

Claiming to Know: Objectivity, Truth, and Science

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I shall begin by thanking Jon Levisohn for a very thought-provoking essay. His setting of the more-or-less arcane issues of philosophy of science squarely in the center of an immediately pressing issue of global public health is a move I applaud — the question that I find the most frustrating to attempt to answer is the ever popular, Does any of this philosophy stuff really *matter*? and here we have been given a very nice demonstration that the answer is, “*Yes*.” It matters. And as Levisohn aptly points out, it matters particularly, when it comes to problems of education.

I find myself largely in agreement with the general thesis Levisohn has advanced, which I take to be, that there are certain fundamental problems that are integral to Helen Longino’s explication of the nature and norms of scientific practice. I also agree that these problems are serious enough to undermine Longino’s critique deeply. So, I will begin my response by reprising Levisohn’s argument, pointing out several aspects that I thought were particularly compelling, and I will offer a few additional observations, along the same general lines. And then, of course, it would not really be a “*response*” if there were not at least a few points of difference along the way.

Levisohn explicates Longino’s “contextual empiricism” by setting out and examining the three central elements that Longino brings together. The first is “traditional empiricism,” and one cannot help but wonder why Longino would not have built upon a more sophisticated model of contemporary scientific practice. But Levisohn is right, she does not. “Observations” are supposed, on this model, to be simple things, that readily and clearly confirm or disconfirm one’s theory. Of course, in practice, observations are not so simple, and one’s data are very seldom unequivocal in their pronouncements.

Levisohn points out that Longino, drawing on philosophy of science, brings in two theses that complicate the traditional empiricist picture of inquiry, the first, that data are not theory-independent, the second, that theories are underdetermined by data. Longino concludes that the judgments that result from a scientific inquiry are shaped as much by the “background assumptions” that the inquirers bring into the study as by the actual state of affairs that is the focus of the study.

If this is in fact the case, it would pretty much undermine any pretensions to knowledge, unless those background assumptions themselves happened to be true. This would be surprising, however, since those assumptions are warranted by nothing, except prior “inquiries” which were themselves shaped by other assumptions, and so on, all down the line. As the saying goes, there is nothing but assumptions, all the way down.

But Longino, apparently, wants to “save” knowledge, to show that scientific inquiry is after all objective, despite the morass of unfounded assumptions on which

it is based. Feminist theory provides Longino with a key insight: appearances to the contrary notwithstanding, human beings are not isolated, but, rather, are *social* beings. Scientific inquiries occur in a social context, and it is the mutual, interactive, social appraisal of one another's work that will compensate, somehow, for all of those implicit assumptions that are necessarily built into everyone's research.

Objectivity, Longino concludes, is not a matter of having one's scientific judgments checked, in the sense of "constrained," by nature, but is instead a matter of having them checked, in the sense of "evaluated," by one's fellows. Scientific knowledge is *social* knowledge, and the quality of a piece of research is a function of the quality of the social unit, the community. Levisohn terms this "the magic bullet," and that characterization I believe is apt.

Levisohn points out a fundamental problem with Longino's position: that a "background" assumption is likely to be invisible to anyone holding it, and that individuals constituting a "community" would be likely to hold the same set of background assumptions in common. So, in a particular community, it would be very likely that nobody would be able to see, in order to point out, the community's background assumptions, never mind being able to see that those assumptions just might be false. The upshot is that only a certain sort of community will do, and Longino sets out criteria that communities must meet in order to guarantee openness, and hence effective criticism and "objectivity."

Levisohn critiques Longino's reconceptualization of objectivity, arguing that it is not the quality of the scientific community that is chiefly at issue when scientific work is evaluated. Rather, what is of concern is, in Levisohn's terms, "the objective validity" of the scientific theory that is the result of the inquiry. I think Levisohn is right about this. However, a small critique: Levisohn speaks of "objective validity." He speaks of a theory's being "correct." He even speaks of a theory's being "correct because at some level, this is the way the *world* is." I think Levisohn should come right out and say that what is at issue here is "truth." The point of seeking out practices, and instruments, and analyses that increase the objectivity of an inquiry, or that increase, if one insists, a community's objectivity, is that such practices are conducive to the development of true statements about the way the world is. They are conducive, over an extended period of time, to the development of theories that truly represent, that is, that represent the world "as it is." (Now, I have heard, of course, that such realism is supposed to be passé — but I believe, along with Longino, that pointing out pervasive and yet nevertheless moribund and ultimately destructive background assumptions is a fine thing to do.)

There is one point in Levisohn's critique that I found troublesome, although it was not a major part of his thesis. This was Levisohn's rejection of the very idea that science could conceivably "settle" an empirical question, and could do so conclusively. Now, this may seem radical, or simply simplistic, but I think the claim that no empirical question can *ever* be settled is false. Although I would agree that not even the most successful of inquirers ought to be considered "infallible," there are, I believe it is safe to say, *some* empirical questions that actually have been settled conclusively. An example: Although it is very hard at times to believe, the fact is, that fruit-flies do not in fact arise by a process of "spontaneous generation." It simply

does not happen; the world does not in fact work in such a way that it could happen. Blood circulates. The sun only just *looks* like it is circling the earth; but in fact it is the other way around.

The general point here is that, from the fact that some questions (or even a great many questions), once thought to have been conclusively settled, turned out not to have been settled at all, it does not follow that no empirical question, simply because it is empirical, can *ever be* conclusively settled. This is the simple thesis that there is in fact a way things in the world are, that true statements about that “way” are not only possible, but can at times be asserted with an unimpeachable warrant. This is to say, in short, that knowledge about empirical matters is possible.

So, to return to Levisohn’s principal questions: What do we teach when we set out to teach science? What is “in,” what is “out,” and how do we go about making those evaluative judgments? I think the answer lies, fundamentally, in the application of the internal values that, Longino correctly points out, are constitutive of scientific inquiry. The contextual values that do creep in, and sometimes overwhelm the constitutive, are to be weeded out, as much as is humanly possible, as soon as the intrusion is noticed. Certainly, given the social nature of humans, this will happen in social context. And it cannot happen, true, unless the relevant community is open to the criticism of its current judgments. But more important, it cannot happen unless the community is committed to the constitutive values of science in the first place. And maintaining and acting upon that commitment is going to require that the community be “open” to only certain kinds of critiques, namely, to those critiques arising out of a similar commitment to the same constitutive values. So the preliminary judgment of the merits of a critique is based upon an assessment of the processes that generated the critique. Critiques arising from a process of “divine revelation,” for example, need not be accorded the same serious consideration that must be accorded to critiques inspired by scientific inquiry.

We can hope that the scientific nay-sayers, and perhaps groundbreakers, will find themselves in “good” communities, in communities that are committed to listening to “diverse voices,” even when what is being said is not particularly welcome at the time. But that commitment is not, primarily, to “diversity.” Rather, it is a commitment to the constitutive values that are being defended, in such a case, by the dissenting camp.

So, what of the mainstream AIDS research community? Its objectivity is a matter of the degree of its adherence to the constitutive norms of science, as is the objectivity of the dissenting “non-mainstream” community. To the extent that the non-mainstream view is the result of credible scientific research practices, its exclusion would be an inappropriate exercise of political power. To the extent that it is not a result of such practices, its *inclusion* would be an inappropriate deference to an inaccurate and substandard *modus operandi* of fixing belief. So, my answer to Levisohn’s questions would be: political commitment to establishing public policy solely on the basis of the best warranted beliefs available at the time is not an abuse of power, but is, rather, a critically important social obligation. And I think the same requirement holds in a publicly provided system of education.