



This article appeared in a journal published by Elsevier. The attached copy is furnished to the author for internal non-commercial research and education use, including for instruction at the authors institution and sharing with colleagues.

Other uses, including reproduction and distribution, or selling or licensing copies, or posting to personal, institutional or third party websites are prohibited.

In most cases authors are permitted to post their version of the article (e.g. in Word or Tex form) to their personal website or institutional repository. Authors requiring further information regarding Elsevier's archiving and manuscript policies are encouraged to visit:

<http://www.elsevier.com/copyright>



Contents lists available at [SciVerse ScienceDirect](http://www.sciencedirect.com)

Studies in History and Philosophy of Science

journal homepage: www.elsevier.com/locate/shpsa



Pluralism and objectivity: Exposing and breaking a circle

Anna Leuschner

Bielefeld University, P.O. Box 100 131, D-33501 Bielefeld, Germany

ARTICLE INFO

Article history:

Received 10 August 2011
Received in revised form 21 December 2011
Available online 21 January 2012

Keywords:

Scientific pluralism
Objectivity
Well-ordered science
Climate research

ABSTRACT

The paper argues that Helen Longino's pluralism implies circularity as it claims a preferably high number of qualified contributions to any scientific discussion that aims for objectivity, but does not regard the question who or what sets and employs the standards that rule the decision who is qualified to contribute and who is not. Therefore, objectivity is premised for a process that is to generate that very objectivity. Philip Kitcher's ideal of democratization of science seems only to bypass the problem by introducing ideal deliberators tutored by appropriate experts, as for the implementation of this ideal the deliberators and experts, again, would have to be appointed by someone. However, Kitcher's approach is based on a Rawlsian egalitarianism and in this sense calls for political intrusion which could be based on case-by-case decisions. This offers a solution. I will illuminate the problem by some examples from climatology and demonstrate how Kitcher's approach can help to tackle the problem by a final case study of pluralism in the Intergovernmental Panel on Climate Change.

© 2011 Elsevier Ltd. All rights reserved.

When citing this paper, please use the full journal title *Studies in History and Philosophy of Science*

1. An Introduction on Pluralism in General

During the last two decades, philosophers of science Helen Longino and Philip Kitcher both worked out the chances of finding a “third way” to overcome the fact-value-dichotomy by combining non-epistemic and epistemic requirements in order to fulfill both moral and social as well as epistemological requirements. Between 1993 and 2002, they led a lively debate on this in which Kitcher (being a strong realist at the beginning) made large concessions to Longino's pluralistic arguments. Their agreement will be illuminated below by exposing some conceptual parallels in their latest arguments. Before I start with Longino's conception of pluralism, some general notes on pluralism seem useful for conceptual clarity.

Pluralism can concern diverse areas of science: There might be a plurality of theoretical approaches for solving a problem or understanding a research object; a plurality of methodological procedures; a plurality of people who assess a phenomenon from different perspectives and, hence, illuminate different aspects; a plurality of people who have different value concepts which can also cause different theoretical or methodological approaches (Kellert, Longino, & Waters, 2006). All these kinds of pluralism

may possibly emerge, they can also forward each other and, moreover, can be specifically demanded. But why?

One reason why people argue for pluralism is that scientific progress mostly flourishes when many different approaches are engaged. A problem, they say, is solved faster if people look for a solution from diverse angles and by using different theoretical or methodological approaches. This argument, however, sets some presuppositions which make it quite controversial. They are to be discussed somewhere else. In this paper, I will concentrate on another reason why pluralism is possibly the better way.

This second reason is less controversial but by no means less important. It says, roughly speaking, that different perspectives can make epistemological biases (based on individual preferences) visible and hence eliminable in order to reach objectivity. It is assumed that contextual values influence the perception and interpretation of individual scientists without being recognized due to the underdetermination of theories.

A scientist, looking for an apt theory based on empirical data, has to decide somehow to accept certain hypotheses at a particular degree of probability, and this decision might be based not only on epistemic considerations (is this theory simple, fruitful, consistent etc.?) but also—consciously or not—on non-epistemic preferences

E-mail address: anna.leuschner@uni-bielefeld.de

(is this theory compatible with my personal preferences?). Therefore, value judgments are inevitably embedded in the practice of science. Richard Rudner put this in a nutshell:

[A]ny adequate analysis or (if I may use the term) rational reconstruction of the method of science must comprise the statement that the scientist *qua* scientist accepts or rejects hypotheses; and further that an analysis of that statement would reveal it to entail that the scientist *qua* scientist makes value judgments. (Rudner, 1953, p. 4)¹

Such value influence may happen in disguise, implicitly, tacitly, or, as Longino names it, via background assumptions (this will be pointed out in more detail below).

In the present paper, I will concentrate on this second reason for pluralism which Longino has been arguing for and which I also consider as fruitful. Yet I will expound a problem in Longino's account that emerges from the central notion any pluralistic approach addresses: the notion of objectivity. This will rise the serious question how Longino's ideal can actually be employed.

2. Helen Longino's Pluralism

Longino works out a new feminist approach in the 1990s that gives us a new notion of objectivity. She recognized that science was often "thought to provide us with a view of the world that is objective in two seemingly quite different senses of that term" (1990, p. 62):

- (1) Objective scientific knowledge (which means "true belief" in a realistic sense²), which is produced iff
- (2) the scientists' procedures and methods are objective (which means "unbiased" in a Baconian sense).

