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How Values Can Be Good for Science

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The Ideal of Value Freedom

Values are good for science — the values of truth, objectivity, accuracy, and honesty in results are integral to most notions of good science. But these are not the values causing concern. The ones being questioned are those that might interfere with the realization of the "good" values: social values and to some extent pragmatic values, ideas about social relations or about social utility that may, without vigilance, be expressed in scientific reasoning or representations of the natural world. While much argument about the role of these sorts of values presupposes that they should not play a role in science, certain epistemological analyses of scientific judgment challenge this assumption. Like many thinkers, I want to urge that philosophy of science include attention to the roles that values, interests, and relationships in the social and cultural context of science play in scientific judgment as well as attention to the impacts of science and science-based technologies on society. I am of the view, however, that in principle there is no way of guaranteeing the eliminability of such values from science. We should stop asking whether social values play a role in science and instead ask which values and whose values play a role and how. But here I want to show not so much how as why values can be good for science.

The recent so-called science wars made this task more difficult by exaggerating the differences between approaches to the sciences that

are social and cultural and those that are philosophical. Social and cultural approaches insist on the embeddedness of scientific inquiry in its social contexts and the impossibility of understanding the direction and outcomes of scientific investigation without taking those contexts into account. They argued on the basis of empirical studies of the progress of work in laboratories and research programs that no principled distinction could be made between cognitive and noncognitive elements among the causal factors in scientific judgment (see Bloor 1991; Knorr-Cetina 1983; Latour 1987). Many philosophers of science found themselves in the position of defending the rationality of science against its perceived detractors (see Laudan 1984; Goldman 1995; Kitcher 1993). And so it went on: reason rules versus unreason rules.

Much philosophical discussion about the relationship of science to its social contexts is pursued under the rubric of values in science. This functions as a kind of catch-all for the messy and complex world of social relationships as it might bear on the practice of science. Treating the social dimensions of science as a set of questions about values in science has distracted philosophers from investigating the social dimensions of science. But there is a reason why philosophers worry about claims that seem to undermine the value-neutrality of science. It is worth pausing, therefore, to note why value-freedom has been thought to be an ideal of and for the sciences.

Being free of values is a virtue for science because we want our acceptance of theories to be impartial and not a matter of wishful thinking. In a culture where so much rests on the sciences we fear that certain kinds of values will lead to acceptance of representations of the natural and social worlds in theories, hypotheses, and models that favor the interests of certain members of or groups in society over those of others. The ideal of value freedom is also bound up with the ideal of universality: what counts as a scientific truth or scientifically supported claim for one person or community should count as such for any other, no matter how different their cultural values.

The natural sciences were thought to exemplify the ideal of value freedom because they prescribed or were thought to prescribe methods of hypothesis and theory testing that guaranteed reliance on logic and observation alone, that is, on universal capacities that could be exercised in a content-neutral way. Scientific inquiry pursued rigorously could lead us to accept representations of natural (and perhaps social)

phenomena and processes that were free of the taint of metaphysics as well as social biases such as the racism and sexism that infected much of nineteenth-century biology and anthropology.

Equating value freedom with methodological rigor cuts in several ways. Taking value freedom as an ideal led some of us, feminists, anti-racists, socialists, to question whether certain scientific research programs were actually value free. Science should be value free, but it is not. Greater vigilance about biases will correct this defect (see Hubbard 1979; Gould 1986). But the value-free ideal has another face: if impartially pursued, value-blind, scientific inquiry produces results that do end up favoring certain groups in society, or that when applied have certain consequences, we must accept those outcomes if they are the result of impartial methods impartially applied. One can see this consequence articulated in the response of advocates of research programs criticized for sexism (Witelson 1985). If science tells us that women are biologically less well equipped than men to do math, well, that's unfortunate, but so be it. This kind of attitude is among the factors that stimulated feminist philosophers to investigate the grounds for claiming that science at its best or in its nature is value free.[1]

Rationality, Sociality, Plurality

The field opened up by the feminist interventions and extended by social studies of science has become crowded and in recent years an unbridgeable rift grew between those who maintain the value freedom of science and those who reject it even as an ideal. Each side of the rift emphasizes its preferred analytic tools to the exclusion of those of the other. As a consequence, accounts intended to explicate the normative dimensions of epistemological concepts, that is, elaborating the relationship of knowledge to such concepts as truth and falsity, opinion, reason, and justification, have been too idealized to gain purchase in actual science, whereas accounts detailing actual episodes of scientific inquiry suggested that either our ordinary normative concepts have no relevance to science or that science fails the tests of good epistemic practice. This cannot be right. The stalemate between the two sides is produced by both sides' accepting a dichotomous understanding of the cognitive and the social.

