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On the Interplay of the Cognitive and the Social in Scientific Practices

Hugh Lacey[†]

I consider the following questions, central to recent disagreements between Longino and Kitcher: Is it *constitutive* of making judgments of the cognitive acceptability of theories that they are made under certain social relations that embody specific social values that have been cultivated among investigators (Longino)? Or is making them (sound ones) just a *consequence* of social interactions that occur under these relations (Kitcher)? While generally endorsing the latter view, I make a distinction, not made by the philosophers under discussion, between sound acceptance and endorsement of a theory, and argue that Longino's view applies to endorsement.

1. Introduction. It is uncontroversial that scientific practices are responsive to both cognitive (epistemic) and social/moral interests. Nevertheless spokespersons of modern science often insist that knowledge gained from these practices and the theories in which it is expressed do (should) not bear the indelible mark of values. The 'core' of science, they say, is unsullied by values: Theories are appraised for the strength of the evidence supporting them and values have no role in justifying properly made methodological decisions. Of course cognitive values, or the criteria for appraising scientific theories, are indispensable; but they are distinguished from social, moral and other kinds of values, and the criteria for identifying them do not depend on non-cognitive value commitments. It is non-cognitive values that are denied a role in theoretical appraisal and fundamental methodological decisions.

Scientific practices, so interpreted, do contain moments at which values may have legitimate roles, most notably when scientific knowledge is applied technologically and choices are made about specific phenomena to investigate and experimental methods to use. The core of science, however, is said (for well known reasons) to remain unsullied by values involved in decisions made at these moments. It should also remain unsullied by

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the personal interests of scientists and by the fact that scientific practices cannot be conducted without the provision of adequate material, economic and institutional conditions and, hence, without the largesse of non-scientific institutions (government agencies, corporations, etc), which typically embody social values that are not subordinated to the value of gaining scientific understanding. Social and personal values may legitimately affect not only decisions made at the 'non-core' moments, but also the dynamic and texture of scientific practices: when, where, by whom they are conducted, how rapidly and extensively they unfold, and whether or not they are embraced or restricted in specific societies. They may also put pressure on judgments made at the core moments. The spokespersons of the tradition readily acknowledge that such pressure is sometimes successfully exerted, for scientists may succumb to threats from power and enticements to conformity or the seductions of personal gain. When it is, they say that scientific understanding has been 'distorted'. Then they add that standard methodological procedures are sensitive to the sources of distortion and provide built-in resistance to it (Lacey 2005, Chapter 2).

According to this view scientific practices have a social dimension and they are affected by the play of values. The connections are external, however; values are not dialectical partners of scientific practices, since at their core moments values should not, and in exemplary instances do not, interact in any indispensable way with the cognitive values. Historical and social investigation is thus illuminating and appropriate for many aspects of scientific phenomenon, but it cannot settle the adequacy of theoretical appraisals and the soundness of methodological decisions.

In order to avoid (non-cognitive) values intruding into the moments where they are held to be inadmissible it is important to distinguish between the cognitive (rational) support of a theory (as providing understanding of a specified domain of phenomena) and the conditions under which it gained that support—thence between the questions "On what grounds is a theory (*T*) accepted?" and "What explains that *T* became a candidate for acceptance and that the conditions for exploring it were made available?" Similarly, Susan Haack distinguishes: "How good is the evidence for a theory?" and "What are the standards for the conduct of scientific inquiry?" (Haack 1997). Social investigation is appropriate for the latter questions but, with respect to the former, it is only thought significant when non-cognitive values *de facto* are among the grounds being appealed to (explicitly or implicitly); then, it may explain how the consequent distortions of scientific knowledge came about. This also means, as Haack has emphasized, that—contingently—certain social conditions may need to be in place in order that properly warranted theories can be accepted. The acceptability of *T* is not a matter for social explanation, only its actual acceptance is. It is only when *T* has been improperly

accepted that the important question becomes: Why (causally) was *T* accepted? Social explanation of the actual acceptance of *T* cannot displace its cognitive appraisal, and a social explanation of its cognitive appraisal tells us nothing decisive about the adequacy of the appraisal.

