AN INTERVIEW WITH HELEN LONGINO

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Each year The Dualist includes an interview with a significant modern figure in philosophy. This year, Helen Longino graciously agreed to answer questions posed by The Dualist and the Stanford Philosophy Department. Professor Longino's interests include the relations of social and cognitive values in the sciences, the philosophical character of feminist epistemologies, the development of a social approach to scientific knowledge, and the epistemological challenges of scientific pluralism. Her most recent book is The Fate of Knowledge, published in 2001 by Princeton University Press.

Longino: Before beginning to answer the questions, I want to thank the Dualist staff for initiating this interview and everyone who submitted questions. I have appreciated the opportunity to think about unanticipated issues, to clarify for myself as well as for questioners matters about which I have been unclear, and to reflect about aspects to which I should devote more thought. Thank you all for an invigorating exchange.

Michael Weisberg:

In a talk you gave at Stanford, you discussed feminist vs. traditional super-empirical theoretical virtues. You explained that for an empiricist like yourself, the choice of these virtues could not be settled with respect to some body of evidence. Thus, the decision about which set to adopt should have to do with the political consequences of adopting one or the other set. But couldn't an empiricist also believe that as a matter of fact, one set of theoretical virtues has tended to lead to more fruitful research programs, hence we should adopt it? If this line is open to an empiricist, do you think it is true that feminist theoretical virtues have tended to lead scientists to more fruitful research programs?

Longino:

Yes, an empiricist would certainly be tempted to claim and believe that one set of virtues could lead to more fruitful research programs than another. But there are two difficulties with such a line of argument. 1) There hasn't been a real test along these lines. The dominance of the traditional virtues has not been demonstrated to be a consequence of their greater fruitfulness if the non-traditional virtues haven't been given a chance. 2) What does fruitfulness amount to? If there are different understandings of what this virtue demands, then the question is no longer a simple empirical one. I think the feminist pragmatic virtues offer a different interpretation of fruitfulness than that offered by Kuhn, for example.

My own view is that which virtues promote more fruitful research programs depends 1) on our understanding of "fruitful", as I just suggested, and 2) on historically contingent features of an investigative community's situation, e.g. whether a research framework satisfying one set of virtues has exhausted itself given the investigative resources (instruments, mathematical techniques, etc.) available. If so, it might be time to try something different.

Michael Strevens:

According to the historians, the characteristic culture of science was created some time in the middle of the second millennium and has been refined ever since. Cultural contingency suggests the possibility of certain weaknesses: prejudice, false presupposition, incompetent institution-building. But cultural evolution suggests the possibility of certain strengths. Two questions:

a. *You have proposed that the traditional "non-empirical" virtues of theories, such as simplicity and conservatism, be augmented with different and to some extent competing virtues: heterogeneity, novelty, and so on. Might it not be the case that the set of traditional non-empirical virtues has evolved to contain the virtues that best promote the scientific enterprise? Perhaps it's a good thing, for example, that modestly talented scientists on the whole prefer conservative theories?*

Longino:

We philosophers talk as though the virtues actually characterize scientific practice. I suspect that philosophers more frequently invoke them than do scientists, and that when the latter do invoke them, they do so opportunistically or, like Steven Weinberg, when in metaphysical moods. How do we know, if there is characteristic culture of science, that it is not the characteristic culture of a form of science that serves some interests and not others? [See the question from the Dualist staff for a stronger version of this question.] Critics have argued that science in the late 20th and early 21st centuries has largely served the military and capital. Comparatively few resources are devoted to innocent contemplation of the universe. If our search for truth is inflected with values other than those of dominance and control, such critics might continue, a different culture of science might emerge. The very idea of the "modestly talented scientist" is an interesting one, suggesting that the society has certain productive expectations of scientific research.

b. *You are one of a number of philosophers advocating some degree of reform to the functioning of science. If scientific practice has evolved as an organic whole, the problem of unintended consequences looms: fixing one thing might break five others. How serious a problem do you think this will be? Are there features of scientific culture that make it more robust, and so safer to reform, than other social practices?*

Longino:

I think I am in some ways a conservative, in that I am moved by a vision of science that is disappearing in this era of big science and of science directed to productive (military or economic) ends. I think that science, as an institution, underwent a sea change in the 2nd half of the 20th century, and that the features that made it robust – openness, critical interaction, diversity of perspectives – are in danger of erasure as more and more scientific activity comes under the aegis of the private sector. If I were to advocate reforms, these would be intended to enhance, preserve, and restore those features (improving on the openness and diversity, of course). We always run the risk of unintended consequences, but the practice of science is subject to economic regimes that also have either unintended or generally unforeseen consequences. My view is that as a society we ought to think about what we want our sciences

to achieve and what we want the practice of science to achieve, and at least add those goals to the mix of selective forces shaping the future of scientific research.