According to the dichotomous understanding of these notions, if an epistemic practice is cognitively rational, then it cannot be social. Con-

versely, if an epistemic practice is social, then it cannot be cognitively rational.[2] What further is meant by "rational" or "cognitive," on the one hand, and by "social," on the other, varies from scholar to scholar.[3] The dichotomy between them, however, structures the thinking of a number of writers on scientific knowledge. Elsewhere I tease apart the dichotomy's components and offer an account of epistemological concepts that integrates the rational and the social (Longino 2002a). Among the components of the dichotomy are two contrasting assumptions, monism and nonmonism, about the content of scientific knowledge. I understand monism as follows:

For any natural process there is one (and only one) correct account (model, theory) of the process. All correct accounts of natural processes can form part of a single consistent and comprehensive account of the natural world.

Nonmonism is often treated as antirealism of some kind, but there can be eliminativist, constructivist, and realist versions of nonmonism. This means that there can be two forms of realism: a monist realism, which holds that there is or will be one correct and comprehensive account of the natural world, and pluralist realism, which I understand as follows:

For any natural process, there can be more than one correct account (model, theory) of the process. This is especially likely in the case of complex processes. It is not necessary that all correct accounts of natural processes form part of a single consistent account of the natural world. Rather than one complete account, multiple approaches may yield partial and nonreconcilable accounts.

Philosophers of science who advocate pluralism disagree about the grounds for the view and about the precise nature of the pluralist claim. (For different articulations, see Dupre 1993; Ereshevsky 1998; Mitchell 2002; Rosenberg 1994; Waters 1991.) Those advocating strong forms of pluralism are claiming that the complexity of natural processes eludes complete representation by any single theoretical or investigative approach available to human cognizers.[4] Any given approach will be partial; and completeness, if achieved at all, will be achieved not by a single integrated theory, but by a plurality of approaches that are partially overlapping, partially autonomous, and that resist unification. For example, organismic development can be investigated in different ways that preclude alternative understandings. Insight into the genetic contributions to development is achieved by holding environmental conditions constant. But then one gets no un-

derstanding of environmental or other nongenetic factors in development. And vice versa.

Many philosophical accounts of scientific knowledge are incompatible with such pluralism. They assume as a condition of adequacy of criteria of knowledge that there is one uniquely correct account of the phenomenon to be known. Conversely, a standard criticism of pluralism is that it makes knowledge impossible. I contend that accounts of knowledge should not presuppose either monism or pluralism. Whether the world is such as to be describable by one model or many is not a priori decidable. Therefore, one of the constraints on the analysis of knowledge ought to be that neither metaphysical position is presupposed. What would such an account look like?

Knowledge as Social

I propose the social account of knowledge as one way to satisfy the constraint. To see how it does so it is useful to start with the central problem to which that account is addressed: the underdetermination problem. At the heart of philosophical reflection about scientific knowledge is the gap between what is presented to us, whether in the kitchen and garden or in the laboratory, and the processes that we suppose produce the world as we experience it, between our data and the theories, models, and hypotheses developed to explain the data. As long as the content of theoretical statements is not represented as generalizations of data or the content of observational statements is not identified with theoretical claims, then there is a gap between hypotheses and data, and the choice of hypothesis is not fully determined by the data. Nor do hypotheses specify the data that will confirm them. Data alone are consistent with different and conflicting hypotheses and require supplementation.

