, when making a choice, the principle guides one to that option with the best protection possible if things go badly. That is, Rawls's second principle of justice—a principle which he takes the maximin rule to underwrite—is as follows: "Second: social and economic inequalities are to be arranged so that they are both (a) reasonably expected to be to everyone's advantage, and (b) attached to positions and offices open to all."²⁴ Kitcher's phrasing of how his restrictions operate echoes Rawls's. "If we shouldn't engage in ventures that can be expected to decrease the well-being of those who are already worse off than other members of society, we should therefore refrain from engaging in S [that area of science]" (p. 98). Kitcher seeks to protect groups within society

against research results which, even if true, would most likely add to that group's disadvantage within that society. Research acts become other-regarding if they threaten some violation of the maximin rule.

Specifically, research becomes other-regarding when it involves two conditions. One Kitcher terms *political asymmetry*—where a belief is held prejudicially, and that prejudice negatively affects a group in a society—and the other he calls *epistemic asymmetry*—people overvalue evidence for a prejudicial belief or undervalue evidence against it (p. 97). Epistemic asymmetric beliefs include cases where people will tend to interpret indecisive evidence as nonetheless strongly supporting a prejudicial belief. Given these two asymmetries, he concludes that “when a certain constellation of conditions is satisfied [viz., those defining the two asymmetries] . . . the pertinent inquiries ought not to be pursued” (p. 98). Research that reinforces or threatens to reinforce existing prejudices against certain groups, prejudices themselves not actually warranted or supported by evidence and that worsen already disadvantageous positions, ought not to be supported or undertaken.

Truth here is idle as a defense of such research. “Far less controversial than any duty to seek the truth is the duty to care for those whose lives already go less well and to protect them against foreseeable occurrences that would further decrease their well-being” (p. 103). So, in brief, research making the social position of a disadvantage group worse off than it would have been otherwise justifies a cautionary approach to this research.

But notice just how odd this imagined clash of duties is. For on a Millian account, why would one have a duty to seek the truth? The imagined conflict is simply bogus. Absent extensive further argument, for example, one linking truth-seeking as a necessary condition of self-development, no such duty to seek truth exists for a Millian, and so, a fortiori, no conflict involving such a duty exists.

A different formulation better expresses the concerns that Kitcher intends his asymmetries to capture. “We can agree with Mill and his successors that the freedom to deliberate is fundamental, and in consequence, *adopt just the argument I have given on the grounds that it promotes a fair distribution of this fundamental freedom*” (p. 104). This follows from Mill's suggestion that learning how one's views fare under criticism points to a basic value of allowing open public debate. Anything that restricts access to means of voicing or accessing criticism thwarts this fundamental freedom.

Notice that Mill does not hedge the freedom to deliberate with an expectation that people will reach consensus, even over some hypothetical long term. The value of deliberation lies in helping us decide who we are by allowing us to explore what to believe. Moreover, Mill imagines no single answer to questions about how to live one's life. Indeed, his position suggests that no consensus should be expected except, perhaps, about the desirable framework for deliberation.

In short, Mill's model is not a model for policy making. By this I mean that the framework for deliberation which Mill urges is not in the service of expediting or formulating policy decisions but quite the opposite. Mill's concern remains throughout the promulgation of those conditions most conducive to individual development.

Primarily for this reason, Kitcher's efforts to transition from a Millian defense of constraints on inquiry to a conception of deliberation as setting public policy priorities for inquiry leads only to ironies. Mill's concern involves keeping an individual free of the restraints a group might impose; Kitcher wishes to rationalize the imposing of constraints on individuals in the name of a group. Kitcher's concern to mark out the space of other-regarding actions leads him to the very un-Millian notion of an "ideal deliberator."²⁵

The question with which we began—Under what conditions is the science of a society well-ordered?—can now be answered. For perfectly well-ordered science, we require that there be institutions governing the practice of inquiry within the society that invariably lead to investigations that coincide in three respects with the judgments of ideal deliberators, representative of the distribution of viewpoints in the society. First, at the stage of agenda-setting, the assignment of resources to projects is exactly the one that would be chosen through the process of ideal deliberation I have described. Second, in the pursuit of the investigations, the strategies adopted are those that are maximally efficient among the set that accords with the moral constraints the idea deliberators would collectively choose. Third, in the translation of results of inquiry into applications, the policy followed is just the one that would be recommended by ideal deliberators who underwent the process described (pp. 122-23).