Angela Potochnik:

In your article in The Aristotelian Society: Supplementary Volume, you argue that cognitive virtues beyond that of empirical adequacy play a role in science insofar as hypotheses are underdetermined by data. Further, you claim that the scientific community should be comprised of subcommunities, each defined by the (provisional) acceptance of some combination of traditional and/or alternative cognitive virtues. Science, then, would involve "a plurality of theoretical orientations that both make possible the elaboration of particular models of the phenomenal world and serve as resources for criticism of each other."

Yet I am confused about how a scientific community modeled after this plan would form a community at all. You say that the scientific metacommunity would/should be "characterized by mutual respect for divergent points of view." This is easy to imagine in the case of, e.g., different models of a single phenomenon proposed by various subcommunities. But what about two mutually incompatible hypotheses accounting for one set of data? In the case of true underdetermination, empirical adequacy does not and presumably will never be able to arbitrate between the two. The subcommunities' cognitive virtues are at odds, so these cannot settle the dispute. It seems the scientific community is fractured along the lines of commitment to the relevant, varying cognitive virtues. One can easily imagine situations in which such a fracture may be crippling to science.

In my view, the possibility for this sort of impasse may be a good argument against the desirability of separate, identifiable scientific subcommunities, in favor of a single, pluralistic set of community standards. I'd be interested to hear why, in your view, this is not the lesson that I should take away from these considerations.

Longino:

The possibility that there should be a pair of mutually incompatible hypotheses accounting equally well for a given set of data seems a genuine one, especially if we take into account that "equally well" may vary in meaning from context to context, and that the individual hypotheses may do better by some components of the data set than by others, but each overall do equally as well as the other. I don't see that this would be crippling to science as a whole, because it concerns a single case. The history of science shows us many instances of such impasse that do not impede other work, and that are eventually resolved, at least for the time being. Secondly, it might be a good thing if the impasse prevents a rush to judgment. It's possible to support, for example, multiple hypotheses about the health hazards of exposure to ionizing radiation on the basis of the data currently available. The different cognitive virtues pull in different directions. Some argue that which hypothesis we accept should reflect value judgments (about the relative costs of being wrong). At the very least, such cases demand reflection on the grounds on which we should accept or reject any of the hypotheses as the basis for policy or action.

With respect to the question in the final paragraph, these seem to me to be different ways of representing the same phenomenon. Even if there is a single pluralistic community (which is what I think there is in the largest sense – a global scientific community whose members answer to some shared and some not shared standards), subcommunities will form around subsets of the standards that seem most likely to satisfy specific cognitive aims. My picture of the relationships

among such subcommunities is that they will pairwise share one or more virtues, even though no single set characterizes the whole. Even how empirical adequacy is understood and weighted will vary depending on other virtues taken to be important.

The Dualist staff:

Contextual empiricism says it is possible to criticize the background assumptions of the scientific practices of different cultures. Objectivity, for instance, is described as a constitutive value derived from the goals of science. Thus, objectivity is a desirable property for any set of scientific practices to have, regardless of the contextual values of the particular scientific community in question. However, it seems like valuing objectivity is a social practice in itself, and one that resulted from a particular segment of a particular society at a particular time. If this is the case, why should the goals of science, such as objectivity, be epistemologically privileged above other culturally specific goals?

Longino:

When I think of objectivity (as a desired feature of inquiry), I think of objectivity as either a property of content, e.g. faithfulness of representation (truth or, as I prefer, conformation) or as a property of method, e.g. elimination, minimization, or control of the influence of subjective factors. Objectivity in both these senses has been a value for Western science, and in much Western epistemology (My view about objectivity has stressed the social, critical component of the method sense.). There may be other cultures characterized by knowledge practices in which objectivity, so understood, is not valued, (for example, cultures in which practical knowledge is valued over theoretical or descriptive knowledge). And there may be cultures in which knowledge is less important than tradition, community cohesion, or other values. The kind of intercommunity criticism I envision must connect with the cognitive aims of those whose beliefs or practices are the target of criticism. I don't think I've argued for privileging the goals of science over other culturally specific goals. It's worth thinking more about this issue. One might want to distinguish or think about the relation between knowledge and science, between community consensus and knowledge, and to identify cultures with a sufficiently different approach to knowledge than ours with respect to which to explore these distinctions.