Philosophers have had a variety of ways of describing and responding to this situation. Pierre Duhem (1954), the first philosopher of science to raise the underdetermination problem (as different from the problem of induction), emphasized assumptions about instruments, for example, that a microscope has a given power of resolution, or that a telescope is transmitting light from the heavens and not producing images internally or not systematically distorting the light it receives. But the content of background assumptions also includes substantive (empirical or metaphysical) claims that link the events observed as data

with postulated processes and structures. For example, that two kinds of event are systematically correlated is evidence that they have a common cause or that one causes the other in light of some highly general, even metaphysical, assumptions about causality. The correlation of one particular kind of event, such as exposure to or secretion of a particular hormone, with another, such as a physiological or behavioral phenomenon, is evidence that the hormone *causes* the phenomenon in light of an assumption that hormone secretions have a causal or regulative status in the processes in which they are found, rather than being epiphenomenal to or effects of those processes. Such an assumption has both empirical and metaphysical dimensions. Assumptions of this kind establish the evidential relevance of data to hypotheses. Among other things, they provide a model of the domain being investigated that permits particular investigations to proceed.

Some philosophers discuss underdetermination as a problem of the existence of empirically equivalent but inconsistent theories. The underdetermination under consideration here concerns the semantic gap between hypotheses and data that precludes the establishing of formal relations of derivability without employing additional assumptions. In this picture, different explanatory hypotheses may not have exactly the same empirical consequences, but instead may have some overlapping and some nonoverlapping consequences. If they are empirically adequate to the same degree, then empirical evidence alone cannot serve as grounds for choosing between them. Particle and wave theories of light stand in such a relationship to each other, but there are many other examples as well. The additional (background) assumptions required to establish a connection between hypotheses and data (reports), then, include substantive and methodological hypotheses that, from one point of view, form the framework within which inquiry is pursued, and from another, structure the domain about which inquiry is pursued. These hypotheses are most often not articulated but presumed by the scientists relying on them. They facilitate the reasoning between what is known and what is hypothesized.

I take the general lesson of underdetermination to be that any empirical reasoning takes place against a background of assumptions that are neither self-evident nor logically true.[5] Such assumptions, or auxiliary hypotheses, are the vehicles by which social values can enter into scientific judgment. If, in principle, there is no way to mechanically eliminate background assumptions, then there is no way to mechan-

ically eliminate social values and interests from such judgment. Some sociologists of science used versions of the underdetermination problem to argue that epistemological concerns with truth and good reasons are irrelevant to the understanding of scientific inquiry and judgment (Barnes and Bloor 1982; Pickering 1984; Shapin 1994; Collins and Pinch 1993; Knorr-Cetina 1983; Latour 1987, 1993). The point, however, should not be that observation and logic as classically understood are irrelevant, but that they are insufficient. The sociologists' empirical investigations show that they are explanatorily insufficient. The philosophers' underdetermination argument shows that they are epistemically insufficient.

My view is that rather than spelling doom for the epistemological concerns of the philosopher, the logical problem of underdetermination, taken together with the sociologists' studies of laboratory and research practices, changes the ground on which philosophical concerns operate. This new ground or problem situation is constituted by treating agents/subjects of knowledge as located in particular and complex interrelationships and by acknowledging that purely logical constraints cannot compel them to accept a particular theory. That network of relationships — with other individuals, social systems, natural objects, and natural processes — is not an obstacle to knowledge but a rich pool of resources — constraints and incentives — to help close the gap left by logic. The philosophical concern with justification is not irrelevant, but it must be somewhat reconfigured to be made relevant to scientific inquiry. The reconfiguration I advocate involves treating justification not just as a matter of relations between sentences, statements, or the beliefs and perceptions of an individual, but as a matter of relationships within and between communities of inquirers.

This expansion of justification sees it as consisting not just in testing hypotheses against data, but also in subjecting hypotheses, data, reasoning, and background assumptions to criticism from a variety of perspectives. Establishing what the data are, what counts as acceptable reasoning, which assumptions are legitimate, and which are not become in this view a matter of social, discursive interactions as much as of interaction with the material world. Since assumptions are, by their nature, usually not explicit but taken-for-granted ways of thinking, the function of critical interaction is to make them visible, as well as to examine their metaphysical, empirical, and normative implications.