However, according to Longino, it is not possible to exclude non-epistemic values from scientific reasoning and decisions because of underdetermination, and even if it were possible, some scientists might just not do it, consciously or not. Moreover, epistemic and non-epistemic values are often not even distinguishable, since non-epistemic values find entrance into scientific reasoning and decisions via "background assumptions" which are influenced by our social, political and ethical value commitments. Such background assumptions cannot simply be excluded by evidence. Hence, scientific objectivity in the sense of (2) cannot be reached, and, therefore, nor can (1) be warranted. What is needed, according to this, is a new concept of objectivity that is not identified with truth any longer.

Longino agrees with standpoint feminism that all hitherto existing scientific programs have been androcentric. She further agrees that only non-androcentric perspectives can criticize and improve them. For example, Sandra Harding (1991) argues that only hitherto repressed perspectives are able to criticize the established epistemological results. Since scientific research has been androcentric so far, only feminism can help to sort out implicit biases. In contrast, Longino (2002, pp. 164–165) does not agree with this conclusion of Harding's, namely that androcentric programs

should be *replaced* by feminist ones, but argues that traditional perspectives should be *supplemented* by feminist perspectives:

To seek a unified, if different, knowledge of the natural and social worlds is to seek power of the sort we reject when exerted over ourselves. The goals driving the search for such knowledge are unworthy of an emancipatory politics. (Longino, 1990, p. 213)

As an alternative solution, Longino proposes an elaborated pluralistic approach. Her idea, strongly influenced by Mill's thoughts on the freedom of speech, is as simple as innovative: In contrast to hitherto existing feminist approaches, Longino in principle considers social differences as epistemological resources rather than obstacles that have to be overcome. For her, any exclusion of qualified contributions makes "not only a social injustice but a cognitive failing." (2002, p. 132) To make background assumptions visible, theories must be exposed to a range of criticism as broad as possible. Only a diversity of perspectives can ensure objectivity, and, therefore, reliable knowledge.

For example, one of Germany's leading climatologists, Hans Joachim Schellnhuber, declared in an interview that he never does demonstrate, is not a member of the Green party, loves meat and drives a BMW (Schellnhuber, 2010). At first glance, this might seem odd because Schellnhuber points to his personal preferences to appear trustworthy *as a scientist* which could be considered as paradox since he obviously wants to argue for value neutrality of his work by invoking certain values. But Schellnhuber's statement must be seen as a desperate reaction to the climate skeptics' standard argument, an argumentum ad hominem, which claims, roughly speaking, that global warming "is a device used by Birkenstock-wearing, tree-hugging, business-hating, liberal intellectuals for advancing their political aims." (Kitcher, 2010, p. 1233)³

Schellnhuber's claim is thus rather to be interpreted in Longino's sense: The influence of values in scientific reasoning can be exposed when as many scientists as possible are invited to reproduce, understand and approve the data, theories and decisions of their colleagues. If all qualified contributions, including their mutual criticism (in climate research: from the very left to highly conservative perspectives) are equally considered, biases become visible. Thus, if a climate researcher as Schellnhuber who is not left-leaning arrives at results which resonate with the ecologic movement, these results should be considered as highly reliable. So Schellnhuber's statement is a positive example for Longino's claim that exposing value assumptions can increase the scientific credibility of a finding. Oreskes and Conway also argue for the IPCC's reliability that way by pointing out that the IPCC is "an organization that encompasses the views and work of thousands of climate scientists around the globe—men and women of diverse nationality, temperament, and political persuasion." (Oreskes & Conway, 2010, p. 268) I agree with this view and consider it a highly plausible argument.

Thus, I am basically very sympathetic to Longino's account. It has been a significantly new and promising contribution to the debates on social epistemology, promising to eradicate biases that are based on idiosyncratic value assumptions or even on value-laden

¹ Heather Douglas (2000, 2009) initiated a new debate on Rudner's old argument which deserves attention. While I do not agree with Douglas that cognitive values are not truth-indicative, I agree with her that these values do not always suffice to decide which theory is true. Other values certainly influence the scientists' decisions, too, and this becomes particularly obvious in research projects that imply risks of both type 1 and 2 errors. However, I consider Rudner's point more fundamental than Douglas seems to. By invoking a Quinean holism, Rudner makes clear that he is pointing to a general feature of the sciences—pure as well as applied science which differ from each other "only in degree and not in kind" (Quine, 1951, pp. 71–72). Although there may be areas of pure research that do not imply any risks, the problem of underdetermination exists there, too. Thus, Rudner concludes that value judgments are an inevitable part of scientific decision-making in general—not only in applied but also in basic research.

² I.e. independent of what people think, feel or are convinced of.

³ Climate researchers are under enormous pressure as the objectivity of their work is frequently being questioned that way. This went so far that in May of 2010, 250 scientists from national academies signed an open letter in *Science* in which they protested against "the McCarthy-like threats of criminal prosecution against our colleagues based in innuendo and guilt by association, the harassment of scientists by politicians seeking distractions to avoid taking actions, and the outright lies being spread about them." (Sills, 2010, p. 689).

prejudices, but at the same time avoiding to impose any (feminist or other) dogma on the scientific enterprise. I consider it both an appropriate way for exposing any value judgments and helpful to overcome the problems the fact-value-dichotomy brings up. However, by further scrutinizing Longino's conception it becomes obvious that it implies a serious problem.

3. Circularity

Longino is arguing for a pluralism of perspectives and values; she also admits a theoretical and methodological pluralism, yet calls for regulatory measures in terms of organizing it: First, criticism must be carried out publicly; second, points of criticism concerning consisting theories must be taken as seriously as new studies and results; third, everybody, in principle, should be able to enter the discourse; but this does, fourth, not mean that any unqualified person is allowed to embark on the debate. In sum, freedom of speech is of paramount importance, yet it must be regularized to avoid a chaos which would hinder the scientific advancement.