David Hills:

For a long time you've maintained that science can succeed in the pursuit of such core cognitive aims as the production and maintenance of true belief only to the extent that it permits and cultivates durable disagreement about substantive scientific matters, disagreement which reflects and expresses the differing extra-scientific concerns and projects of individual participants in scientific inquiry. As you yourself point out, this conviction is one you share with the Mill of On Liberty. Mill himself goes on to maintain that inquiry's need for disagreement runs so deep, the collective collaborative pursuit of truth can't result in the gradual liquidation of the disagreement that makes it possible, can't result in the gradual forging of an ever broader consensus about what's true. This leaves him with a puzzle about what the attaining of truth in collective collaborative inquiry can amount to, given that one thing it can't amount to is durably shared true belief.

How do you view the shape of your affinities with and departures from Mill and Mill's arguments? Have your opinions about the strengths and weaknesses of this stretch of On Liberty

changed over the years? Recently, democratic theorists influenced by Rawls have begun to contend that normative inquiry must permit and cultivate durable disagreement. Sometimes they go on to conclude that we must stop conceiving of democratic debate as aiming at and ideally issuing in a community-wide consensus about important normative matters. Is this an accidental and superficial convergence, or do philosophers of science and political theorists have important things to teach one another hereabouts?

Longino:

I don't know enough about contemporary moral and political theory to pronounce with certainty about the nature of the convergence, but I am inclined to say that it is not accidental, but a result of philosophers abandoning the search for a priori norms and standards in favor of a somewhat more naturalized understanding of them, e.g. as immanent in and emerging from human practices. My own view is that collective or shared beliefs ratified by collective collaborative inquiry are variably durable – some (of limited scope, such as singular perceptual claims) are very durable, while others are less so. Beliefs change, come under challenge at different rates, so there's always a shared fabric, and a certain amount in contention, but what these are change. The point at which there were full (and correct) consensus about every conceivable point would be the end of inquiry, but, and here I agree with Mill, the end of knowledge, too.

David Hills:

You've wanted to say that the epistemic virtues and standards that feminists characteristically bring to scientific inquiry are distinctively <u>feminist</u> virtues and standards only insofar as (and only for as long as) they tend to advance core feminist projects. You've also wanted to say that at present, the core feminist epistemic project is that of <u>making gender visible</u>. I've got two questions about this.

a. *One of the feminist epistemic virtues figuring prominently on your list is complexity of relationship. A model or theory exhibits complexity of relationship to the extent that it portrays important causal influences as mutual and avoids portraying any one factor as "dominant or controlling." Yet it's easy to suppose that gender itself is a set of institutional arrangements and classificatory practices in which one of the two genders enjoys and maintains some kind of dominance, some kind of control, over the other. So it can look as if models in which gender itself figures will be models that don't exhibit the feminist virtue of complexity of relationship, despite the fact that the point of the feminist virtues is to make gender visible. Do we need to distinguish different kinds of dominance or control in order to sort this out?*

Longino:

I think to show gender *as* dominance and control is to show complexity of relationship, i.e. to show gender as gender, rather than treating the gendered dimension of human relationships as autonomous or independent processes. To suppose the dominant gender acts on its own, without the supportive labor of the dominated, would be to conceal gender. But I take the point that in the account of interactive or mutual causal influence in nature, I mean "dominant or controlling" in the sense of being the sole causal factor, whereas I take political domination to be more complex.

b. *You say that for the time being, the core feminist epistemic project is that of making gender visible. But you appear to suggest that feminism is such that as times change, other epistemic aims (and virtues) might come to be characteristic of it. How might this go? Could feminism become less centrally occupied with making gender visible and still be feminism?*

Longino:

What I've said is "For as long as and to the extent that their [the alternative cognitive virtues'] regulative role can promote the goal of revealing gender, and as long as revealing gender remains the primary goal of feminist inquiry, they can serve as norms or standards of feminist inquiry."