The point is not that sociality provides guarantees of the sort that

formal connections were thought to provide in older conceptions of confirmation, but that cognitive practices have social dimensions. Acknowledging this social dimension has two consequences. In the first place, any normative rules or conditions for scientific inquiry must include conditions applying to social interactions in addition to conditions applying to observation and reasoning. A full account of justification or objectivity must spell out conditions that a community must meet for its discursive interactions to constitute effective criticism. I have proposed that establishing or designating appropriate venues for criticism, uptake of criticism (that is, response and change), public standards that regulate discursive interaction, and what I now call tempered equality of intellectual authority are conditions that make effective or transformative criticism possible (Longino 2002a, 128–35). The public standards include aims and goals of research, background assumptions, methodological stipulations, and ethical guidelines. Such standards regulate critical interaction in the sense of its serving to delimit what will count as legitimate criticism. Thus, these standards are invoked in different forms of critical discussion, but most importantly they are themselves subject to critical scrutiny. Their status as regulative principles in some community depends on their continuing to serve the cognitive aims of that community. The conditions of transformative criticism may not be the conditions ultimately settled on, but I contend that something like them must be added to the set of methodological norms.

Second, even though a community may operate with effective structures that block the spread of idiosyncratic assumptions, those assumptions that are shared by all members of a community will not only be shielded from criticism but also, because they persist in the face of effective structures, may even be reinforced. One obvious solution is to require interaction across communities, or at least to require openness to criticism both from within and from outside the community. Here, of course, availability is a strong constraint. Other communities that might be able to demonstrate the non-self-evidence of shared assumptions or to provide new critical perspectives may be too distant, spatially or temporally, for contact. Background assumptions then are only provisionally legitimated; no matter how thorough their scrutiny given the critical resources available at any given time, it is possible that scrutiny at a later time will prompt reassessment and rejection. Such reassessment may be the consequence not only of interaction with

new communities but also of changes in standards within a community. These observations suggest a distinction between a narrow and a broad sense of justification. Justification in a narrow sense would consist in survival of critical scrutiny relative to all perspectives available within the community, whereas in a broad or inclusive sense justification would consist in survival of critical scrutiny relative to all perspectives inside and outside the community.[6]

Clearly, that a theory is acceptable in C at t, in the narrow sense of justification, does not imply that it will be acceptable to C at t_2, or that it must be acceptable to any other community. Furthermore, there is no requirement that members of C reject background assumptions simply because they are shown to be contingent or lacking firm support. Unless background assumptions are shown to be in conflict with agreed on data or with values, goals, or other assumptions of C, there is no obligation to abandon them — only to acknowledge their contingency and thus to withdraw excessive confidence. Background assumptions are, along with values and aims of inquiry, the public standards that regulate the discursive and material interactions of a community. The point here is that they are both provisional and subordinated to the overall goal of inquiry for a community. Truth simpliciter cannot be such a goal, since it is not sufficient to direct inquiry. Rather, communities seek particular kinds of truths. (They seek representations, explanations, technological recipes, and so on. Researchers in biological communities seek truths about the development of individual organisms, about the history of lineages, about the physiological functioning of organisms, about the mechanics of parts of organisms, about molecular interactions. Research in other areas is similarly organized around specific questions.) Which kinds of truths are sought in any particular research project is determined by the kinds of questions researchers are asking and the purposes for which they ask them, that is, the uses to which the answers will be put. Truth is not opposed to social values, indeed, it *is* a social value, but its regulatory function is directed/mediated by other social values operative in the research context.

The possibility of pluralism is a consequence of the possibility of alternative epistemological frameworks consisting of rules of data collection (including standards of relevance and precision), inference principles, and epistemic or cognitive values. Other philosophers have advanced pluralism as a view about the world, that is, as the consequence

of a natural complexity so deep that no single theory or model can fully capture all the causal interactions involved in any given process. While this may be the case, the epistemological position I am advocating is merely open to pluralism in that it does not presuppose monism. It can be appropriate to speak of knowledge even when there are ways of knowing a phenomenon that cannot be simultaneously embraced. Whether or not it is appropriate in any given case depends on satisfaction of the social conditions of knowledge mentioned earlier. When these are satisfied, reliance on any particular set of assumptions must be defended in relation to the cognitive aims of the research. These are not just a matter of the individual motivations of the researchers but of the goals and interests of the communities that support and sustain the research. On the social view all of these must be publicly sustained through survival of critical scrutiny. Thus, social values come to play an ineliminable role in certain contexts of scientific judgment.