But what, then, makes an "ideal deliberator" ideal? Here matters quickly become clouded. Kitcher stakes a kind of noncognitivist view of values; he opts for the view that "individual preferences should form the basis for our understanding of the personal good that inquiry (among other social institutions) is to promote" (p. 116).

Deliberators then use significance graphs to, supposedly, simultaneously reveal the value of what they propose and have revealed to them the impact and value of what others propose (p. 118).

But significance graphs chart the significance only on the assumption that discussants agree on the value of a particular issues. For interest in the original issue gives items on the significance graph significance. This should come as no surprise in the context of Kitcher's position, since prior agreement on importance of issues and associated values stands as a standard condition for even the possibility of rational discussion in a noncognitivist framework.

Consider two recent Commentary columns in the *St. Louis Post-Dispatch* concerning arguments for and against banning cloning and stem cell research.²⁶ The anticloning piece was published under the names of two U.S. Senators, Sam Brownback (R-Kansas) and Mary Landrieu (D-Louisiana)—cosponsors of a bill before Congress banning the cloning of human embryos. William Danforth, a medical doctor, a former university chancellor, and a brother of a prominent former U.S. Senator, wrote the opposing piece.

What divides the positions? Both sides oppose cloning humans. They disagree regarding what counts as doing so. For Brownback and Landrieu, any cellular entity a scientist creates that could have developed into a human being represents an act of wrongful creation, an immoral use and destruction of a human life. For Danforth, "banning the transfer of a cell's DNA-containing nucleus into an egg cell, or ovum, for the purposes of generating human stem cells" makes no sense. The intent here, he maintains, is not to clone but to harvest cells of a certain sort.

For those familiar with aspects of the abortion debate, the argument here is déjà vu all over again. When does human life begin? At conception? At some point in the developmental process? Most important, does anyone actually believe that these issues, if to be settled by discussion at all, will yield to analysis via significance graphs? No one disputes the significance of research. The matter in dispute concerns what entities have rights and what violates them. Notice here that Kitcher's appeal to "tutored collective preference" (p. 135) proves idle as well, since how preferences should be tutored itself involves a normative decision which, once made, determines the moral question.²⁷

Conflicts about research agendas prove often enough to be conflicts over moral priorities, priorities not subject to tutoring or change by experts or inclusiveness. Differences do not represent lack of infor-

mation, but contrasting norms that people use to evaluate information. Kitcher betrays, I suggest, a fatal naiveté when he suggests that he expects "ideal deliberators to imitate the imaginary discussion of the family" (p. 118). But this suggests bonds that do not in fact exist and run counter to his proposed strategy of inclusiveness for dealing with disagreements and complications in discussion (p. 120).

Now, Kitcher nowhere claims his account of ideal deliberation ensures that deliberators reach consensus. But failure of consensus is not my criticism. Problems arise not because people fail to reasonably approximate ideal deliberators but rather because what goes to make deliberators ideal proves to be either empty (assumes prior agreement on matters of significance) or idle (offers no basis for rational influence if parties disagree about significance). Kitcher's deliberative ideal does not require a view from nowhere but an equally implausible view from everywhere.

Which values held by individuals are open to influence by facts and which not? For example, those of us who believe that rights ought to be equally distributed find uninteresting debates about IQ. Nothing by way of rights hangs on how intelligence may be distributed across groups. Likewise, I suspect that those with whom I disagree about the moral permissibility of stem cell research and cloning would simply find my arguments beside the point. I do not see clumps of cells in early stages of development as rights bearing entities; others disagree. Too many beliefs basically immune to revision in light of facts buttress each person's views here. Nothing in the literature regarding abuses of science or scientific practice suggest that the failures involved anything like neglect in consulting some else's significance graph. Kitcher's prescription for change assumes that lack of information of a certain sort causes the process to go wrong. But what evidence do we have for this? (See pp. 195-97.)