A problem is coming up here, as to support her fourth requirement, Longino demands "public standards" by which it is to be decided whether a contribution is qualified enough to become part of a scientific discussion, and who, for that matter, is qualified to contribute to scientific decision-making (1995, p. 385, 2002, pp. 129–132). This is supposed to avoid a "cacophony" (Longino, 2002, p. 133) that would threaten scientific advancement if unqualified people were allowed to contribute to scientific decision making. However, the third and fourth point necessarily get in conflict: The requirement of evaluation standards causes circularity when for objectivity only *qualified* contributions are acceptable, yet the decision about whether a contribution is qualified or not already *requires* quality standards that are shared by all community members. Thus, a contribution must fit to the community members' established opinions and expectations. That way, objectivity (of the standards) is premised for a process (pluralism) that is to generate that very objectivity.

The basic problem for Longino is that the free exchange of arguments is seen as a method that advances the sciences in an objective way. This however presupposes point (2) mentioned above, namely that the scientists' procedures and methods are unbiased as this is required for claiming that the epistemically best approach in fact wins the day. But (2) is based on individual impartiality which contradicts Longino's assumption that individuals are influenced by personal preferences so that only social interaction is a sufficient means of generating objective knowledge.

At first, let me try to relieve Longino's approach from this objection. A possible answer could be to simply presuppose certain standards by claiming their objectivity. This means just to *define* that a contribution is only acceptable in the scientific debate if it fulfills certain conceptual, theoretical or methodological preconditions. But this raises the question who defines these standards and who has built those conceptual, theoretical or methodological preconditions that shape the standards. The problem with simply presupposing certain quality standards is that this way neither the standards themselves nor their preconditions get into the position to get criticized. This would exclude those arguments from the scientific debate which do not meet these standards, e.g. because they come from outside the scientific community. Such exclusion however would contradict Longino's claim that objective knowledge (and also objectivity of the quality standards) can only be reached by pluralistic procedures that include all relevant criticism.

In the debates about climate research, for example, climate scientists have taken the arguments advanced by the skeptics serious, have tested the objections, refuted them or modified their methods

and claims. The skeptics, however, instead of refuting the arguments by climate scientists or accepting them, typically ignore them: Being refuted in a discussion, for instance, and being compelled to concede a point, the skeptics stick to their original position at the next opportunity nonetheless, simply ignoring the evidence provided by the climatologists (Oreskes & Conway, 2010). The ad hominem arguments mentioned above typically come along with this ignorance. This behavior can especially be observed in research fields the outcomes of which concern certain, especially industrial stakeholders' interests. This phenomenon has currently been thoroughly analyzed by a new approach in the science studies named *agnotology* (Proctor & Schiebinger, 2008).

In other research areas, it was just the other way round when scientists ignored important criticism coming from outside the scientific community. Steven Epstein has illuminated in a history of the research of AIDS how (epistemically and morally) important it was that people from the outside, namely concerned laypeople, interfered in the scientific research, and what huge difficulties these people had to overcome (Epstein, 1995). Another convincing case study delivers Brian Wynne's paper on *Sheep Farming After Chernobyl* exposing how experts can fail if they ignore lay people's knowledge:

The allocations of authority and power inherent in routine decision making communicate built-in assumptions about which kinds of experience and social groups are worth something and which are marginal. If a communication program ignores this social and historical context, it is likely to be self-contradictory, unrelated to rooted experiences and concerns, and thus ineffective. (Wynne, 1992, p. 57)

Assuming that in these cases competing groups discredit each other from the outset, pluralism in Longino's sense cannot work since her third requirement that everybody, in principle, must be able to enter the discourse is undermined. Hence, as long as one agrees with Longino's premise that for epistemic reasons it is desirable to have as many contributions to scientific discussions as possible because only that way objectivity can be generated, it would cause inconsistency to premise the objectivity of certain standards to sort out inconvenient contributions.

One could insist that these standards in fact only sort out *unqualified* contributions; Longino would possibly answer that way. In fact, she distinguishes two possibilities of how to disqualify contributions: "evidential criticism" relating to the accuracy of data and "conceptual criticism" regarding the background assumptions (Longino, 1990, pp. 71–74). Certainly, the circularity I have located concerns only the second kind of criticism. However, if a community decides that for example a set of raw data is not sufficient or just not relevant or should be interpreted in another way, it will not play a role any more whether these data are accurate or not. Therefore, Longino's two kinds of criticism do not help to break the circle either.

Even more, arguing that there could be standards that sort out only *unqualified* contributions tacitly claims that the objectivity (A) of scientific knowledge is independent of the objectivity (B) of a scientific community's epistemological setting, and, thus, independent of the idiosyncratic or value-laden preferences of the community. Yet claiming such independence just poses the basic problem mentioned above as this claim entails the traditional linear model of scientific epistemology: If scientists only proceed in an unbiased way, their criteria will be purely epistemic, and, hence, their judgments will be objective. This again presupposes the above objectivity conception (2) and, thus, simply ignores that individual assumptions within empirical sciences might be biased (consciously or not), a problem arising from the underdetermination thesis that Longino so thoughtfully takes into account.

For consistency, Longino would, hence, have to argue on the contrary that the standards that set the conditions of (A) can hardly be independent of (B), i.e. the theoretical, conceptual and methodological standards a scientific community already holds. And it is important to see that such independence would not even be desirable, unless, indeed, a “cacophony” would be. But it is this dependence of (A) and (B) that leads us to the very circle from the beginning of this section.

