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# Genuine Problems and the Significance of Science

Matthew J. Brown

This paper addresses the political constraints on science through a pragmatist critique of Philip Kitcher's account of "well-ordered science." A central part of Kitcher's account is his analysis of the significance of items of scientific research: contextual and purpose-relative scientific significance replaces mere truth as the aim of inquiry. I raise problems for Kitcher's account and argue for an alternative, drawing on Peirce's and Dewey's theories of problem-solving inquiry. I conclude by suggesting some consequences for understanding the proper conduct of science in a democracy.

## 1. Introduction

My topic for this essay will be the social and political constraints on the operation of science. Modern science is a large-scale social and institutional endeavor, and in order to understand it, we need to understand its role within society and amongst our political institutions. What will be the research agenda for science? How should we distribute funding amongst potential and ongoing scientific projects? How should science be arranged in order to be just? What are the social and political responsibilities of scientists qua scientists?

To many scientists and philosophers of science, these questions will seem inappropriate. It has been a widespread belief that science is an essentially value-free activity, especially in philosophy of science after World War II.[1] When it functions well, it provides for us a store of objective truths. When moral, political, and social values enter in, they are essentially corrupting – Lysenkoism is a stock example. Technology, on this common view, is just the application of science and instrumental rationality towards some goals – while values enter in, it is only as goals set from the outside. This view is generally shared both by the boosters and debunkers of science, differing over whether actual science manages to live up to this ideal or whether science has become "corrupt."

It is becoming harder and harder to deny that values – not just so-called "cognitive" or "epistemic," but moral, social, political values – play an essential role in science, and that science is essentially a social activity. At the same time,

many now argue that this need not threaten the integrity of science. A growing number of philosophers are attempting to craft a new image of science, in which the role of values of science are not corrupting, in which they might even play a positive role. In such efforts it is common to explore the social nature of science, determine the proper relation of science to democracy, and problematize the simple dichotomy between science and technology. John Dewey also rejected the view of science as value-free that later became the orthodox view; unlike many present-day philosophers of science, he was not trained under the subsequent orthodoxy. His work provides a useful starting point for trying to understand values in science and science in democracy, in part because Dewey does not face the threat of falling into old, bad assumptions about science (he was never taught to make them). By taking the lead from Dewey, I hope to start from a position free from the mistaken assumptions and false starts of the tradition in philosophy of science.

In this essay, I will analyze recent work by Philip Kitcher in which he works towards an image of science as value-laden. Kitcher has in recent years begun to draw on a variety of pragmatist ideas and espouse some distinctively pragmatist views. I will analyze Kitcher's presentation in his *Science, Truth, and Democracy* (2001), which sets out a two-part theory of the relation of science to democracy and the social, political, and moral constraints on science: First, he provides an argument for viewing science as context-dependent but nonetheless objective, in which the concept of *scientific significance* plays a major role. Scientific significance is supposed to capture the knowledge that a scientific community or discipline has about what areas of research are important, of interest, or worthy of attention. Second, this context-dependent representation of scientific significance is used as an input to an ideal democratic deliberation procedure, in which ideal representatives of the preferences of citizens deliberate and attempt to reach consensus in order to determine the ideal research agenda for science (in our liberal democracy). He calls this ideal "well-ordered science." The philosophical-epistemic story about what is significant about science is thus a first step in a socio-political ideal of science. This ideal will be useful, e.g., in funding decisions and decisions of individual scientists in what research to pursue, because we can compare the actual situation and future options to the ideal.[2]

I'm going to focus here on the first part of the story, the account of scientific significance. This paper will challenge, and attempt to improve on, that account, and then trace briefly the consequences for the relation of science to democracy. The main challenge is that Kitcher's account of significance leaves out too many of the concrete features of the contexts that give the sciences their significance. Kitcher captures some of the conditional or relational components of what makes certain scientific pursuits or claims significant. I argue that these are not enough, however, and that he leaves out components of significance that are immediate or inherent in the practice itself.

John Dewey's pragmatist theory of how problems arise and spur inquiry provides part of the missing story. By analyzing how problems arise from concrete situations, by understanding when and how such problems are genuine, we can also get a better picture of how significant they are. Dewey said that it was the neglect of the "context which controls the course of thought" (*Context and Thought*, LW 6:6)[3] which was the most serious and pervasive fallacy of philosophy. Kitcher does much to avoid the problem, but not enough. I will try to take the project one step farther.

I first summarize the motivations for Kitcher's search for an account of the significance of science, and in particular, motivate the type of contextualist account Kitcher is after. I then give an overview of Kitcher's account and raise some objections. I next outline the main features of the pragmatist theory of inquiry common to Charles S. Peirce and John Dewey, which I use to construct an alternative account of scientific significance which avoids the mentioned problems. I conclude by indicating the consequences that would follow for the second part of Kitcher's project: understanding the relation of science to democracy.

## 2. Why Significance?

Kitcher is not the only philosopher of science to have searched for an account of the significance of science. The positivists sought criteria of "cognitive significance" that would rule out all non-scientific statements as meaningless. More recently, Joseph Rouse has argued that an account of the significance of scientific practices ought to be a category at the forefront in science studies, helping analyze every level of practice (Rouse 1996, 25ff). Kitcher's project, like Rouse's,[4] aims to answer a range of questions, some of which were traditionally filled by less modest traditional notions such as "objectivity" and "objective explanation." Kitcher's project also bears similarities to Larry Laudan's analysis of the evolving aims of science in *Science and Values* (1984), though the latter attends exclusively to so-called "cognitive" values.

One way Kitcher motivates the need for an account of "significance" over and above mere truth is by an analogy to maps. One might imagine that the ultimate goal of cartography is the production of an ideal atlas, a set of maps which can be used to serve any purpose. Kitcher thinks the possibility of such an atlas that is sufficiently comprehensive and practically useful is absurd: "There is no good reason to believe in the ideal atlas" (60). The wide variety of actual and possible aims served by map-making, the competing constraints, the need for selectivity in crafting useful maps, and the finitude of resources, casts doubt on the realizability and even the coherence of an ideal atlas. The only map with sufficiently rich information for all purposes is the territory itself; but the territory itself is not a map at all.