Moreover, Kitcher, unlike Feyerabend, still uses phrases such as "scientific research," "scientific truth," and "scientific results" without asking exactly what work the term *scientific* is doing here. Scientific as opposed to what? To assume some set of conditions necessary or sufficient for defining a generic notion of science endorses a form of the "unity of method thesis" Kitcher otherwise claims to reject. But in denying that there exist such defining conditions, his use of the term fails to mark off research in any particular area from any other. This absence of a clear conception of what makes science science reveals another problem for Kitcher: his case for constraints, if correct, applies generally to all research or creative activity, for nothing about his

argument limits its applicability to one discipline as opposed to another.

Kitcher rightly questions the elitism that lies behind many claims that only scientists possess the capabilities to assess and guide research, which results in incursions of science and technology into contemporary life. As alluded to earlier, I find no reason to think that there exists a bright line separating cases of where we may exercise our freedom of expression even when it causes others upset or discomfort²⁸ from those where we may not because of harm to others. There may be merit to the suggestion that discussion of these matters needs to be more inclusive, but should not the same reasoning then apply to all areas of public funding? If NSF or NIH panels represent unacceptable elitism, what about NEA and NEH?

Fuller in this regard entertains the provocative suggestion that intellectual activities be cut loose from public funding altogether. Let the market, in effect, decide which science (or art or book projects) to fund. Kitcher, having glimpsed the moral complexities that arise if one transposes the fundamental Millian problematic of distinguishing between self- and other-regarding actions to the public sphere, never contemplates solving the problem by simply cutting the Gordian knot. The problem of which research to fund goes away if the government gets out of the business of funding research.²⁹ I confess deep uncertainty and ambivalence about this conclusion, but note nonetheless how consonant it is with the general themes on which Kitcher relies.

Against Kitcher's search to institutionalize a decision process for what makes research other-regarding, I invoke another famous observation from Mill: "But the strongest of all the arguments against the interference of the public with purely personal conduct is that, when it does interfere, the odds are that it interferes wrongly and in the wrong place."³⁰ Kitcher's proposals only aggravate and do not settle such fears.

NOTES

1. Certainly much of the work pioneered by Barry Barnes and David Bloor articulates this view. One of the best brief overviews of that program remains their essay "Relativism, Rationalism and the Sociology of Knowledge" (1982). A classic case study in this tradition is Shapin and Schaffer (1985). See also Pickering (1984).

2. Those despairing of efforts at conceptual analysis incline to the view that no quiddities have been found because none exist for the finding. For example, one most

plausibly read the term *natural science* as naming only an academic kind (the sort created by decree by university administrators) and not a kind marked by, for example, some special method or distinctive logical structure. Likewise, despite important and impressive results for the semantics of formal languages, nothing even close to a consensus exists for a systematic account of how the truth predicate functions for natural languages. Arguably, the jury remains out on democratic theory. My advice: don't hold your breath here either.

3. But matters prove not so simple. An issue not addressed by Kitcher concerns just which statements can stand as candidates for the truth predicate. Moral realists, for example, would declare certain normative statements eligible; rational inquiry can sort out just which. Objectivists with regard to aesthetic judgments maintain a similar position. Much depends, *inter alia*, on how one draws the cognitive/noncognitive divide (or if one draws it at all). I question the strategic presumption that one can dispel the mystery for a specific subject area, for example, the natural sciences, absent a general theory of what truthmakers are.

4. Kitcher runs together the notions of truth and accuracy, sometimes stating that "success betokens truths" (p. 26), sometimes speaking of "the connections we observe between success and accuracy" (p. 27), and sometimes speaking of representations as being "true or accurate" (p. 61). But I suggest both the notions of success and accuracy make implicit reference to interests of those involved. That is, it makes sense to ask of any representation (linguistic or otherwise), "Is it true?" But the questions, "Is it [a representation] accurate?" or "Is it successful?" cannot be answered without further information regarding the purposes for which the representation is wanted. To say the same of "true" relativizes the notion to interests, a view Kitcher must resist. This tension between the semantics of success or accurate and that of true breaks the link he attempts to forge between people agreeing on the success of a representation or judging it accurate and then inferring from either its truth. I elaborate on this point below.