All in all, the interdependence of (A) and (B) causes a fundamental problem: Only those contributions that are qualified with regard to standards which are already established within a scientific community become part of the community's discussions.⁴ Sylvia Culp pointed to this problem:

[I]f intersubjective agreement depends only on community standards, even when dependence on idiosyncratic presuppositions is eliminated, dependence on presuppositions shared by the entire community might not be. Any raw data interpretation dependent on a false theoretical presupposition, even one shared by an entire community, will be biased. (Culp, 1995, p. 438)

The only comprehensive solution for this problem is to argue for an “anything goes” and altogether reject shared quality standards. But that would be likely to end up in that very “cacophony” which Longino has with good cause sought to avoid. Karyn Freedman, by discussing what she names “realistic pluralism”—i.e. a pluralism as Longino's that does not allow “a multiplicity of conforming representations of the world that are contradictory, inconsistent, or mutually exclusive” (Freedman, 2009, p. 54)—gets to the core of the matter here:

[W]ithout some assurance that the world will decide which of our contextually based standards and goals of science triumphs when there is real disagreement between and within communities, the pluralist realist is left defenseless against epistemological relativism, with each group using their own standards to judge their own results as superior. (Freedman, 2009, p. 54)

Keeping this in mind, I do not consider “anything goes” as a satisfying solution of the problem. We need in fact “some assurance”. Hence, I will argue that Philip Kitcher's ideal of Well-ordered Science provides us with an account how to break the circle, even though not comprehensively but case-specifically.

4. The Ideal of Well-ordered Science

After years of controversy, Kitcher meanwhile (2001, 2011) agrees with Longino's considerations in principle. He is aware of the problem of circularity of objectivity and suggests to bypass the problem by introducing so-called deliberators who serve as intermediaries between the research community and the broader public. These deliberators, in Kitcher's sense, consist of representative groups of citizens who are tutored by appropriate experts and, in light of this tutorial, have to come to a consensus about which research agendas should be pursued. That Kitcher claims for representativeness of the groups means a basic agreement with Longino's very precondition that no relevant perspectives should be excluded. Moreover, the ideal deliberators make their decisions behind a Rawlsian veil of ignorance: The ideal decision is

considered as made under the decision makers' complete ignorance of their own position. If they do not know whether they will be, say, profiteers or victims of their own decision, they will weigh these possibilities up and try to come to a really fair decision. Thus, also the tutored deliberator in Kitcher's account would avoid any injustice:

They do not know how the various alternatives will affect their own particular case and they are obliged to evaluate principles solely on the basis of general considerations. [...] They understand political affairs and the principles of economic theory; they know the basis of social organization and the laws of human psychology. [...] There are no limitations on general information [...]. (Rawls, 1999, pp. 118–119)

The deliberators in Kitcher's sense are tutored by ideal appropriate experts. These experts “agree on exact probabilities with respect to the outcomes of research.” (Kitcher, 2001, p. 120) Ideal scientific decisions, then, are those that are reached through such deliberation, and science can be considered well-ordered when its specification of the problems pursued is endorsed by an ideal conversation, embodying all points of view that are concerned by the respective scientific decision.

However, by introducing a deliberative democracy Kitcher's account is threatened to just take the circularity that Longino's approach suffers from *to another level*, as again somebody has to decide somehow who is qualified and sufficiently informed to partake, namely in the deliberation process. We even find a terminology parallel to Longino's in Kitcher's approach: Again, “public standards” are required; Kitcher names them “objective standards” (2002, p. 555), and again, these standards are essential to avoid a “cacophony” to which Kitcher refers as “vulgar democracy” (Kitcher, 2001, pp. 117–135, 2004, p. 55).⁵ Here, it becomes obvious: The problem that concerns the scientists in Longino's approach also concerns the people within Kitcher's deliberation model, namely the ideal deliberators and experts. The standards are required to decide what groups of citizens are representative and what experts are in fact appropriate to be asked for tutorials. Yet the objectivity of such standards cannot be generated by an ideal deliberation process because such process already requires the objectivity of quality standards.

It is important to see that this is not a problem for Kitcher's ideal as such but for its *implementation*. He himself admits that “the challenge is to find institutions that generate roughly the right results, even though we have no ideal deliberators to make the instantaneous decisions we hope to replicate.” (Kitcher, 2001, p. 123) Accordingly, I will argue in the following that this circle can and should be broken by pragmatic measures, namely through political intrusion. Moreover, I will extend his ideal by arguing that in order to face the inner-scientific circle, deliberative instances staffed with experts (not with citizens) should be commissioned.

5. A Pragmatic Solution

The problem for pluralism is, as we have seen, very unpleasant and quite comprehensive: If pluralism takes as a premise that no qualified contribution should be excluded, it must establish certain standards that define qualification. Yet such standards already require certain methodological, conceptual and theoretical

⁴ In fact, we can observe that women or other disadvantaged groups have been excluded from scientific fields despite shared quality standards that often even have been introduced to overcome just that kind of discrimination (“women will be preferred in case of equal qualification”). Even worse, these standards can be misused to *justify* decisions as objective that are in fact discriminatory as one can always find a way to interpret the given qualification criteria in a way that disqualifies a certain contribution. Notably, Janet Kourany (2010, pp. 58–62) has recently pointed to this gender problem from another direction by arguing that even if Longino's requirements were completely met one could think of a community that is, even though open now to women's contributions, completely staffed by men; this is, as Kourany correctly argues, due to the fact that the best quality standards are worthless as long as women are not in the social (e.g. educational, financial or emancipated) position to contribute.

