# Reply to Helen Longino*

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I am grateful to Helen Longino for her thoughtful and sympathetic reading of my book, *Science, Truth, and Democracy* (Kitcher 2001a). In this brief reply, I'll focus on three residual areas of disagreement, one very minor and two that are more interesting.

First, the small point: Longino assimilates the arguments for "modest realism" in the early chapters of my 2001 book to those I offered earlier in my 1993 book. There's some continuity, of course, but Chapter 2 does try to come to terms with issues that I failed to deal with adequately earlier, and Chapter 3 provides a more systematic approach to questions about underdetermination than I gave in 1993. The case for modest realism is further developed in Kitcher 2001b, and I hope it is now more convincing than the incomplete argument of my earlier book.

Second, an issue about my conception of well-ordered science, which Longino describes as offering my vision of "the governance of science." I *think* Longino recognizes an important distinction between an *ideal* at which the sciences aim and a *procedure* for working towards that ideal; but, since other readers have often confused the two, it's worth making the point explicit. Well-ordered science is intended as an ideal, and, though my 2001 book poses the problem of how we might work towards this ideal, I believe that solving this problem (giving a substantial account of the governance of science) requires a significant body of empirical knowledge (which I lack). Thus I provide evidence to show how scientific research currently seems to be organized in ways that lead it to diverge from my ideal, and invite a collaboration between philosophy of science and

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the social sciences to investigate how we might make up for some of the deficiencies.

Towards the end of her review, Longino points out, quite correctly, that scientific research is increasingly funded by the private sector, and she asks for the grounds under which "privatized research could be brought under the umbrella of well-orderedness." From my perspective this is a somewhat peculiar question. If one is firmly committed to well-ordered science as an ideal—and I think we should be committed to this ideal or to something close to it—then the privatization of research looks like a trend that we should want to resist strenuously. When research is subjected to market pressures, the inquiries undertaken are likely to be influenced by raw (untutored) preferences of the rich and powerful. It's hardly surprising that the outcome in biomedicine (for example) is an intensification of the 10/90 gap (in which 10% of the world's research resources are directed towards diseases that constitute 90% of the burden), and greater investment in attempts to reduce obesity or to halt hair loss than in finding a vaccine for tuberculosis. I take it as a strength of the ideal of well-ordered science that it treats the pitching of scientific research into the marketplace as a disaster.

My third disagreement with Longino is, I think, the most significant. She recognizes that a principal difference between my 1993 and my 2001 books lies in the adoption of pluralism, but she is concerned that the form of pluralism I espouse is too weak. She contrasts my view with a "more radical" pluralism in which the sciences offer us "overlapping systems, each meeting its own or a common standard of adequacy, but giving accounts of common clusters that are not consistent with each other." It seems to me to be important to draw a distinction here. Surely it's true that at many stages in the history of the sciences, there have been proposals for treating phenomena that answered to Longino's description; a well-studied example is the Bohr model of the atom, where the picture of electrons arrayed in shells and "jumping" between them was inconsistent with classical electromagnetic theory. In cases like this, the inconsistency is typically seen as a spur to further research; scientists want to amend one, or both, of the conflicting representations (theories, models, maps, or whatever) so as to arrive at a consistent account of the area of overlap.

The pluralism I propose consists of the following claims: (1) there are many different systems of representation for scientific use in understanding nature; (2) there is no coherent ideal of a complete account of nature; (3) the representations that conform to nature (the true statements, the accurate maps, the models that fit parts of the world in various respects to various degrees) are jointly consistent; (4) at any stage in the history of the sciences, it's likely that the representations *accepted* are *not* all consis-

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tent. My "conservatism" (in Longino's classification of my views) consists in my adoption of (3). As noted in the last paragraph, accepting (3) doesn't commit me to denying (4), which appears to me a commonplace.

In her book, Longino (2002) seems to want to illustrate the pluralism of which she approves by examples from theoretical biology (and the philosophy of biology). So far as I can tell, all the examples she offers are thoroughly in line with the pluralism I've just espoused. For example, to suggest that ant colonies are like organisms in some respects and like collections of organisms in others isn't to flout (3); similarly, to claim as C. Kenneth Waters (1991) does (and as Kim Sterelny and I (1988) do[1]) that selection processes can be modeled in different ways isn't to advance incompatible claims about the *real* units of selection—the idea of a "real unit of selection" is, the pluralist suggests, something from which we should emancipate ourselves.

I began my 2001 book with two polar images of the sciences precisely because I think that each has something importantly right (as well as something importantly wrong). The modest realism I defend is supposed to strip away the metaphysical excresences that have disfigured a traditional picture of the sciences, and to allow the issues about ethical, social, and political values to enter, not under the rubric of some confused metaphysical or epistemological proposal, but exactly as and where they should. Philosophers of science haven't had a good idiom for discussing questions about these values; and that has created muddy and muddled discussions in which the prevalent language of philosophy of science (a language that thrives on epistemological and metaphysical categories) has been used to take up those questions. Longino's hankering for a more radical pluralism simply seems to me more of the same. So far as I can see, there's no basis for wanting to go beyond my modest realism and "conservative" epistemology. The issues about values we both want to raise can be perspicuously and precisely posed in the idiom my 2001 book tries to offer.

Of course, I may be wrong about this, missing the need for further reform. If so, I hope very much that Helen Longino will offer us a clear example to demonstrate why a pluralism that adopts (3) hasn't gone far enough. So far as I can see, neither she nor anyone else has yet done so.

1. It's an interesting irony that Longino cites a number of recent defenders of pluralist positions in the philosophy of biology, many of whom make similar points to those advanced in my essays on genes (Kitcher 1982), genetic explanations (Kitcher 1984a, 1999), species (Kitcher 1984b), and the units of selection (Sterelny and Kitcher 1988); but she consistently views me as a defender of monism. Given the view of my 1993 book, that's quite reasonable; my essays in the philosophy of biology, however, are in line with the pluralism I'm advancing here and in my 2001 book.

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