2. Cognitive and Social Dimensions of Science. The view just summarized, that social (non-cognitive) values have no proper role at the core moments of scientific practices, those that concern theory appraisal and methodological decisions, has been challenged or reconsidered in various ways, most recently by Helen E. Longino and Philip Kitcher (Longino 2002a; Kitcher 2001). Although they concur that the core moments inevitably involve both cognitive (epistemic, rational) and social (non-cognitive) aspects they disagree about how the cognitive and the social interact at these moments (Longino 2002b, 2002c; Kitcher 2002a, 2002b). Their disagreements concern: (1) the relevance of social factors to the cognitive acceptability of theories, (2) the character of the pluralism that (both of them say) should mark scientific practices and their methodologies, and (3) the role of democratic decision-making in them.

In this article, I will address only the first point of disagreement. Is it *constitutive* of making judgments of the cognitive acceptability of theories that they are made under certain social relations (that embody specific social values) that have been cultivated among investigators (Longino)? Or is making them (sound ones) just a *consequence* of social interactions that occur under these relations (Kitcher)?

3. Theory Acceptance: A Consequence of Social Interactions. My remarks draw upon a third position (Lacey 1999, 2005) that (I believe) enables us to put into sharp relief the strengths and weaknesses of Longino's and Kitcher's positions. I distinguish between the moments of *adoption of strategy* and *theory appraisal/knowledge confirmation*. These are the core moments of scientific practices. A third moment, *application of scientific knowledge*, will be important later in the argument. Adopting a strategy involves identifying the kinds of possibilities that are to be explored, and consequently making methodological decisions about such matters as what constraints are to be put on theories that may be investigated (including what categories are to be deployed in them) and what kinds of empirical data are to be sought out and recorded.

Whereas Longino rejects (what she calls) a *dichotomy* between the cognitive and the social, I maintain that there is an *important distinction* between *cognitive and social* (and other non-cognitive) *values*, even though there is not a *separation* between them, for cognitive values are manifested (in theories) only in contexts where social values are also manifested (in scientific practices and institutions). This distinction is indispensable for

understanding the place of social values at the core moments (Lacey 2004). Cognitive values—which include such items as empirical adequacy, explanatory power, and power to anticipate possibilities open to the phenomena within a theory’s compass (Lacey 1999, Chapter 3)—are the desiderata of theories that are required in view of the aim of science, which I take to be: To generate and consolidate theories that express empirically-grounded and well-confirmed understanding of phenomena (Lacey 2004, 2005). They are not grounded in the social or moral value of a theory, its potential to be applied to further human flourishing, a conception of the good society, or privileged economic interests.¹

Social values often have important legitimate roles at the first core moment, adopting a strategy (Lacey 1999, 2003).² Here I agree with Longino, but I disagree with her that they may have indispensable roles at the second, when a theory is accepted, rejected, or held to require further investigation (Lacey 1999). At the second moment all that matters is whether the cognitive values are manifested in the light of available empirical data to a sufficiently high degree so that further investigation is unwarranted. A theory is soundly accepted (of a specified domain of phenomena), I maintain, if and only if it is confirmed that it expresses well-grounded understanding of these phenomena, if and only if it manifests the cognitive values highly with respect to an appropriate array of empirical data gained from observing these phenomena (Lacey 1999, 62–66)—sufficiently highly that the knowledge expressed in the theory can properly be considered *settled*, to need no further evidential support.

When a theory is soundly accepted in this way, it is (I think) properly given what Kitcher calls a “modest realist” interpretation. The evidence supports that it expresses truths about aspects of the phenomena of which the theory is accepted (and some of the possibilities that they permit). These are ‘modest truths’ carrying little metaphysical baggage, and no implications about conveying complete understanding of these phenomena (or ‘the world’). They are consistent with other theories expressing

1. Kitcher (2001, Chapter 6) illuminatingly discusses the *significance*—a notion with both cognitive and social dimensions—of scientific theories and results.