5. I follow Kitcher's own lead here in focusing on critiques by sociologists of science of the sort of position which Kitcher defends. See, for example, Philip Kitcher (1993, chap. 5).

6. I assume for the sake of argument that the parties in a scientific dispute can in fact specify and agree on what the relevant evidence is. But as will become obvious, the sort of problems that Kitcher acknowledges apply, *mutatis mutandis*, to this notion as well.

7. On analogy with Ian Hacking's (1983, 23) view that if you can spray them, they're real, Kitcher offers what one might call representational realism—if a map or theory gets you where you want to go, it represents reality. This strategy nicely deflates the realism/antirealism debate into a quibble over the acceptable uses of "real."

8. I owe this example to Jim Bohman.

9. Danto introduces this in his book, *Analytical Philosophy of History* (1985, 147-82).

10. According to Kitcher,

When we think about scientific inquiry as responding to a relatively narrow range of explanatory projects, to wit the kinds of questions we find worth posing, there's little harm in conceiving of this type of patchwork. But when we drop the reference to ourselves and our concerns, I see no reason to think there's any manageable system at all. To put the worry bluntly, why should we suppose that the number of classificatory schemes and unified treatments for all nature's phenomena is *finite*? . . . To revert to the motivating analogy of the last chapter, the Unity-of-Science view made it look as though

there was a fundamental set of maps from which any map we might care to use could be constructed, and so gave content to the conception of the ideal atlas. Once we abandon that view, it looks as though all that may remain is a collection of charts that may proliferate indefinitely with our changing interests. (Pp. 72-73)

11. Kitcher develops here a neat example of different ways to account for why more males are born in any given year than females (see pp. 71-75). Of particular significance in his elaboration of this account is the explanatory appeal to the notion of an evolutionarily determined equilibrium. Explanation here depends on “seeing the state we wish to understand as an equilibrium and identifying the factors that maintain the equilibrium—in a sense a causal account, but one that doesn’t relate effects to completely specified antecedent causes” (p. 74). Clearly, appeal to equilibrium “explains” why, given the higher incidence of male mortality prepuberty, one needs a higher number of male births relative to females in order to obtain a 1:1 ration of males to females at puberty. But appeal to equilibrium provides no explanation whatsoever of why nature cares to have a ratio here of 1:1. Indeed, that this ratio looms into view as something to be explained probably says more the social preconceptions of the good Dr. Arbuthnot than anything else.

12. Note in passing how Kitcher’s characterization above breaks with that of his own earlier work (Kitcher 1993). Against the line espoused above, the Kitcher of *The Advancement of Science* maintains,

The deep point of the sociological critique is that the social forces that operate in this modification of practice—the rules for consensus shaping . . . the training process and broader socialization within a larger community—may be sufficiently powerful that the effects of nature are negligible. If correct, this point would undermine my account of progress from within. (1993, 162)

The key point in his earlier work concerned explanatory unification over (indefinite) time. “On my account the primary modes of scientific progress are conceptual and explanatory progress” (1993, 169). But the central elements of that program appear nowhere in the later account.

13. See Pickering (1984, 405-6). In fairness to Pickering, I note that in later work he at least appears to back off from such a hyperbolic formulation of his view. More representative of his current thinking is his *The Mangle of Practice* (1995). A classic and sensible statement of the sociological view which provides a philosophically nuanced presentation of the sociological position is David Bloor’s *Knowledge and Social Imagery* (1991), especially his Afterword. Note that the first edition came out in 1976. The core issues are not new.

14. An account that Kitcher needs to confront but does so neither in this book nor *The Advancement of Science* is Peter Galison’s impressive and influential work *How Experiments End* (1987). Galison examines just the sort of convergences of scientific opinion of which Kitcher makes so much, but indicates no place for Kitcher’s more metaphysical conclusions (see especially pp. 276-78).