⁵ Kitcher uses the term “vulgar democracy” in contrast to the “enlightened democracy” in his ideal of Well-ordered Science.

presuppositions: The quality standards will exclude such contributions that contradict these presuppositions. In this way, a contribution that is substantially new or just inconvenient will have a hard time finding its way into the scientific debate. Longino's pluralism is circular in this respect, and in reality quite unhelpful as a generator of objectivity.

"Anything goes" has also been rejected as an alternative as long as one wants to avoid a "vulgar democratic cacophony". Rather, we must somehow reach an agreement between pluralistic requirements and epistemic quality standards. But this must be a political task since we cannot solve the problem analytically, i.e. in accordance with the conceptual and methodological conditions that Longino's pluralism provides. This means that one must agree to impose some regulations on the "free market place of ideas". Yet imposing certain regulation (for example by introducing hiring quotas or by constraining some kind of research which is clearly meant to cause disadvantages to some groups of people) would cause inconsistency within both Longino's and Kitcher's accounts. It would threaten their shared basic claim for the freedom of speech since it would undermine the requirement that, in principle, everybody should be able to participate in the discourse, and, thereby, contradict the endeavor to develop an account that rejects power which is exerted over some groups of people. What could be brought up against this problem?

Jim Brown answers to this problem in a criticism of Kitcher's ideal of Well-ordered Science: Kitcher, Brown says, should not be worried that political regulation could cause the danger of "some kind of backlash [...] in the form of demoralizing insults like] 'You got the job just because you're a woman.'" (Brown, 2004, p. 602)

[T]hese insults, while real, have turned out to be not all that common and, in any case, are rather insignificant compared with the very dramatic improvements in the prospects of women and minorities. It is not as if sexists and racists would refrain from vicious comments if there were no affirmative action programs to provoke them. The real alternative is: job plus insult versus no job plus insult. (Brown, 2004, pp. 602–603)

I totally agree with Brown on this and conclude that Longino's, say, neoliberal idea of pluralism, based on the classic liberal idea that "the best test of truth is the power of the thought to get itself accepted in the competition of the market", a market that regulates itself through "free trade in ideas" (Justice Holmes, quoted after Krotoszynski, 2006, p. 14), without any assessment and control from the outside (i.e. from the public and the politics), must be dropped. Free trade in ideas certainly brings us closer to the truth, but only if it is politically regulated since it is always in risk to be dominated by certain non-epistemic interests. This idea of free exchange should also be dropped by Kitcher who sticks to it no less than Longino. Though he currently makes large concessions to his former critics by pointing to the problem that some democratic values like free debate or the will of the people, ideals once expressed by Mill and Jefferson, are obsolete regarding today's problems, and though Kitcher admits that sticking to these ideals does not enhance the freedom of people, he still dislikes the idea that some form of regulation must be imposed on the sciences through political intrusion (Kitcher, 2011). The regulation he has in mind rather concerns economic policies like controlling the emission of greenhouse gases.

On the contrary, I argue, in accordance with Brown, that the consequence of the circularity problem must be to demand that certain regulation measures be politically imposed on research

projects if this is important, e.g. by quota regulations to support disadvantaged groups or also by systematic funding of research approaches that are essentially new⁶—or even to constrain research that should not be undertaken for social or moral reasons (e.g. research on racial differences in intelligence). The decision whether a research project should be subject of such deliberation must be made by political instances. This seems essential to overcome biases based on idiosyncratic value assumptions or value-ladenness and hindering the scientific advancement.

It is important to see that "politically imposing" does not necessarily mean governmental action. I use the term in a far broader sense: Scientific boards can also be political instances. Their decisions are political insofar they are not results of a mere reduction from natural evidence but the outcome of negotiations that take both parts into account, epistemic and non-epistemic requirements. A university's hiring quota can be such a political decision just as an international regulation of risky technologies or a national or international order to undertake a certain research project. Brown sets things straight here by pointing out that "academic freedom and excellence require something like speech codes and hiring quotas." (Brown, 1997, p. 381) I argue likewise that such regulatory measures are helpful to reach Longino's and Kitcher's common ideals in epistemic as well as moral respect: *Epistemically* they are helpful in avoiding a disqualification of fruitful new approaches just because these do not fit into established standards (that might be biased themselves); *morally* they seem helpful by improving the situation of the disadvantaged that have been excluded from scientific research so far.

Kitcher's ideal of Well-ordered Science with its deliberative instances that function as mediators between science and the public is useful here as it shows a viable way how regulatory measures could be found and realized. Kitcher's claim for including citizens in the process is to be seen as an educational model to face the erosion of scientific authority in the public caused by overcomplexity of scientific relations or even by disinformation of the public; he points, for example, to Fishkin's model of deliberative democracy (Kitcher, 2011, pp. 222–226). To face the inner-scientific circularity problem, Kitcher's model must, however, be extended by deliberating expert panels that, while representing the scientific consensus, explain problems and risks to the broader public and in particular to politics in order to *regulate* the sciences that way, i.e. to assign the case-specific problem identification and solution finding to politics. To decide how such expert panels are to be staffed and what their rights and duties are must be the task of democratically legitimated instances (e.g. the UN or national governments or committees advocating equal opportunities). We can observe that it is in fact a political standard procedure to commission such (local as well as global) deliberative instances in order to regulate certain scientific fields by trading off epistemic against non-epistemic requirements—especially in those areas of scientific research that are politically (i.e. economically, ecologically, morally, socially etc.) relevant—think of the International Atomic Energy Agency (IAEA), Independent Ethics Committees or the Intergovernmental Panel on Climate Change (the latter will be explored in a case study below). The more particularistic interests are affected by a field of research, its decisions and results, the more stakeholders and political parties are interested in certain outcomes, and the greater this interest is, the greater the pressure. Here, deliberative instances have to balance epistemic and non-epistemic requirements particularly carefully.