2. Neither Longino nor Kitcher deploy the notion of strategy. Longino’s views are easily paraphrased in terms of it (Lacey 1999, Chapter 9) and her ‘pluralism’ can be portrayed to involve a pluralism of strategies, each one of which is dialectically linked with specific social/moral values. Once a strategy has been adopted, there are still decisions to be made about the specific focus of research, decisions that may clearly be influenced by non-cognitive values. It is unclear to me whether Kitcher upholds a role for social values at (what I have identified as) the first core moment (for he may deny that there is strategic pluralism), or only at the (subsequent) moment of choosing the specific focus of research. This is pertinent to the issue of democratic decision-making in science.

other truths about these phenomena and their possibilities. While not ‘certainties’ (‘necessities’) or *in principle* not vulnerable to further investigation, to accept them is not just to make a provisional judgment. The research that has been conducted has settled the matter practically. There *is* no reason (grounded in considerations of the cognitive values) to submit them to additional investigation, reflecting that there is a clear enough distinction between practically settled results and provisionally entertained hypotheses.

Normally there will be broad *agreement* (within the relevant scientific community) about what is, and what is not, settled knowledge. But agreement is not constitutive of settled knowledge, although that there is agreement on all of the following matters may provide a good reason to believe that a theory is soundly accepted: (a) available empirical data are representative of data that could be obtained from observing the phenomena (in the relevant domain); (b) in the light of this data and other actually accepted theories, the theory manifests the cognitive values highly of domains of phenomena whose limits have been thoroughly tested; and (c) criticism has been exhausted, i.e., there are no further proposals—after allowing for a suitable lapse of time, and being open to the input (criticisms) and testing of divergent perspectives—of (potential) research projects whose outcomes might be expected to put the result into question. Certainly, disagreement on such matters can be expected when a knowledge claim is not settled (see the example in Section 5 below) or a theory is not soundly accepted. But although agreement on these matters provides a good reason to believe that a theory is soundly accepted, it does not follow that agreement is partly constitutive of a soundly accepted theory—just as the reason I have for believing that Fermat’s Last Theorem has been proved, viz. that experts in number theory testify that it has been proved, is not partly constitutive of the theorem being a proved result.

4. Theory Acceptance Partly Constituted by Social Interactions. A soundly accepted theory (of a specified domain of phenomena) is, I repeat, one of which it is confirmed that it manifests the cognitive values highly with respect to (an appropriate array of) empirical data gained from observing these phenomena. Its being soundly accepted is also the outcome of a social process, an outcome of interactions among investigators located in various institutions that embody values considered appropriate in view of the aim of scientific practices, and that foster certain norms of inquiry. Haack (1997) points out that science being social, in this way, “is an important factor contributing to its epistemological distinction.” Note: “contributing to,” not “constitutive of.” The social process has been shaped and institutionally nurtured so that it is conducive to producing soundly accepted theories. But it is not being the outcome of any specific

(actual or ideal) social process that is constitutive of a soundly accepted theory—just as it is not constitutive of being a good automobile that it has been produced in a generally reliable manufacturing process. Kitcher agrees with Haack on this point, although he differs from her (and agrees with Longino) in holding that it is important that the community of investigators reflects a suitable variety of social values.

In contrast Longino considers the “satisfactory performance of certain kinds of social interactions” to be constitutive of accepted theories, interactions engaged in by communities of inquiry that are governed by norms—public recognized venues for appraisal of evidence and methodology, responsiveness to criticism, public standards of appraisal, and tempered equality of participants in research practices (Longino 2002a, 129–135)—followed so as to ensure that there is critical scrutiny from as many perspectives as possible. What is at stake in insisting on “constitutive of” and not just “consequence of”? The key, I conjecture, is Longino’s rejection of Kitcher’s “modest realism” (as well as any other form of scientific realism), a rejection that implies that judgments of sound theory acceptance (as defined above) are never available. Rather, she seems to hold, the cognitive worth of theory acceptance cannot be dissociated from the community in which agreement about it is reached; and thus that any distinction between settled and provisional results is community-relative and reflective of the background assumptions brought to the inquiry by a community. The relativity in question is compatible with there being a considerable amount of trans-communal agreement—even, in principle in the long run, a single shared theoretical perspective on the world—but judgments of acceptance cannot be separated from the background assumptions of the communities of inquiry. For her, judgments of the kind “*T* is soundly accepted” are not available, but only those of the kind “given a community’s background assumptions (and its commitment to a set of cognitive values), *T* ought to be accepted.”