To see why this is so, consider three different maps: a topographic map, an electoral map, and a subway map. The topographic map contains many

geographical and geological features, and is especially informative about changes in elevation. On the other hand, one would have a difficult time navigating a city based on a topographical map, since so little of the available information is relevant. An electoral map – of the sort so many of us were obsessed with throughout the latter half of 2008 – contains precious little in the way of the information on the topographic map. There are no roads, no landmarks, no cities; no changes in elevation, rivers, or lakes. About all the map shows are political divisions – states, counties, districts – and the predicted or actual pattern of voting within those divisions. A subway map bears some resemblance to an electoral map: its geographic features are distorted, it contains little information about streets or natural landmarks. These simplifications are necessary to the effectiveness of the map, and even the basic relations of north-south, east-west are optional (and sometimes left out in the ones on the subway car itself). These examples give a clear picture of the way in which constraints of map-making compete, and how intimately tied up they are with our purposes. These capture just three of a potentially infinite variety of maps serving our potentially infinite variety of purposes (to say nothing of things like star maps and maps of abstract spaces).

So we must understand maps as representing territory in a way that picks out the significant features for *our* particular purposes. As it goes for maps, so too for science. Science does not merely seek truth, but *significant* truth. Mere truth is no good: most truth is uninteresting (the infinity of truths about the contents and arrangement of my office over time, for example), and some of it is unwelcome or dangerous. What we want is *significant* truth, the significance of which, Kitcher argues, is highly contextual and interest-relative: "[W]hat counts as significant science must be understood in the context of a particular group with particular practical interests and a particular history" (61).

It is important to point out what significance is and is not supposed to capture for Kitcher. It does not provide an answer to the old "demarcation problem." Whether some truth or some line of inquiry counts as significant is not meant to tell you that it is or is not "scientific." I will follow Kitcher and presume from the outset a rough-and-ready understanding of what science is, and that we are talking about the sciences already. What this talk about "significance" is meant to capture is the relative importance of different parts of science. For Kitcher, this importance must be understood in terms of *our* goals, purposes, and interests. Further, though everyone should recognize that *practical ends* play an important role in attributing significance to certain scientific projects, what is needed is a portrayal of a goal that is distinctively *epistemic.*

The objectivist or strong realist might try to avoid Kitcher's move by seeking an objective goal for science. They would thus account for which truths are scientifically significant in a context-free way, as being those truths that contribute to the objective goal. Kitcher considers several traditional views on the epistemic or theoretical aim of science, including identifying laws of nature, providing a unified account of nature, or discovering the fundamental causal

processes (66). Each of these fails, because of the difficulty of answering for each, "What would be so valuable about gaining that?" (66). Once we rule out practical and theological justifications, it is hard to find any justification for these goals. According to Kitcher, the most promising traditional view is: "The (epistemic) aim of science is to achieve objective understanding through the provision of explanations" (66). Objective understanding in this sense is *not* based in the activity of explanation that responds to actual questions, but is the recognition of whatever special facts or relationships exist that *grounds* particular explanations (if they are genuine or objective).

The reasons that this view fails, according to Kitcher, are neither subtle nor complex. We are seeking an understanding of scientific significance that will help us pick out important parts of science from the myriad of banal facts. Thus, the aim of science, if it is to be "an all-purpose explanatory device" that is context-independent, it must be systematic. It will fail "if it is simply a long list of potential explanations, one for each context" (68) because then it will fail to sort the epistemically significant from the significant, including everything somewhere on the list. The easiest way to guarantee this sort systematicity is to defend some sort of Unity-of-Science view,[5] in which intertheoretic reduction of some sort could be attained between the various special sciences, including definitions that could link the diverse vocabularies of the various disciplines (69). The failures of these views is familiar: the successful cases of reduction from which the movement drew inspiration were of a fairly limited class involving individual or small clusters of laws, whereas it is difficult to imagine that much of biology or psychology could take this form; there is much science that has little or nothing to do with general laws at all (69); linking definitions between theoretical vocabularies seems a near-impossible goal for disciplines like psychology (69); the crucial features of many sciences involves "the form of [the] processes, not the material out of which the things are made" (70–71), and these forms are quite diverse and explanatory, in many situations in which a reductive explanation would have zero explanatory power; consider Kitcher's example of trends in the number of births of males versus females: an "explanation" in terms of the psychiochemical basis of this trend would not advance our understanding at all, whereas a non-reductive explanation in terms of selection pressure would be much more helpful. Just as the idea of an ideal atlas to serve all possible cartographic purposes is untenable, so too the Unity-of-Science view fails.[6]

One might argue that the failures of this view, instead of signaling the impossibility of objectively sorting significant from insignificant truths, merely shows that it is an open project for philosophy of science to discover what notion of objective understanding will serve the purpose (73).[7] But again, this approach will fail if all kinds of "mundane truths" are counted as significant (73). It won't be the case that everything in the store of information in which objectively complete answers lie, the store that picks out the truths that are significant, will be relevant to *any* question, because of the failure of the Unity-

of-Science view. So we will need a way to filter just the truths that are "pervasive" (but not completely so) from the banal (74). One possibility is to say that whatever truths play a role in a *complete causal narrative* of an event are the objective explanatory resources for the event, but this fails because often the causal history doesn't give us the explanation we need (as in cases in which structural features or equilibrium conditions do the explaining) (74), and indeed any truth will figure in *some* causal narratives (74–75). One could try to solve the filtering problem by *counting* the number of explanations that each truth might play a role in, but this will fail because any true statement will figure in an infinity of possible explanations (continuum many, if time is continuous) (75).