⁶ The Volkswagen Foundation, for example, has been particularly funding projects which are substantially new and may, hence, well harbor potential risk to fail but, on the other hand, stand a chance to provide "a change in working methods, or inducing the realization of latent synergies by new forms of cooperation." (VW, 2011) Note that this does not necessarily mean that such pluralism brings us *faster* to knowledge as the other claim for pluralism mentioned in the general introduction on pluralism in the beginning of this paper. It rather means that this pluralism is important to reach objectivity since a plurality of research approaches leads to a broader range of criticism.

Longino's considerations play an important role here as deliberative instances in such policy-relevant cases are in an ideal case pluralistically organized. But how this pluralism is structured, what standards are involved to decide who is qualified to partake in the deliberation process and who is not must be decided politically on a case-by-case basis since these decisions will probably not turn out as a final consensus from a scientific "free trade in ideas" (at least not in highly complex and policy-relevant research fields such as (bio-)medical research, genetic engineering, or climatology—and it is these fields that Longino's considerations are particularly important for). Instead, political or moral interests that are concerned by a scientific field of research should be considered by politically commissioned deliberative instances which must come to case-specific decisions to be forwarded to the public and politics. Kitcher's Rawlsian ideal of Well-ordered Science can be very helpful here to find suitable norms how such instances should be ideally organized in order to enable these instances to come to decisions that fit best for all those who are concerned by the respective research program.

This solution appears very pragmatic. What can we expect from it? Or, more specifically asked: What exactly is meant by political case-by-case decisions which have to trade off pluralistic requirements against epistemic quality standards? I eventually want to respond to these questions by illuminating a case study that shows that these methodological considerations concern by no means only feminist problems but all people in all societies.

6. Case Study: The IPCC

A good example of a deliberative instance in the sense I interpret Kitcher's ideal (i.e. the instance consisting of experts rather than citizens) is the Intergovernmental Panel on Climate Change (IPCC). Who is in charge of deciding who is qualified to deliberate here?

As the problems identified and researched by climatology are global, it is, for good reasons, the UN which established the panel in 1988 and still controls it: Thus, almost all countries that are concerned by the IPCC's decisions are represented. The panel is organized in a strictly pluralistic way, regarding all aforementioned areas of science, its staffing as well as the methods and theories being used.

First, the panel consists of scientists from all around the world, and from both industrial and developing countries: there is social pluralism. Second, in the IPCC assessment reports, many information sources are taken into account to warrant a balanced assessment of the literature—including non-English language and gray literature from non-scientific publications from NGOs or developing countries. This is important to hold the IPCC's assessments up to date; in particular, many important raw data collections stem from developing countries, such as data from the World Bank, the International Energy Agency, or the International Monetary Fund (Edenhofer, 2010). Third, the panel's reports include many different scenarios for possible future conditions on Earth, each of them having to run through different simulation models (Parker, 2006); this means that there is also theoretical and methodological pluralism.

From an epistemic point of view, this comprehensive pluralism seems wise since biases (e.g. induced by industrial countries' economic interests) are held in check by keeping interests transparent through the integration of members from all over the world. Moreover, the highly complex scientific object, the global climate, can be understood best if as many data from as many sources of

regional knowledge as possible is available: Climatology can only operate by modeling possible scenarios; since it is required to assign probabilities to these different scenarios, expert knowledge regarding regional social, economic and ecological conditions is needed (Schneider, 2002).

From a political point of view, it seems reasonable to cooperate on a broad international basis to achieve resilient international agreements on alleviating climate change and mitigating its consequences. If the members came solely from industrial countries (which has been demanded by prominent climatologists since "Climategate"), administrations of developing countries could refuse to cooperate by claiming the assessment reports to be essentially influenced by western industries, hence being biased and not reliable.

Unfortunately, this very pluralism caused some faults in the last assessment report: Some data errors were revealed which stemmed from non-scientific sources. Himalayan glaciers, the second IPCC group's assessment report claimed, "will likely shrink from the present 500.000 to 100.000 km² by the year 2035." However, these data were copied from a flawed WWF paper: the Himalayan glaciers cover only 33.000km². Additionally, the year 2350 was transposed as 2035.⁷

This fits exactly the theoretical problem analyzed above: Epistemic quality got in conflict with pluralistic requirements. On the one hand these data were urgently needed; on the other hand these very data fulfilled only low quality standards (Cogley, Kargel, Kaser, & van der Veen, 2010). In general terms, the risk to include flawed data is to be traded off against the risk to exclude appropriate data. In this case, the trade off failed.

The conflict is additionally problematic here as climate research concerns many competing interests; it is a highly political field of research. This is why these troubles fell on a fertile ground of skepticism: The conflict escalated in November of 2009 in the "Climategate" scandal when the private emails of a group of climatologists had been hacked. In the following period of time, huge public debates arose about how to proceed in the IPCC-case. In *Nature*, for example, it was discussed whether the IPCC should be "tweaked or scraped"? (Hulme, Zorita, Stocker, Price, & Christy, 2010) Eventually, three commissions came to the unanimous result that there was no reason to allege any kind of conspiracy within the climate research community. The UN commissioned a so-called InterAcademy Council (IAC) to analyze the management and review structures of the IPCC. IACs consist of members of national academies of science. The IAC that reviewed the IPCC worked out some recommendations how to improve certain weak points. There is no basic criticism on the methods and procedures in the IAC's report, even if some improvements are considered useful, e.g. regarding the responsibilities and duties of leading positions within the panel. The IAC also suggests to release the report of the Working Group I a year or two before the reports of the other two Working Groups (on possible impacts and mitigation of climate change) so that these can benefit from the physical scientific results of the first group. As for the use of gray literature, the IAC recommends to work out specific guidelines for evaluating these sources more thoroughly but not to give it up as relevant and appropriate data would be lost otherwise (IAC, 2010).