There are theories, Kitcher and I hold, that are soundly accepted. True, the judgment that *T* is soundly accepted is made in a particular community of inquiry, and the community must be constituted in accordance with certain norms (perhaps the four proposed by Longino!) for its judgments to be considered authoritative. This does not imply, however, that making the judgment that *T* is soundly accepted (implicitly) incorporates reference to the community. Compare: At the moment of my writing this passage, the tree outside of my office window has lost most of its leaves. This is true. I see that this is so. Clearly if I were not located as I am, I would not be able to see it. But I do see it and so I judge that the statement is true; it is worthy of my belief and on the basis of my testimony it is worthy of belief generally. But my seeing it is not constitutive of the truth of the statement; rather, my recognition of its truth is a consequence of

my seeing it. The truth of the statement is not relativized to me although (at the moment of writing) only I was placed to recognize that it is true. This does not relativize the truth of the statement to the conditions under which it can become known. Similarly the truth of “*T* is soundly accepted” is not relativized to the community where the conditions are present for it to become known. Of course, outside of this community, its truth can only be known on the basis of testimony from this community, just as (now) you can only know the truth about the tree outside my window on the basis of my testimony. The inference cannot be validly made, from the fact that gaining knowledge depends (causally) on the availability of certain social and moral conditions, to the conclusion that knowledge is relativized to these conditions, that the conditions are constitutive of the knowledge.³

Longino does not mistakenly make this inference. Rather, her position seems to have two sources. The first has already been indicated: She denies that judgments of the kind “*T* is soundly accepted” are available and that we can dissociate accepting theories from community-specific (perhaps not explicitly articulated) background assumptions. Even in a community committed to “critical scrutiny of theoretical proposals from as many perspectives as possible,” the play of the empirical data and the cognitive values leaves *T* *underdetermined*. Then, what an accepted theory *is* (or what scientific knowledge is) is partially constituted by interactions (and the values they express) in the community of inquiry. Second, Longino questions whether cognitive values can be sharply distinguished from social values, and thence she entertains the view that communities of inquiry (depending on the social values they embody) may identify the cognitive values differently (Longino 1997). Then, since cognitive values provide key public points of reference in the critical scrutiny of theories, what constitutes an accepted theory will be partially dependent on the social values of the community of inquiry.

I (and Kitcher) disagree with Longino on both points. I will not address the second point here; for now, suffice it to say that her view is a consequence of failing to separate the two core moments of scientific activity: adoption of strategy and choice of theory (see Lacey 1999, Chapter 9 for a detailed critical discussion). Regarding the first point, consider what I take to be exemplary instances of settled scientific results, items of scientific knowledge expressed in soundly accepted theories (of specified domains of phenomena)—e.g., molecular chemistry, viral and bacterial causation of disease, electronic theory as applied in technological devices, classical mechanical accounts of terrestrial motions. When I say that they are

3. These conditions may be constitutive of the social/moral value of the knowledge.

settled I mean that there *is* no plausible scenario that we can describe in which a new perspective would lead to putting these results into question by, e.g., suggesting new experimental investigations that might put them into doubt. I do not say that there *cannot* be such scenarios. Modest realism does not trade in necessities. We cannot categorically rule out that things may radically change should hitherto unimagined and unconceived possibilities come to be realized. While it is within the scope of scientific activity to explore hitherto unrealized possibilities and how to realize them, and to expand our conceptual and imaginative powers in doing so, scientific judgments are not held to the standard of necessity. Once this is recognized there is no barrier to holding that—as a matter of fact—the sorts of results just mentioned are settled.

Longino does not discuss concrete instances like these where, Kitcher (2002c) and I maintain, underdetermination is not a serious issue. The second source (above) may explain her reluctance to draw the distinctions I draw, and so not to separate appraisals of the sound acceptability and social value (significance) of a theory. Or perhaps the explanation is that there have been theories (put to use socially in opposition to value-laden projects that Longino supports) that were wrongly considered to be settled. These theories were accepted although they did not manifest the cognitive values in the appropriate way, and so were accepted in part because of the social value they have in the light of their adherents' interests. These cases suggest that (at least in some domains of inquiry) it may be important to have a plurality of values represented in the scientific community, reflecting that a theory's sound acceptance may be the consequence of interactions shaped by an appropriate array of values.

5. Legitimizing Applications. Most of the settled results just mentioned are widely valued socially; they have led to numerous applications in social life, and probably for all value-outlooks actually espoused today some of the applications are valued. Other settled results are not like this. Consider:

1. Transgenic maize plants may be genetically engineered so that they produce a toxin fatal to a certain class of insects.