The general problem here is that our actual, everyday explanations are quite heterogeneous both in the questions they answer (not just "Why?" but also "How?," "What?," "How is it possible?," etc. (73)), in the kind of information (causal or otherwise) that they rely on, and in what determines what is relevant to that explanation. Explanation is a task that is too context-dependent to be given a context-independent foundation. It is not that there is no such thing as objective explanation (in line with the ideal of objectivity that Kitcher pursues in Chapter 3):

> Objective explanation goes on in the sciences, then, but only against the background of our questions and our interests. The most we can expect from a theory of explanation is some understanding of how these questions and interests shift as our inquiries, and the complex environments in which they occur, evolve. (75–76)

Hence the need for a theory of the significance of science: we want to know the aim and importance of inquiry; "discover truth" will not do, as most truth is banal and insignificant; none of the accounts in terms of laws, causes, unification, or objective explanation that is free from considerations of context and interest will do; thus, we need to understand how our questions and interests, both practical and theoretical, work to pick out certain things as significant.

### 3. Kitcher's Theory of Significance: Significance Graphs

Kitcher's explanation of how elements of science count as significant proceeds from his insights into the complex interconnectedness of science. We are naturally interested in a number of broad questions, such as "What were our hominid ancestors like?" and "How do single-celled organisms regulate their metabolism?" (76). In addition, much of science is concerned not with general laws or broad questions, but with rather narrow issues and very particular results (76). Large projects and more mundane accomplishments are interconnected (76–77), but the flow of significance should not simply be seen as going from

the theoretical top to the particularistic bottom (77). Epistemic and practical interests are interwoven (76). So a treatment of significance ought to provide a picture in which "the connections that confer significance seem to radiate in many different directions" rather than being a simple hierarchy (77).

Kitcher uses an apparatus he calls "significance graphs" to capture the way that different parts of science get their significance. They are directed graphs that show connections between the research projects, questions, problems, claims, techniques, parts of the natural world, and practical goals dealt with by a scientific field. (See Figure 1 for a toy example which traces the significance of some areas of thermodynamics.)

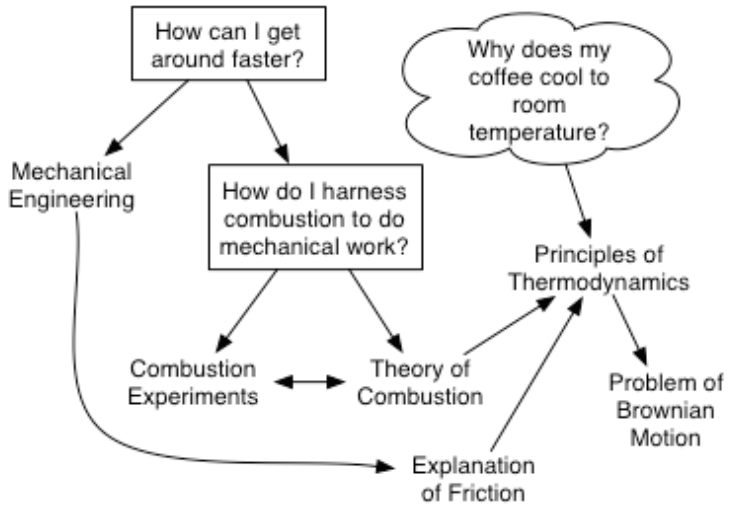


Figure 1: Toy Significance Graph. Practical goals are outlined in square boxes, and questions about which we might be naturally curious are outlined by clouds (my addition). See figures 1 and 2 in Kitcher (2001, 79–80) for more detailed examples.

Significance graphs display the ways in which particular scientific efforts come to inherit significance from other projects. They are indexed to a particular time and will change dynamically as the field in question develops. The significance graph is meant "to make explicit what workers in the field know at the time" (78); they are part of what we might call the "disciplinary matrix" of the field.[8]

Notice that the information these significance graphs capture is relational or conditional. These graphs trace the ultimate source of significance to either practical questions (boxes) or questions that stem from "natural curiosity" (clouds). The latter is the basis of the *epistemic* component of significance in Kitcher's account. Everything else has significance *only derivatively*, via an

inheritance arrow drawn to it from a practical concern, a natural question, or another scientific concern. Thus, the significance of most scientific concerns is a mix of practical and epistemic components, deriving ultimately from these sources. There are many ways the inheritance can work: a more technical question must be answered in order to help answer a larger question; data can serve as evidence for a claim; new experiments are suggested by a comprehensive theory. Not only does the explanation of scientific significance by way of significance graphs account for all the insights and concerns above, but the significance-graph framework also takes into account the variety and the complex interconnections of scientific activities, and the fact that significance is dynamic and historically situated.

## 4. Problems for Kitcher's Theory

The major problem for Kitcher's theory is the weakness of his account of the epistemic sources of significance. While Kitcher is committed to practical and epistemic sources of significance being interwoven, he does not want to reduce all significance to practicality. The only other source, on Kitcher's account, is the contribution of "natural curiosity." This is a weak peg on which to hang the significance of science.[9] Recall the test that Kitcher applied to other candidate accounts of the aim of science: "What would be so valuable about knowing that?" When we ask this of one of these "natural" questions, Kitcher has little to say. He insists that

> Human beings vary ... with respect to the ways in which they express surprise and curiosity.... But ... we do count some of our fellows as pathological, either because they obsess about trifles or because they are completely dull. In claiming that *sciences ultimately obtain their epistemic significance from the broad questions that express natural human curiosity*, I am drawing on this practice of limited tolerance, on our conception of "healthy curiosity".... (81, my emphasis)

This story about "the ultimate source of epistemic signifance" he says, is "commonplace and disappointing to those who expect a grand theory that will invest the sciences with overriding importance" (80).