15. According to Kitcher,

Frequently, relevance relations reflect our interest in the covariation of properties we find salient or in factors that we can manipulate and control. Objective explanation goes on in the sciences, then, but only against the background of our questions and our interest. The most we can expect from a

theory of explanation is some understanding of how these questions and interests shift as our inquires, and the complex environments in which they occur, evolve. (Pp. 75-76)

16. As Kitcher states,

A longer view would reveal that the questions we pose, the apparatus we employ, the categories that frame our investigations, even the objects we probe, are as they are because of the moral, social, and political ideals of our predecessors. (P. 86)

17. I develop this point at length in my "Will the Real Scientists Please Stand Up? Dead Ends and Live Issues in the Explanation of Scientific Knowledge" (Roth 1996).

18. Barry Barnes develops this notion in his early and justly influential paper, "Natural Rationality: A Neglected Concept in the Social Sciences" (1976). Kitcher alludes to this notion on p. 13.

19. Kitcher devotes one chapter—chapter 11—to a sketch of the policy divide between Vannevar Bush and Senator Kilgore. Here he only provides hints of the much deeper issues generating this divide. Unfortunately, Kitcher nowhere acknowledges or discusses the writings of Steve Fuller, surely the person who has a great deal of interest to say precisely on the topics of how federal science policy comes to have the form it does and on how to promote successful inquiry once one abandons the idea, as Kitcher does, that would link the success of science to some special method. Of particular note here are the following books by Fuller: *Philosophy of Science and its Discontents* (1989) and *The Governance of Science: Ideology and the Future of the Open Society* (1999). Fuller polemically presents his own views on how science comes to be disordered in contemporary society in *Thomas Kuhn: A Philosophical History for Our Times* (2000). I discuss Fuller's work in "The Bureaucratic Turn: Weber contra Hempel in Fuller's *Social Epistemology*" (Roth 1991) and "Fuller's '18th Brumaire of Thomas K'" (Roth 2001).

20. John Stuart Mill (1978, 12).

21. Mill (1978, 9).

22. I note in passing that Kitcher never confronts the other great champion of a Millian view of humans and their relation to scientific inquiry, namely, Paul Feyerabend. Perhaps this is because Kitcher worries only about how to constrain science as a species of inquiry, while Feyerabend worries about received science as exercising a constraint on inquiry more globally construed. Yet Kitcher should share Feyerabend's worry, since he too disavows any hegemonic view of what constitutes scientific rationality. In this book, Kitcher nowhere as explicitly characterizes the view of science he rejects as he does in chapter 1 of *Advancement*, but certainly the negative characterizations of certain views of science offered throughout Part I of *Science, Truth, and Democracy* strongly suggest no alteration in his views on the topic of "Legend." This points to an additional irony in Kitcher's account, for Kitcher's celebration of Galileo as a paradigm of scientific argumentation overlooks Alasdair MacIntyre's witty and apposite apothegm that Feyerabend "turned himself into the Emerson of the philosophy of science; not 'Every man his own Jesus,' but 'every man his own Galileo.'" With regard to Feyerabend on Mill, see my *Meaning and Method in the Social Sciences* (Roth 1987, 89-95).

23. Remarks by Stephen Turner alerted me to the Rawlsian elements in Kitcher's account.

24. John Rawls (1971).

25. If Paul Feyerabend has title of the Mill of natural science, Kitcher is its Lord Devlin.

26. *St. Louis Post-Dispatch*, 3 June 2002, p. B7.

27. Although he treads over some of the same ground and comes to not dissimilar conclusions, conspicuous by its absence in Kitcher is any discussion of Steve Fuller's account of the distorting impact of elitism on science policy. See Steve Fuller (2000).

28. Here in Missouri, the state legislature voted in its current (2002) term to withhold money from the University of Missouri system because a professor published an article questioning, for certain cases, the harm of under-age sex.

29. Joe Rouse suggested two important caveats here: First, we should not think of "leaving it to the market" as some kind of neutral arbiter, without recognizing that markets are institutions with their own structures and norms which incline outcomes in particular directions, often in close involvement with political institutions and practices (e.g., should government get out of the patenting business?); second, government is not merely an active producer of scientific knowledge but also an avid consumer, in ways that raise many of the same problems (should we also ask that government avoid relying on any scientific work as well as avoiding supporting any?).

30. Mill (1978).

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