This is settled scientific knowledge. Nevertheless, (1) does not generally have social value; it is not applicable in certain types of agricultural practices (organic, agro-ecological) and indeed its widespread application would pose threats to the integrity of these types of agriculture (Lacey 2002). On the other hand it does have social value for contemporary agribusiness and other institutions (that tend to embody values of capital and the market) that highly value extending human powers to control natural objects into ever more domains of human life (Lacey 2005, Chap-

ter 6). Indeed the research that settled (1) was conducted under (biotechnological) strategies that tend to be prioritized because their products have such social value. Having this origin does not taint the sound acceptability of (1), although it sets limits to its general social value.

Now consider the claims:

2a. There are no environmental hazards arising from the use of transgenic crops that pose serious risks of significant magnitude and probability of occurrence, which cannot be adequately managed under responsibly designed regulations.

2b. There are no alternate ways of farming that could be deployed instead of the transgenic-oriented ways that could be expected to produce greater or comparable benefits.

Although widely propounded in the mainstream scientific community, these claims are not settled; critics continue to propose research projects that might produce evidence that would challenge them.⁴ But unless (2a) has (presumptive) support the *legitimacy* of applying (1) (regardless of the *efficacy* that its being a settled result attests to) is cast into doubt; and (2b) is necessary to legitimate public agricultural policies that prioritize transgenics. Wherever transgenics are used these claims are as important as (1). But the research that settled (1) barely begins to address (2a) and (2b); strategies that address the molecular structure of plant genomes and their possibilities for modification by genetic engineering lack the resources needed to investigate plants qua components of agro-ecosystems, and thus for investigating the social and environmental impact of transgenics and the possibilities of agricultural alternatives based on sustainable agro-ecosystems.

The scientific community is expected to render judgments about (2a) and (2b) (cf. Machamer and Douglas 1999). Empirical evidence *for* them is ultimately the absence of evidence *against* them. Consider (2a). Evidence *against* it would be the actual identification of an environmental risk of significant magnitude and likelihood of occurrence that is intractable to management. But inability at the present time to identify such a risk provides evidence for (2a) only if appropriate and sufficient research has been conducted.

What counts as relevant research? All parties recognize that there are risks. The dispute is about their character, extent, probability, seriousness and manageability under well-designed regulations. Proponents of transgenics claim that all currently known risks can be taken care of under

4. The argument that follows is elaborated in Lacey (2002; 2005, Chapters 9 and 10). In it (2a) and (2b) are given more nuanced formulations.

available regulatory guidelines. Opponents offer theoretical reasons to think that serious risks are involved, some of which are connected with the socio-economic relations of production of transgenic crops, that will only become apparent in the long term—potential harmful effects to the environment, to the maintenance of biodiversity, to the preservation, regeneration and creation of sustainable, productive agroecosystems, and to the livelihoods of poor farmers. Proponents affirm and opponents deny that enough research has been done and that, therefore, the latter should assume the burden of proof. What is “enough”? That depends on the seriousness of the moral stakes involved. They are high for both sides with opponents concerned about potential threats to sustainable agroecosystems and proponents citing potential benefits to farmers, agribusiness corporations and national economies. The values of the opponents underlie their demand for higher standards of testing (and, although I cannot make the argument now, testing under a plurality of strategies). Note: Higher standards of empirical testing not that empirical testing is irrelevant. The matter is not settled. Proponents endorse (2a) considering it supported by sufficient evidence to legitimate plantings of transgenic crops, whereas opponents endorse its negation. Both parties draw upon evidence but to different standards (and kinds!) of evidence that reflect conflicting value judgments. Their respective judgments about (2a) thus involve considerations of both cognitive and social values.

Judgments like these are often made in the course of scientific practices (Douglas 2000). They are appealed to at the moment of application (or policy formation), although they are not simply judgments about the social/moral value of the proposed application. When we attend to them, it is apparent that there is a kind of theory appraisal that is not reducible to acceptance, rejection or deferment pending further investigation.