Not only is it commonplace and disappointing to those who exalt science unduly, it is difficult to see how the significance of scientific projects, even on a *modestly* pro-science account, can have its source *entirely* in practical questions and curiosity. Are the questions and projects at the bleeding edge of science all ultimately of interest only through the practical projects they might relate to and the very general questions about which we are "naturally curious"? If epistemic significance comes down to a purely subjective feeling of curiosity, natural though it may be, the whole project of distinguishing scientific from mere utilitarian significance hangs on just a feeling. By pushing back the explanation

to items of *natural curiosity*, his account of significance hangs on a claim that I find doubtful: that people will, *without further reason*, agree on a broad swath of what they find interesting. Put another way, Kitcher's ideas will radically underestimate the significance of many projects in science. The mere fact that some or even many people feel a bit curious about some topic counts for very little in the face of our pressing needs. Practical significance will undoubtedly wash out the effects of curiosity.

Kitcher's criticisms of traditional accounts of the context-independent aim of science turn on their inability to answer the question "What would be so valuable about gaining that?" Could *they* not answer in the same way that Kitcher has? Why can they not simply reply that those who cannot see the inherent value in such pursuits are dull and incurious? If the answer is not satisfactory in their case, it will not work in Kitcher's, either. A related and more familiar situation might be trying to explain the significance of technical work in philosophy by referring to general questions that people should *obviously* be naturally curious about, like "What is knowledge?" or "How are scientific concepts related to the world?" I have found that in the face of such claims, many people remain pretty unimpressed. Perhaps most of the non-philosophers I know are just dull, but the suggestion is at least impolite and at worst overwhelmingly elitist – a bad start for an attempt to communicate with laypersons[10] about the significance of science.

What's more, Kitcher also underestimates the potential for idiosyncrasy of curiosity. A significance graph crystallizes the implicit knowledge *of a discipline* as to what is significant in that field. The broad, "natural" questions in their significance graph then need not necessarily be natural for everyone. The questions that drive my basic curiosity might only be "natural" for people like me in certain respects, and that respect might be what draws people to say, physics, but not to microbiology. The questions that most physicists are "naturally" curious about might be quite idiosyncratic. For example the micro-biologist and nobel laureate Salvador Luria (1984) "confess[es] a lack of enthusiasm ... in the 'big problems' of the Universe or of the early Earth" (119).[11] The questions of supposed "natural curiosity" which drive astronomy, physics, or even much of biology would be of little interest to Luria, as compared with the concrete problems facing microbiologists, about which it is possible to make obvious progress. In such a case, Kitcher will either devalue the field (who cares what those physicists are curious about? ), or become an elitist (such that only physicists determine or have access to whether their projects are significant), which is in tension with his attempt to subject scientific aims to what ideal democratic layperson-deliberators would choose. Of course, there is no reason that curiosity can't or shouldn't play a role in attributing significance; but it is inadequate to carry so much of the project.

To sum up, the problems with Kitcher's use of "natural curiousity" as the basis of epistemic significance are:

1. It fails his own test for candidate aims of science: "What would be so valuable about knowing that?" As a result, his defense of natural curiosity undermines his criticisms of other, anti-contextualist approaches.
2. Curiosity seems too subjective to account for the epistemic significance of science.
3. Practical problems count for much more than curiosities, and thus the epistemic significance of science will be washed out by practical significance.
4. There are reasons to think that curiosity might be idiosyncratic to a scientific field or even within a field. To the degree that curiosity is idiosyncratic, Kitcher faces the dilemma of degrading the epistemic significance of vast swaths of science or falling into extreme elitism about whose curiosity counts.

In the face of these problems,[12] I suggest an alternative approach, based on the pragmatist views of Peirce and Dewey. Their theory of inquiry can help us further understand the problems with Kitcher's theory, as well as pointing the direction towards fixing it.

## 5. The Pragmatist Model of Inquiry: Peirce's Insight

It is worth setting out some key features of the pragmatist theory of inquiry developed in detail at this point, with an emphasis on those features of the theory relevant to the question of the significance of science and the value of various lines of inquiry. I will begin with Peirce's somewhat simpler way of putting the point, by way of introduction, before explaining Dewey's view and showing how it should be developed. This requires a divergence from addressing the main topic – significance – to which I will return in §7.

One of the founding insights of Peirce's pragmatism was his analysis of the structure of belief-formation,[13] which provides an important distinction between *genuine* doubt that leads to inquiry and new belief, and *paper* doubt that is often used for pernicious philosophical purposes. The basic idea is the familiar difference between the experience of an actual, pressing problem or a real, nagging uncertainty, versus the posing of an idle question, the seemingly silly questioning of what's obvious without any *reason* behind it.[14] Peirce claims that all inquiry begins with genuine doubt. One way Peirce offers for understanding genuine doubt is by contrast to the methodological doubt of Descartes. Such doubt is complete and schematic, and, Peirce thinks, feigned. According to Peirce, this method is fruitless, because *genuine doubt* requires more than just putting a question on paper:

> Some philosophers have imagined that to start an inquiry it was only necessary to utter a question whether orally or by setting it down upon paper.... But the mere putting of a proposition into the interrogative form

> does not stimulate the mind to any struggle after belief. There must be a real and living doubt, and without this all discussion is idle. (EP 1: 114–115)[15]

What distinguishes genuine doubt is that, first, it must be felt or experienced. The feeling that Peirce talks about is variously characterized as one of unease, surprise, or novelty. Second, it is not *merely* a feeling, but also a practical obstacle, in a very broad sense of "practical": it is an obstacle to the continuation of some human activity. Without these new beliefs, one is unable to move forward. It is an experience that breaks up old beliefs and habits, and leads one to struggle after new beliefs. A simplistic example is coming to an unexpected fork in one's path through the woods. You are surprised, and perhaps uneasy about which way to go. You must settle at least on a tentative belief about which way to go before you can move forward.

Peirce argues that the formation of *all* beliefs has a complex logical and temporal structure, and no belief can arise immediately.[16] Peirce's model, which we might call the doubt-belief model of inquiry, proceeds from *genuine doubt* into *inquiry* and finally to *settled belief.* This is the core of Peirce's theory of inquiry. Genuine doubt must precede (or be an early stage in) any genuine inquiry. The temporal development of inquiry, when it is successful, moves away from this doubt and towards some resolution.