The UN's decision neither to suspend the IPCC nor to change its comprehensively pluralistic structure (which was demanded even by some of the IPCC-members themselves) but rather to call an IAC for newly balancing both parameters in the IPCC—the pluralistic requirements and epistemic quality standards—is a political case-by-case decision in the pragmatic sense stated above, and, as it

⁷ There are other errors like e.g. the claim that 55 percent of the Netherlands lie below sea level while it is in fact only 26 percent. This was quoted from a publication of the Netherlands Environmental Assessment Agency.

seems, the best decision that could have been made in epistemological, political and moral terms.

7. Conclusion

I argued that in general, and especially in socially, morally, economically or ecologically relevant sciences that have to deal with complex research objects, deliberative instances including a plurality of perspectives are helpful for both moral and epistemic reasons. Pluralism, in all aforementioned facets, is required here to sort out biases that are imposed by distorting background assumptions, and to reach an agreement between non-epistemic requirements and epistemic quality standards. But pluralism does not work all by itself; without regulation from the outside, it is not a reliable method to sort out biases neither in epistemic nor in moral terms because (as long as it is not anarchistic in Feyerabend's sense) it needs to employ shared quality standards. These standards can be biased themselves or can be too restrictive in order to disqualify inconvenient contributions. Here, we enter a circle as the shared standards determine which contribution or criticism is considered as qualified and which is not. However, if only those contributions or points of criticism which meet the shared standards are taken into consideration, the uptake of criticism which is essential for scientific advancement is threatened, and the standards themselves are less likely to be called into question. Despite of shared standards, contributions might be disqualified although they are important in epistemic or moral terms. To break this circle, political case-by-case decisions are essential.

Within Longino's account, the circularity problem is not analytically solvable. I argued, therefore, that one of Longino's premises must be dropped, and I consider it plausible that what should be dropped is her idea that imposing regulation on scientific projects is "unworthy of an emancipatory politics". Although pluralism is needed, it must be somehow assessed and regulated from the outside to make sure that it does not end up in a circle by only reproducing biases. To quote Brown again, if a professor told class "about the 'international Jewish conspiracy,' or that blacks are not as intelligent as whites, or that women can't do math, then speech codes should come into play. When stupidity and viciousness are in unholy alliance, our teaching and research goals are undermined. Academic freedom must *serve* our aims, not subvert them." (Brown, 1997, p. 383) This brings up another crucial point: Of course, some ethical principles might be indisputable and universal. But others might be controversial. Thus, in many cases, local rules will have to be found that are applicable to only one special field of research in a particular moment in history.

What is needed are comprehensive case-specific discussions on the utility, limitations and location of non-epistemic values in sciences, in particular when research projects become policy-relevant. Here, Kitcher's considerations come into play. Even though Kitcher's model of deliberation is basically affected by the same circularity as Longino's pluralism because the people involved in the deliberation process have to be appointed by someone for some reasons as well, Kitcher's ideal seems helpful as he proposes an ethical framework, based on a Rawlsian egalitarianism, which points to important features of how deliberation should be ideally organized.

In sum, my analysis yields the following outcome:

1. Longino's pluralism of perspectives is fruitful to eradicate biases that are based on both idiosyncratic value assumptions or value-laden prejudices. Yet her approach suffers from an inherent circularity.
2. Kitcher's ideal of Well-ordered Science proposes a helpful ideal approach of how to break this circle and to realize Longino's pluralism. Yet an implementation of this ideal is not at all unlikely to suffer from the very same problem of circularity as Longino's.
3. I located the reason of this circularity in their both adherence to Mill's ideal of a "free market place in ideas" which implies the assumption of individual impartiality and, thus, contradicts Longino's and Kitcher's common premise that eradicating biases and distortions requires social procedures.
4. Therefore, it is important for moral and epistemic reasons to drop this ideal and to rather claim for political intrusion into the scientific endeavor in order to meet the egalitarian ideal Kitcher and Longino strive for. Such political intrusion means case-specific regulation through deliberative expert panels.

As a proposal how to put this outcome into action I argued for an implementation of Kitcher's ideal of deliberative instances, albeit not staffed by tutored citizens. Instead I argued for deliberation through politically commissioned expert panels like Independent Ethics Committees, the IAEA, or the IPCC. Such instances should be pluralistically organized, and they should be commissioned and regulated by political instances (governmental and intergovernmental boards, scientific boards of universities or universities' departments etc.) so that Kitcher's Rawlsian egalitarianism can be met.

In general, it appears an important task for philosophy of science to provide more of such normative discussions on how to organize ideal deliberative instances, in general and in every single case: in climatology as well as in nuclear issues, in genetic engineering or nanotechnologies, and even in fields of basic research that are politically irrelevant when it comes, for example, to questions of gender quota. Case-by-case decisions might be considered an unattractive solution for a universal theoretical challenge like this. But even if it is not possible to find a satisfying solution for all cases, this does not mean that it is not possible to find a satisfying solution for every case. For this purpose, Kitcher's ideal of Well-ordered Science provides us with a fruitful ethical framework, and the IPCC with this broad pluralism in its methodological, theoretical and social structure, and politically regulated by the UN, is a good example how it could be brought to work.