While I'm inclined to agree that feminism could not abandon its concern with gender and remain feminism, some feminists might come to argue that understanding gender alone is not sufficient to overcome gender oppression, that we must understand the co-constitution of gender with other axes of oppression or power asymmetry. Whether one sees this as deemphasizes gender is a matter of perspective, however. But, secondly, if gender is visible and women are still oppressed, then there must be something else going on, and it would be the aim of feminist inquiry to figure out what that is.

Elliott Sober:

Reichenbach and other logical empiricists thought there was an important distinction between the context of discovery and the context of justification. I'd be interested in your commenting on this distinction in connection with your views about the roles played by political values in science. It seems pretty clear that political values can be an important influence in the context of discovery -- for example, they may influence which questions a scientist chooses to ask. However, it is less obvious what role these values might play in the context of justification. Indeed, someone might contend that political values are not relevant to the question of how justified this or that theory is -- for example, how well supported it is by evidence, how probable it is, etc. What are your views here?

Longino:

It's quite correct that my discussion so far seems to have been focused on questions of justification. It looks, then, as though I am endorsing Reichenbach's distinction. I agree that there is a distinction, but I'm really trying to dislodge some old ideas about the independence of the context of justification from that of discovery. While, ideally, our acceptance of a theory or hypothesis would be dependent only on such questions as how well supported it is by evidence, etc., the logical structure of inquiry just doesn't permit this, because of the dependence of judgments of evidential relevance and support on background assumptions. These background assumptions are also operative in the context of discovery, i.e. the context of hypothesis and model formulation. Part of the work of a critical philosopher of science is to make these assumptions visible and to understand the systematic role they play in scientific inquiry. I've argued that one cannot formally exclude such assumptions without impoverishing science to an unacceptable degree. But it sometimes sounds as though I go further and endorse the reliance on particular background assumptions or values, ones that might be found in some feminist inquiry.

I don't endorse them as truth-conducive, however, but as (potentially) providing as good an account as one conforming to more orthodox assumptions or values. When two or more models or hypotheses each account for the same proportion of data, additional considerations will have to be employed to choose between them (if a choice is necessary). Those considerations may be metaphysical or pragmatic, and they may be politically or normatively valenced. My philosophical aim is only to make this aspect of inquiry visible and to draw (what I hope are) appropriate philosophical conclusions from it.

After thinking about the discussion after my talk at Stanford in January, I am inclined to think of the virtues as heuristics, rather than as justificatory, but the consequence of relying on one subset of heuristics rather than another is that the hypotheses that will be candidates for acceptance will be limited by the heuristics that made them salient and worth pursuing in the first place.

Peter Godfrey-Smith:

Imagine two possible futures for some particular area of science -- an important area, like developmental biology. In one scenario, a single theory has triumphed spectacularly. It has no serious competitors, and we can feel considerable confidence that we have understood the relevant part of the world very thoroughly. In the second scenario, there is no spectacularly successful theory, but a small group of different theories that each does quite well, and which are being debated and developed in an impressively democratic and fair-minded way. Traditional philosophies of science have tended to see the first outcome as preferable. After all, in this scenario we have reason to believe that we have succeeded in learning what the world is like. Some radicals, like Feyerabend, would always prefer the second situation. Where do you stand?

Longino:

David Hills' question about Mill raises similar issues. My suspicion is that the second alternative will turn out to be correct, but my considered philosophical view is that there may well be areas of science whose subject matter is such that human cognitive resources can develop a single, true, comprehensive theory. It is an empirical matter, not one to be determined a priori. It might even be the case that at the end of science, this would be the case for all subject matter, i.e. that there would be a single comprehensive account of all phenomena. This is not the case now, however. Now there are some areas where the combination of subject matter and resources is such that there is a small group of theories each of which explains some aspect of a given subject matter well, but none of which explains all. Until the end of inquiry (a mythical point at which all possible questions have been answered), the mere fact of having no competitors may be a function of a limitation on cognitive goals, political or institutional factors, or lack of imagination. The division of the world into physical structures and processes, chemical structures and processes, biological structures and processes, etc., is in part an artifact of the development of disciplines. In reality chemical, physical, and biological phenomena are not segregated from one another, so change in one area can disrupt a settled consensus in another. As long as one has not reached the end of inquiry with a single, true, comprehensive theory, one cannot be sure that a single theory triumphs because there can and will be no rivals or triumphs because of factors that may be subject to challenge and change. So, I agree with Feyerabend that while we continue to pursue inquiry, pluralism is the best way to avoid false complacency, and with Mill that it is the best way to keep our knowledge alive.

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