## 6. Dewey's Elaboration of the Model

John Dewey takes up Peirce's line of thought in his own writings on logic and inquiry:

> The function of reflective thought [i.e., inquiry] is ... to transform a *situation* in which there is experienced obscurity, doubt, conflict, disturbance of some sort, into a situation that is clear, coherent, settled, harmonious. (*How We Think*, LW 8:195, my emphasis)

The affinity with Peirce is clear, in that inquiry takes us from a situation that is (among other things) doubtful to one that is settled, but Dewey elaborates and transforms Peirce's insight. The most crucial transformation is from Peirce's terminology of mental-states like doubt and belief to Dewey's discussion of "situations." A situation is not merely personal and subjective; it includes the whole person *or group of persons* and the constituents of their environment relevant to the inquiry or practice at hand.[17] Problems of inquiry do not arise as purely intellectual matters, but rather due to "incidents occasioning an interruption of the smooth, straightforward course of behavior and that deflect it into the kind of behavior constituting inquiry" ("Reply to Albert G. A. Balz," LW 16:282). An indeterminate or problematic situation for Dewey is a "breakdown" of practice, as it is for Heidegger, and in both cases it is what

makes reflection and knowledge possible.[18] The unit of analysis is not the *mind* but *behavior* or *practice.*

We do not begin inquiry with an already set problem, but with some *problematic situation*. Dewey uses the phrase "problematic situation" in order to emphasize that the location of a perplexity is not simply in the mind of the inquirer, but in the whole situation. A *problem*, or a problem-statement, is an explicit formulation of the source of what is problematic about the situation, i.e., it states what the difficulty is and which factors contribute to it. It is hard work to get the problem right: "A problem well-put is half-solved" (LW 12:112). And in fact, we should say also that a problem completely-well-put is entirely-solved. That is, we can never quite set a statement of the problem in stone until we've found its solution, since it could always be that to find the solution we might need to reformulate the problem.

Consider a common situation in medical practice. A patient comes in showing familiar symptoms, and the physician prescribes the usual antibiotic. If everything works out fine, the smooth course of behavior continues; there is no "inquiry," properly so-called. On the other hand, if the antibiotic doesn't seem to work,[19] there is a disruption of the habitual course of activity. As the physician, looking at what you have in front of you, it isn't clear which way to go, what the features of this situation signify. You must dig for more evidence, consider alternative explanations, and try to sort out what to do before proceeding with a course of treatment.

A second difference between Dewey and Peirce is the phenomenological richness of the terms of Dewey's account, the elaboration of the qualities that characterize the initial and final moments of inquiry. Thought begins with a situation that is obscure, doubtful, conflicted, disturbed, etc., and it terminates when the situation attains clarity, coherence, settledness, harmony. What Dewey provides here we might call an *aesthetics of logic*, an analysis of the nature and role of qualities in the production and guiding of inquiry.[20]

Unlike Peirce's terminology of "belief" and "doubt," which connote subjective, individual mental states (despite Peirce's own understanding of such terms in terms of habits and practices), Dewey rigorously avoids presupposing fixed dichotomies of mind/body and individual/world in laying out his phenomenological description of problems and inquiry. Dewey considers all human activity to be a species of embodied life-activity, in which an organism is always engaged in transactions with an environment. In this situational picture, qualities of the situation like "doubtful" or "indeterminate" describe the transactions between organism and environment, the particular character of the goal-directed, situated activity of an embodied creature. "We are doubtful because the situation is *inherently* doubtful" (*Logic*, LW 12:109, my emphasis). The indeterminacy of the situation is not *merely* a subjective feature, but rather an objective imbalance or disequilibrium in the organism/environment system. Subjective states of doubt that are not evoked by a "doubtfulness" or instability in the situation are pathological (ibid.).[21] Furthermore, Dewey allows us to leave

open the possibility that the inquiring agent is not an isolated individual who doubts or believes, but an entire social group that responds jointly to a doubtful situation and works to resolve it. Indeed, such a possibility is necessary if the theory of inquiry is to be useful in understanding science, which is essentially social.

The difference between *curiosity* and a *doubt or problematic situation* must be emphasized. The difference is not that doubt is a more practical affair that curiosity, in Kitcher's sense (that would collapse the practical-epistemic distinction into the practical, and beg the question against Kitcher). Rather, the difference lies in the fact that curiosity seems far more intellectually idle, a question posed in a moment of fancy and speculation, while doubt arises in the course of activity, and presents a blockage of such activity. The activity may be as "unpractical" as you like, as far removed from the immediate needs of getting food or a living, from application in engineering or industry: composing a concerto or constructing an axiomatic mathematical system. Doubt is a hesitation in the vital beliefs that structure that activity and that press for solution. Doubt is a discoordination that is not merely a subjective feeling; hence, Dewey's use of the term "problematic situation." This is not to diminish the importance of curiosity, especially as a personal or social tendency. Indeed, the curious mind is the one that asks questions that may lead us to discover dubious ideas and unstable situations, and so allow us to resolve problems before they catch us unawares. But it is in the process of creating real and living doubts that curiosity becomes significant. The mere questioning of a thing does not make that question significant in any degree.

There is another reading of "curiosity" which makes it equivalent to doubt or perplexity in the pragmatist sense.[22] If Kitcher had mean this, however, there would still be reason to fault his account. On the pragmatist theory of inquiry, there are no "natural" curiosities just as there are no universal doubts. Doubt, curiosity, perplexity, or problematic situations are individual, particular, and situational. If understood in this way, curiosity is not so much the locus of Kitcher's problem, but the quasi-foundational attempt to find context-free, universal (though human) sources as grounds for the epistemic significants of particular projects.