Acknowledgments

I want to thank Mark Brown, Martin Carrier, Kevin Elliott, Stefan Gärtner, Philip Kitcher, Ágnes Kovacs, Ulrich Krohs, Bertolt Lampe, Cornelis Menke, Torsten Wilholt and two anonymous reviewers for this journal for helpful criticism and valuable comments. Thanks also to those who provided kind and helpful feedback when a very early version of this paper was presented at the Future of Philosophy of Science conference in Tilburg in April 2010.

References

- Brown, J. R. (1997). Academic freedom, affirmative action, and the advance of knowledge. *Interchange*, 28(4), 381–388.
- Brown, J. R. (2004). Book reviews: Science, truth, and democracy. *The Journal of Philosophy*, 101(11), 599–606.
- Cogley, G., Kargel, J., Kaser, G., & van der Veen, C. (2010). Tracking the source of glacier misinformation. *Science*, 327, 522.
- Culp, S. (1995). Objectivity in experimental inquiry: Breaking data-technique circles. *Philosophy of Science*, 62(3), 430–450.
- Douglas, H. (2000). Inductive risk and values in science. *Philosophy of Science*, 67(4), 559–579.
- Douglas, H. (2009). *Science, policy, and the value-free ideal*. Pittsburgh: University of Pittsburgh Press.
- Edenhofer, O. (2010). IPCC kommt auf den Prüfstand. Online: <http://www.ipcc-wg3.de/Members/edenhofer/in-the-media/10-february-2010-frankfurter-allgemeine-zeitung-ipcc-kommt-auf-den-prufstand>. Accessed 10 January 2012.

- Epstein, S. (1995). The construction of lay expertise: AIDS activism and the forging of credibility in the reform of clinical trials. *Science, Technology & Human Values*, 20(4), 408–437.
- Freedman, K. (2009). Diversity and the fate of objectivity. *Social Epistemology*, 23(1), 45–56.
- Harding, S. (1991). *Whose science? Whose knowledge? Thinking from women's lives*. New York: Cornell University Press.
- Hulme, M., Zorita, E., Stocker, T., Price, J., & Christy, J. (2010). IPCC: Cherish it, tweak it or scrap it? *Nature*, 463, 730–732.
- IAC (2010). Climate change assessments. Review of the processes and procedures of the IPCC. Ed. by the Committee to Review the IPCC of the InterAcademy Council. Online: <http://reviewipcc.interacademycouncil.net/report/Climate%20Change%20Assessments,%20Review%20of%20the%20Processes%20&%20Procedures%20of%20the%20IPCC.pdf> Accessed 10 January 2012.
- Kellert, S., Longino, H., & Waters, K. (2006). Introduction: The pluralist stance. In S. Kellert, H. Longino, & K. Waters (Eds.), *Scientific pluralism* (pp. vii–xxix). Minneapolis: University of Minnesota Press.
- Kitcher, P. (2001). *Science, truth, and democracy*. Oxford: Oxford University Press.
- Kitcher, P. (2002). The third way: Reflections on Helen Longino's the fate of knowledge. *Philosophy of Science*, 69(4), 549–559.
- Kitcher, P. (2004). On the autonomy of the sciences. *Philosophy Today*, 48(Suppl. 5), 51–57.
- Kitcher, P. (2010). The climate change debates. *Science*, 328, 1230–1234.
- Kitcher, P. (2011). *Science in a democratic society*. New York: Prometheus Books.
- Kourany, J. (2010). *Philosophy of science after feminism*. Oxford: Oxford University Press.
- Krotoszynski, R. (2006). *The first amendment in cross-cultural perspective. A comparative legal analysis of the freedom of speech*. New York: New York University Press.
- Longino, H. (1990). *Science as social knowledge. Values and objectivity in scientific inquiry*. Princeton: Princeton University Press.
- Longino, H. (1995). Gender, politics, and the theoretical virtues. *Synthese*, 104, 383–397.
- Longino, H. (2002). *The fate of knowledge*. Princeton: Princeton University Press.
- Oreskes, N., & Conway, E. (2010). *Merchants of doubt. How a handful of scientists obscured the truth on issues from tobacco smoke to global warming*. New York: Bloomsbury Press.
- Parker, W. (2006). Understanding pluralism in climate modeling. *Foundations of Science*, 11(4), 349–368.
- Proctor, R., & Schiebinger, L. (2008). *Agnotology: The making and unmaking of ignorance*. Stanford: Stanford University Press.
- Quine, Willard van Orman (1951). On Carnap's views on ontology. *Philosophical Studies*, 2(5), 65–72.
- Rawls, J. (1999). *A theory of justice*. Oxford: Oxford University Press.
- Rudner, R. (1953). The scientist qua scientist makes value judgments. *Philosophy of Science*, 20(1), 1–6.
- Schellnhuber, H. J. (2010). Die Wolkenschieber. *Der Spiegel*, 29 March 2010, 140–149.
- Schneider, S. (2002). Can we estimate the likelihood of climatic changes at 2100? *Climatic Change*, 52, 441–451.
- Sills, J. (2010). Climate change and the integrity of science. *Science*, 328, 689–691.
- VW (2011). Challenges—For academia and society. Online: <http://www.volkswagenstiftung.de/funding/challenges.html?L=1> Accessed 10 January 2012.
- Wynne, Brian (1992). Sheep farming after chernobyl: A case study in communicating scientific information. In B. Lewenstein (Ed.), *When science meets public. Proceedings of a workshop organized by the AAAS* (pp. 43–67). Washington: Committee on Public Understanding of Science and Technology.