6. Endorsing a Scientific Claim. Sometimes theory appraisal involves (what I will call) endorsing a theory (hypothesis). A community (or a person) *endorses* *P* when it judges that *P* manifests the cognitive values sufficiently highly so that applying it legitimately (or acting in a way that is informed by it) does not have to take into account (a) that further research might disconfirm *P* and (b) that—if *P* were false—the manifestation of the values (held by affected social groups) might consequently be threatened (cf. Rudner 1953; Douglas 2000). Thus, it is constitutive of endorsing a theory (hypothesis), which does not express settled knowledge, that it is produced under certain social relations (that embody specific social values) that have been cultivated among investigators. Different communities may endorse incompatible propositions without violating any of the canons of empirical inquiry and while engaging in research that aims to test the degree of manifestation of the cognitive values in

theories (hypotheses). It seems to me to be an open question whether all judgments of endorsement, in principle after exhausting empirical inquiry, can be replaced eventually by items of settled knowledge.

Longino, when she speaks of “accepting” theories, does not (on my reading) distinguish judgments of sound acceptance and endorsement. I think the distinction is important, and that matters of efficacy, item (1), should be separated from matters of legitimacy, items (2a) and (2b).⁵ While judgments of sound acceptance are not partly constituted by the social relations (and the values they express) present in the communities in which they are made, judgments of endorsement are so constituted. Longino is, thus, partly right.⁶ At the outset, I summarized the commonly held view that social values should play no part in the proper appraisal of scientific theories and hypotheses (as bearers of understanding and knowledge). Whereas Longino challenges this view head on, I draw a distinction. Social values are not partly constitutive of the sound acceptance of a theory; but when a theory is not soundly accepted, they are of its endorsement.

REFERENCES

- Douglas, Heather (2000), “Inductive Risk and Values in Science”, *Philosophy of Science* 67: 559–579.
- Haack, Susan (1997), “Science as Social?—Yes and No”, in Lynn Hankinson Nelson and Jack Nelson (eds.), *Feminism, Science, and the Philosophy of Science*. Dordrecht: Kluwer, 79–93.
- Kitcher, Philip (1997), *The Lives to Come: The Genetic Revolution and Human Possibilities*. New York: Touchstone.
- (2001), *Science, Truth and Democracy*. New York: Oxford University Press.
- (2002a), “The Third Way: Reflections on Helen Longino’s *The Fate of Knowledge*”, *Philosophy of Science* 69: 549–559.
- (2002b), “Reply to Helen Longino”, *Philosophy of Science* 69: 569–572.
- (2002c) “A Case of Misrepresentation”, *Science* 295: 1466.
- Lacey, Hugh (1999), *Is Science Value Free? Values and Scientific Understanding*. London: Routledge.
- (2002), “Assessing the Value of Transgenic Crops”, *Science and Engineering Ethics* 8: 497–511.
- (2003), “The Behavioral Scientist Qua Scientist Makes Value Judgments”, *Behavior and Philosophy* 31: 209–223.
- (2004), “Is There a Significant Distinction between Cognitive and Social Values?”, in Peter Machamer and Gereon Wolters (eds.), *Science, Values and Objectivity*. Pittsburgh: Pittsburgh University Press, 24–51.
- (2005), *Values and Objectivity in Science, and Current Controversy about Transgenic Crops*. Lanham, MD: Lexington Books.

5. Although Kitcher does not make the distinction explicitly, he has discussed the potential moral and social implications of applications of medical biotechnology in a way that intertwines empirical investigation and his broadly liberal moral outlook (Kitcher 1997).

6. This has implications for pluralism in the scientific community that I cannot address here.

- Longino, Helen E. (1997), "Cognitive and Non-cognitive Values in Science: Rethinking the Dichotomy", in Lynn Hankinson Nelson and Jack Nelson (eds.), *Feminism, Science, and the Philosophy of Science*. Dordrecht: Kluwer, 39–58.
- (2002a), *The Fate of Knowledge*. Princeton, NJ: Princeton University Press.
- (2002b), "Science and the Common Good: Thoughts on Philip Kitcher's *Science, Truth and Democracy*", *Philosophy of Science* 69: 560–568.
- (2002c), "Reply to Philip Kitcher", *Philosophy of Science* 69: 573–577.
- Machamer, Peter, and Heather Douglas (1999), "Cognitive and Social Values", *Science and Education* 8: 36–41.
- Rudner, Richard (1953), "The Scientist Qua Scientist Makes Value Judgments", *Philosophy of Science* 20: 1–6.