I will now indicate how the pragmatist theory of inquiry bears on the question of scientific significance.

## 7. Genuine Problems and Scientific Significance

There are two ways we can connect the previous remarks on inquiry and genuine problems to Kitcher and the question of how to assess scientific significance. The first is to make *genuineness* a necessary condition on a problem having *any* significance. The second is to look more deeply at the factors which *make* a problem a genuine problem, and see if that can give us a lead on how to assess *degree* or *amount* of significance. First, I'll use these two

general ideas to diagnose what is dissatisfactory about Kitcher's account, then I will fill out the alternative view.

The first problem with Kitcher's account is that just because one *can* trace out some logical connections between what is going on in the field and some new question, that doesn't really make the question a significant one. To put it in the pragmatist idiom, you could sit down and draw out a significance graph for many a "paper problem," but that doesn't make it a real problem. Surely, Descartes' evil daemon has certain connections to any area of inquiry whatsoever; if the evil daemon exists, then we can't trust the results of any observation or reasoning. And yet, this isn't a serious worry for any scientist or scientifically-minded philosopher, and not because skepticism has been directly refuted. In other words, having significance-graph connections a'plenty is not a sufficient condition for significance.

The second problem is that Kitcher seems to deny the possibility that truly novel areas of inquiry can arise and still be significant. It seems possible that a whole new area of inquiry might open up in an area of practice hitherto unproblematic, or even in an area not known before to exist.[23] Such an area might have thin connections on a significance graph to prior scientific pursuits, or even to narrowly practical application and natural curiosity, and yet capture our attention in a way that makes it quite significant. It seems then that being thickly connected via significance-graphs isn't even necessary for being very significant.

The crucial problem with Kitcher's account is that despite aiming at a contextual account of scientific significance, the significance-graphs only relate the particular context of an inquiry or project to context-free, quasi-universal sources of significance: practical advances that meet basic human needs and wants and questions about which all human beings are naturally curious. While practical advances can be more clearly understood in the context of particular, current, culturally-situated projects, the force of epistemic significance in Kitcher's account depends upon the dubious universality or naturalness of general curiosities. A contextualist account of significance of scientific projects should turn away from these generalities and look to the particular features of the problem-situation to derive the significance of those projects. These features will still include systematic and historical connections to other human concerns, but those distant features will not exhaust our account of significance. While Kitcher has shown the way in demonstrating the need for a context-dependent theory of the significance of science, much work is left to be done in providing an adequate answer. He needs yet a stronger grounding in the concrete features of the situation in order to limn the significance of scientific pursuits. In other words, we need to understand not only the intellectual-historical context of items of science, but the concrete situational context that constitute the problems that science aims to resolve. Here's how I think the account ought to go:

Inquiries have significance in virtue of addressing some genuine problem. The conditions of genuine problematicity tell us whether some pursuit is

significant. In other words, *it is a necessary condition on attributing significance to some inquiry that it address a genuine problem, and any work on mere "paper problems" is disqualified from being counted as significant.* Abstract skeptical worries don't count as significant problems. Problem sets in a college physics course aren't significant scientific research.

Secondly, the amount of significance depends on the features of the context or situation that make a problem genuine. Remember, a genuine problem is based on a real problematic situation. A situation is defined by a certain practice, and the situation becomes problematic when that practice is disrupted. The key questions for determining how *significant* the problem is, I want to suggest, depend on just what is the practice, the situational transaction, that is disturbed. *How important is that practice*, and so what is the urgency that we resolve the disturbance? *And how much is it disturbed?* (See Figure 2).

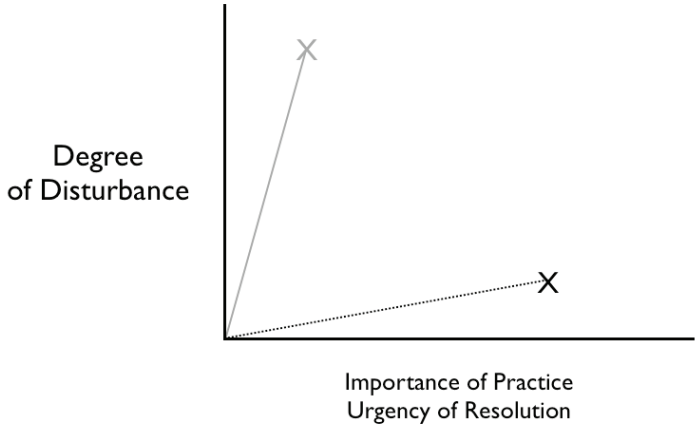


Figure 2: Measure of Immediate Significance. In addition to the kind of relational information about significance captured by Kitcher's significance graphs, we must take into account the degrees of immediate significance of an inquiry. Problematic situations can arise from greater and lesser degrees of disturbance of some standing practice, and the practice itself has some importance that indicates the urgency that disturbances of it be resolved.

We can imagine a small disturbance in a quite important practice may be very important. For example, suppose that we become aware of even a relatively small flaw in the practice of vaccination, such as a very low level uncertainty about its side effects. Because of the importance of vaccinations to modern medicine, this presents itself as a crucial matter. Second, consider a rather large disturbance in a much less important practice. Suppose you put very little stock

in research in high energy physics.[24] Nevertheless, a problem which shakes that area at a fundamental level might be quite significant indeed. Kitcher's significance graphs cannot capture the qualitative differences between these cases, nor will they attribute the right degree of significance unless these factors always lead to greater numbers of connections to other parts of science, which seems doubtful.

Convincing others of the significance of a problem that occupies you looks very different according to this alternative. For Kitcher, it should be enough to trace logical, causal, or historical connections between your concerns and basic practical applications and questions that excite natural curiosity. All it should require is an accurate significance graph. On this alternative account, you have to make three kinds of argument. You have to show that you do, indeed, face a genuine problem, rather than a mere paper problem. You have to provide positive reasons for doubt or show that the practice really fails to provide a sure plan of action in the present circumstances. Second, you have to convince them that it is a serious rather than minor disturbance in the practice. Finally, you may have to argue that the practice itself is important.