Values in Science, Again

I maintain that this is an account of scientific knowledge and inquiry (or the basics of one) that both integrates the rational and the social and avoids begging the question for or against pluralism. As to the first, the philosopher is right to see the sciences as a locus of cognitive rationality; the sociologist or sociologically sensitive historian is right to see the sciences as a locus of social interactions (that are not containable within the lab or research site). The mistake is to accept a conceptual framework within which these perspectives exclude each other. With respect to the second, we can talk about knowledge of a phenomenon X made possible by one set of methodological commitments and standards guided by a particular question and also about different knowledge of the same phenomenon made possible by a different set. As long as two (or more) incompatible models of X are working in the ways we want (are narrowly or even broadly justified), why not accept that they are latching on to real causal processes in the world, even if these cannot be reconciled into one account or model? Only a prior commitment to monism precludes this, but whether we end up at that mythical end of inquiry with one true account for each domain or more than one is a matter of how the world is and is neither presupposed nor settled by epistemological reflection.

I now draw some lessons concerning the relationship of science and

values. The possibility of pluralism that is part of this account has implications for the ideals both of universality and of impartiality. Universality does not make sense as an ideal except in a very restricted way — results hold for those sharing an investigative framework, cognitive aims, and the values in relation to which a given cognitive aim makes sense. What might be genuinely universal is the judgment that within a framework organized by a particular cognitive goal a given result holds, but this, of course, leaves room for a different result emanating from inquiry differently organized. What about impartiality? One of the aims of many philosophers of science has been, as I mentioned at the beginning, to show how, in spite of the de facto presence of social (and personal) values and interests, scientific inquiry can nevertheless be cleansed of them. The very possibility of pluralism turns the value-free ideal upside down — values and interests must be addressed not by elimination or purification strategies, but by more and different values. To see this, consider the following.

First, suppose pluralism is right, that is, the world is not such as to be in the end describable by one theory or conjunction of theories. Then, even if a given theory has impeccable evidentiary support, is justified in the narrow sense, that it has problematic or noxious social consequences (that is, its acceptance would advance or undermine the interests of one or more groups in society relative to others) is reason not directly to reject it but instead to develop an alternative approach that has equivalent empirical validity. (This is not an armchair pursuit; it takes time, effort, and resources.) The social payoff is an escape route from natural inevitability arguments. The epistemic payoff is an increase in the range of phenomena that we can know or explain. This multiplicationist strategy is constrained, but not foreclosed, by requirements of empirical adequacy. It does not encourage one thousand flowers to bloom, but only two or three may be needed.

So, even if the arguments attributing racial differences in IQ tests were impeccable by the standards of behavior genetics, a commitment to equality or to one's race or sex is reason to explore an alternative explanation, such as Claude Steele's theory of exacerbated performance anxiety or some other. Similarly, feminists' objections to gene-centric or master molecule accounts of biological processes are expressed not just by rejecting them as determinist or reductionist but also by developing alternative accounts. Pluralism affirms the partiality, that is, incompleteness, and not the falsity of gene-centric accounts.

Suppose, on the other hand, that monism is right, that the world *is* describable by one theory or conjunction of uniquely domain-specific theories. Even if this is so, there is no reason to believe it unless those theories that belong in the set have been tested against all possible alternatives, so that a theory's having noxious consequences is again good reason for one with different values to develop an alternative approach. This will increase the alternatives in play and increase the likelihood that eventually, in the long term, we will exhaust all possible alternatives and settle on the conjunction of uniquely correct domain-specific theories.

Feminist interventions in physical anthropology and primate ethology since the 1970s constitute a recent classic example of value-driven research that has improved quality of science in those areas. Feminists have brought new phenomena and data to the attention of their disciplines and have drawn new and different connections between phenomena that were already known to their communities.

The standoff among different research approaches in bio-behavioral sciences offers another example. I have been studying contrasting and competing approaches to the study of human behavior. In particular, I have been looking at differences (and similarities) among behavior genetics approaches and approaches that emphasize aspects of the social environment as explanatory of behavioral profiles or dispositions (see Longino 2001, 2003, and forthcoming). Classical behavior geneticists look for and develop methods for identifying and interpreting intergenerational behavioral correlations ("concordances"), and molecular behavior geneticists look for and develop methods for identifying correlations between genetic structures and behaviors. Social environmental approaches, including family systems and developmental systems approaches, look for and develop methods for identifying environmental and social determinants of behavioral differences. Members of each side characterize the other as politically and ideologically motivated. The social environmentalists accuse behavior geneticists of being socially insensitive, rigidly reductionistic, and giving support to racism, sexism, and social policies that perpetuate racial and gender injustice. Behavior geneticists accuse social-environmentalists of being fuzzy-headed liberals who want to engage in dangerous social engineering. This mutual caricature under- and probably misstates the values involved. In addition to whatever political values are involved, the research is driven by divergent professional interests both within

the research communities and in the clientele they serve, by aesthetic values, and by social values and overall conceptions of human nature.