Kitcher's significance graphs will not work as a way of representing the full significance of inquiry, but they may serve other useful functions in understanding and communicating about scientific significance. They do succeed in providing due recognition to the complex connectedness of science, and they may provide for us a spur to new inquiries, helping us discover new problematic situations that have not yet come to our attention. But they are not the right starting-point in understanding the significance of inquiry. Kitcher over-intellectualizes the problem of significance; in his account, it is the problem-formulations themselves that matter, not the indeterminate situations that lie behind them. What's crucial is that we begin with *practices that matter*,[25] and the more or less serious problems that arise in the course of those practices. This is what makes the problem-solving endeavors of science significant.

This pragmatist alternative to Kitcher's framework for assessing the significance of science avoids the problems that plague Kitcher's own account:

1. It stands up to Kitcher's test by answering "What would be so valuable about knowing that" in the following fashion: resolving genuine problems is always valuable in that it removes a difficulty for a practice that matters or, in the worst case, shows that that practice is ultimately unworkable and must be abandoned or replaced.
2. It is not too subjective, because genuine problems are never merely the result of subjective factors. It is not by mere fantasy or appearance that the transactions between the world and the practitioner become discoordinated.
3. Epistemic significance does not reduce to practical significance, in Kitcher's sense of "practical." Many of the scientific practices that constitute established scientific disciplines, during the pursuit of which

problems arise and inquiries are spurred, have an importance that far exceeds their distant connections to practical results. How these practices come to matter is a complex historical affair; that they matter is a much more certain thing than is any reductionist attempt to explain why they matter in more fundamental terms. However, all scientific significance is "practical" in a much broader sense, the sense connected with the meaning of the term "pragmatism," meaning merely that it is connected with the practices and activities that constitute life.

4. Is significance on my account idiosyncratic? Yes and no. No, in the sense that whether a genuine problem exists is not just a matter of personal preference; no amount of desiring it to be otherwise will make it go away, and no amount of questioning an unproblematic situation will make it problematic. On the other hand, a genuine problem might be idiosyncratic in that it might be a problem for a practice that you're not engaged in or don't care about. But these sense of idiosyncrasy will have some impact on the importance of the practice, and so significance and idiosyncrasy will vary in the right way.

One nice feature of this framework is that it nicely tracks some of the features of a method already in place for evaluating the merits of scientific projects for pursuit. It is common to evaluate grant proposals based on both their relevance to solving problems that are important to the progress of a field or fields and also to consider the importance of that advancement, including intellectual, educational, and social benefits. The latter is important as a consideration relevant to the second stage of Kitcher's account: the democratic ranking of projects. It is to the implications for this part of Kitcher's project that I will now turn.

## 8. Consequences for Science and Democracy

Attempting to trace out the consequences of these criticisms and alternatives for Kitcher's project of providing a framework for understanding the social and political constraints on science, or providing an ideal of "well-ordered science" in a liberal democracy, would require an additional essay, perhaps a whole book. I will point to some fairly obvious consequences, which seem to me also to improve upon Kitcher's account.

First, I hope it is clear that fully comprehending the significance of some part of science is going to require much more intimate knowledge about the situational context of that part than is available in Kitcher's significance graphs. The "importance of practice" in my replacement framework for analyzing significance captures much of what Kitcher is after with his idea of well-ordered science. Unlike Kitcher's account (but like the view that Simon (2006) thinks Kitcher is necessarily but unfortunately committed to), social values already come in at the point of assessing significance. Also, assessing the significance of

particular research requires an understanding of the larger practice. This makes carrying out Kitcher's project much harder than it would be on his original account. Kitcher's significance graphs are suppose to make key information for assessing significance accessible to lay deliberators; democratic assessment of science would thus be possible. Unfortunately for Kitcher, this can't be done in the schematic way that he hopes. Kitcher wants a short-cut solution to a hard problem, but such attempts in philosophy, while common, tend to create more troubles than they the resolve. There are rarely, if ever, such easy ways out. I've told a story about how to get "significance" right, necessary for engaging with the policy, and engage we must! No simple diagrams or idealized democratic deliberation-processess will set the research agenda for science. A more complex process will be necessary.

Of course, I've provided equally or more simplistic representations of my own, but my graph is not meant to be a full story. I'm not suggesting that the added component of significance can be captured by a two-dimensional vector space and assigned Cartesian coordinates. Rather, I think that in order to be able to understand concretely a claim such as Figure 2 represents, you need to know about the standing practice in question and the way in which the problem disrupts that practice, as well as having some sort of sympathetic connection to the practitioners in question and the way in which they experience that disruption to be confusing, troubling, etc. This means that, insofar as information about significance is supposed to "tutor" the preferences of our ideal deliberators, that process will have a significantly more human face. In fact, I think the whole discussion needs to sound less like an ideal Rawlsian fairytale and more like an actual human discussion.

Second, I think that the demarcation between significance, which comes from the scientists' side, and the image of well-ordered science, produced on the basis of ideal representatives of the interests of layperson groups, becomes untenable. Assessing the significance of a particular part of science will depend on the significance of the practice of which it is a part. To oversimplify, the problems that arise in physics depend on the significance of the ongoing tradition of work going on over there in the physics department. However that gets cashed out is going to depend in part on complex relations of science as a practice to the rest of human life and affairs, and a necessary part of that story is going to be social, ethical, and political values. The way in which significance "informs" debates about science is going to be a more iterative, more reciprocal process. Science and social policy cannot be set apart and interact with each other in a thin way. We need to understand more closely the relationship between scientific practice and *social* problems, a project that Dewey called for long ago.[26]

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## NOTES

1. See Richardson (2002, 2003); Howard (2003, 2007); Reisch (2005); Douglas (2009)

2. Or, to be more precise, since Kitcher's procedure doesn't produce an *actual* research agenda, but instead points to the *kind* of procedure that would produce one, it would be more accurate to say that what Kitcher provides is a ground for arguments about what would or wouldn't be on the agenda. The judgments one would be able to make would necessarily be fairly coarse (e.g., pursue research on third-world disease rather than more advanced liposuction techniques).