Close examination shows that these approaches are in a narrow way incommensurable. Each parses the space of possible causes differently, so it is not meaningful to compare how well they fit the data. The data and the contexts in which they emerge as data are different from approach to approach. But each program is capable of revealing empirical regularities that the other cannot. The different values that partially sustain each approach ensure their persistence. The consequent plurality of nonreconcilable accounts of the behaviors studied enhances our scientific understanding rather than diminishing it. Human behavior may be so complex that no single research approach can provide complete understanding. Divergent values prevent foreclosure and drive an expansion of knowledge and understanding rather than narrowing them. Persuasive arguments for plurality may also lead policy makers to turn to science for narrowly conceived purposes, but not for general accounts of human nature that might guide social policy in any global fashion.

These examples concern research on behavior where the multiplicative strategy may be socially and pragmatically appropriate. Other areas of research may require different ways of handling the possibility of pluralism. For example, estimates of the strength or degradability of materials used in nuclear waste storage facilities cannot be left in a state of uncertainty. Conversely, however, the consequences of acting prematurely without considering the variety of frameworks within which estimates might be generated could be catastrophic. Scientific advisory panels and granting agencies cannot proceed as though there will be just one correct account of a phenomenon under investigation, thus taking a string of empirical successes as proof of that correctness. They must instead incorporate the possibility of plurality into their decision-making procedures.[7] How this should be done is a topic too broad to address here. Richard Rudner's analysis (1953) would not be a bad place to begin one's inquiry.

Conclusion

The ideal of value freedom was advanced because it was thought that value-free science could best ensure impartial (unbiased, socially neutral) science and universally valid science, that is, results that would

hold for anyone, anywhere. This has led individual investigators to suppose that they must keep their own values out of the laboratory and that doing so would be sufficient to guarantee value-free, impartial science. I contend that the conception of inquiry this thought is based on is untenable and furthermore that the values held by the entire community will not be checked by vigilance for the idiosyncratic. The alternative, social, account of knowledge indicates that the objectives of the value-free ideal are better achieved if the constructive role of values is appreciated and the community structured to permit their critical examination. Structuring the community to include multiple perspectives and values will do more to advance the aims in relation to which value-free science was an ideal — impartiality and universality — than appeals to narrow methodology ever could.

NOTES

1. Of course, one might take an alternative view and argue that what the sciences proclaim about human differences should have no bearing on social policy, that such policy ought to be determined by our political goals and values and not be composed of transient empirical theories. I agree that there is a good argument to be made for this conclusion, but I do not think this precludes an investigation into the grounds for the claims of scientific value freedom.

2. I use *cognitively rational* and *cognitive rationality* to distinguish the kind of rationality in question here from pragmatic rationality, which is not understood as excluding the social in the same way.

3. One factor contributing to the confusion is ambiguity of the word *social*. It is used to refer to human relations, activities, and interactions; to the content of both normative and descriptive propositions; and to the shared character of some content.

4. My formulation here deliberately equivocates between an ontological and an epistemological articulation.

5. This is to say, not that scientists face a gap over which they leap with careless abandon, but that the ways in which the gap between hypotheses and data is closed involves reliance on assumptions that are contestable.

6. Using this social account of justification one might then say that some content *A* (a theory, model, hypothesis, observation report) is epistemically acceptable in community *C* at time *t* if *A* is supported by data *d* evident to *C* at *t* in light of reasoning and background assumptions that have survived critical scrutiny from as many perspectives as are available to *C* at *t*, and the discursive structures of *C* satisfy the conditions for effective criticism. In Longino (2002a, 135–40), I use this notion of epistemic acceptability to provide accounts of epistemological concepts.

7. For debate on this matter, see the exchange between Philip Kitcher and myself (Kitcher 2002a,b; Longino 2002b,c).

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