3. According to standard practice, references to John Dewey's work are parenthetical citations to Dewey (1991), cited according to sub-collection: *The Early Works* (EW), *The Middle Works* (MW), and *The Later Works* (LW). Citations are made with these designations followed by volume and page number, along with essay or manuscript title where this is not clear from context.

4. Kitcher does not cite Rouse, but their way of putting the problem is uncannily similar. A likely common source is Popper (though only Kitcher cites him as a source of his ideas on significance, in the bibliographic notes to chapter 6). Rouse seems more keenly aware of the specific issues of scientific practice as such.

5. Kitcher's discussion of the "Unity-of-Science movement" may depart drastically from the actual historical movement headed by Otto Neurath. See Reisch (2005) and Cartwright et al. (1996).

6. One may find fault with Kitcher's characterization of the Unity-of-Science view, or find his criticisms lacking. There are certainly many careful and sustained critiques of the idea. Kitcher refers the reader to Fodor (1974); van Fraassen (1980, 1989); Kitcher (1984); Dupré (1993); Cartwright (1999). Dewey's own critique and re-interpretation of the Unity of Science (as an anti-reductionist opposition to supernaturalism) can be found in his contribution to Volume 1 of the *International Encyclopedia of Unified Science*, "Unity of Science as a Social Problem" (LW 13:271–280). Paul Feyerabend was widely critical of such a view, arguing for the necessity of metaphysically and conceptually incommensurable theories and an antagonistic theory of scientific progress along the lines of Mill's *On Liberty*. See also Galison and Stump (1996). Despite the difficulties of the issue, I suspect a quite simple argument will serve Kitcher's purpose. We ought to believe that science *actually* provisions explanations, and that we are currently able to make reasonable judgments about significance. However, science at present does not form the unified edifice dreamed of by the Unity-of-Science movement, nor does it even approximate it. Therefore, any explanation of the significance of science applicable to science *as it actually exists at present* cannot depend on this far-off ideal of the Unity of Science.

7. This is one way to understand much recent work on "models" and "mechanisms" which treat those as general schemes of scientific explanation.

8. Following Kuhn (1970, p. 271).

9. P. D. Magnus has raised a variety of problems for Kitcher's use of "natural curiosity" in an unpublished manuscript (Magnus 2008), which are related but independent.

10. Even idealized ones!

11. Quoted in Feyerabend and Terpstra (1999, p. 148) and Feyerabend (1988, p. 35).

12. Further problems arise when one attempts to use Kitcher's analysis of significance for Kitcher's own project of reconciling science and democracy. See Simon (2006).

13. The *locus classicus* being his 1877 series of essays in *Popular Science Monthly*, especially "The Fixation of Belief" (Peirce, 1877).

14. The lack of a good reason for doubt is crucial to the idea of a paper doubt. Part of what Peirce and Dewey are after is an explanation of what counts as a good reason for doubt.

15. "The Fixation of Belief" (Peirce, 1877). Citations of Peirce will refer to Peirce et al. (1992) according to (EP *volume*:*page*), and citations in this paper refer to Peirce (1877) unless otherwise noted.

16. Belief here should be understand in its dispositional, not occurrent sense.

17. In Dewey's usage, a situation is not a mental state, subjective experience, or collection of sense-data, nor is it merely a spatio-temporal region or the physical surfaces of apparent objects. It is not singular object or set of objects, but a contextual whole. A situation for Dewey is an "environing experienced world" (*Logic*, LW 12:72–73). It is essentially subject-centered or subject-relative, without being subjective. It is a context or a perspective, which must be understood *relative to a subject*, but which is not *subjective* in a problematic way, though unlike "context," which sometimes notes the latter side of the figure-ground relationship, situation encompasses both. See Giere's *Scientific Perspectivism* for an argument about why we should not be worried by science being perspective-dependent.

18. See Koschmann et al. (1998).

19. And such failures aren't a familiar occurrence for which there is another immediate response.

20. This is especially developed in Dewey's essay, "Qualitative Thought" (LW 5: 243–262).

21. Both Peirce and Dewey think it is a characteristic of the scientific attitude to seek out problems, not merely passively wait for them to occur. See Bernstein (1966, p. 105) and Browning (1994).

22. See, for example, Dewey's discussion of "curiosity" in this sense in *How We Think* (LW 8:141–144).

23. While it is doubtful that any inquiry is possible that is *completely* disconnected from prior practical and scientific investigations, and it seems unlikely that any significant area of research could arise without many such connections, there do seem to be several candidates for areas of inquiry whose significance far *surpasses* the relatively thinner connections to prior questions, problems, results, etc. of earlier science, as well as practical application and natural curiosity: Darwinian evolutionary theory, cellular automata theory, chaos theory, computer science, mathematical logic, and climatology, especially in the earlier days of those sciences, are quite novel in terms of

their problems, subject-matter, and methods. Unlike Kitcher's favorite examples, it seems difficult to explain the intense level of interest in these areas at the time of their inception in terms of significance graphs; the significance of these cases appears to precede their dense connectedness on Kitcher's graphs.

24. Along the lines of Luria (1984), discussed above in §4.

25. My thanks to Nancy Cartwright for suggesting this apt phrase for describing my view.

26. See, e.g., the penultimate chapter of *Logic: The Theory of Inquiry* on "Social Inquiry" (LW 12:481–505) or "Unity of Science as a Social Problem" (LW 13:271–280)

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Matthew J. Brown
Assistant Professor of Philosophy
Center for Values in Medicine, Science, and Technology
University of Texas at Dallas
800 W Campbell Road, JO31
Richardson, TX 75080
United States