Social Epistemology And Political Critique

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I.

Political critiques of science are all the rage. It is alleged by Marxists, feminists, and other thinkers that our science and other epistemic enterprises are biased in favor of the powerful, are instruments of oppression, or are otherwise politicized.[1] It is further alleged that these political implications are obscured by a false theory of knowledge acquisition, according to which it is individual humans, theoretically completely independent of one another, who form beliefs and acquire knowledge. They go on to say that the proper corrective for this misunderstanding of knowledge acquisition is understanding the social nature of knowledge production. Such an understanding will not only help to identify sources of bias, but will also expose the political motivations of the bias, opening the epistemic practice in question to criticism on political grounds.

By "the social nature of knowledge" these theorists typically mean the feature of knowledge acquisition that makes it something undertaken by large groups of people, not isolatable individuals. In other words, they all claim that, at least for some types of knowledge, the only reason an individual can gain knowledge at all is because of something prior that the social groups in which the individual is embedded have been, or have done. Knowledge production, at least in some important realms, cannot be undertaken by an isolated individual.

Though all these thinkers agree that knowledge is a social enterprise, and many think it is essentially so, their agreement conceals a wide variety of views.[2] Having noted that some social group must be or do something before any individuals in it can acquire knowledge, there are a great many different things we might claim the group must be or do. In other words, there are a great many different ways in which the social group can be thought to be prior to the individual; there are lots of different social epistemologies. Not all social epistemologies justify all political critiques. It is the purpose of this paper first to sketch out a taxonomy of social epistemologies and second to defend one such class of views as both probably true and adequate to accommodate the view that our epistemic practices have been politicized, and so stand in need of reform.

II.

I will start from a general and sketchy account of knowledge. These things I take to be entailed by a true claim of the form "S knows that p": 1) that p is the

case; 2) that S believes that p; and 3) that S's belief that p is warranted (or justified, or based on an adequate ground, or some such). Note that I do not offer this as determining the extension of the term 'knowledge,' never mind as an analysis of the concept of knowledge; these are simply features or aspects of knowledge. Each of these features is a place where knowledge might be social. First, it might be S herself is a social construct. Included in this category are not only post-modern views according to which selves only exist insofar as they are constructed by social groups, but also views according to which important kinds of knowledge are most correctly expressed with plural subjects. For example, accounts of group belief count as projects in social epistemology. If it makes sense to talk of things that some group believes, with or without reducing the group's belief to the beliefs of individuals, then there is work for social epistemology to do in accounting for it. Second, what it is that makes p the case might be social facts. Again, this kind of view is frequently motivated by post-modern constructivist views of ontology, but it is not confined to that. Even those who believe that reality is mostly found, not made, can admit that important parts of it are made. They can even admit that parts of it are made by our thinking and saying certain things (think of what distinguishes a diploma from a piece of paper). It might well be that important facets of scientific knowledge have to do with such objects. Third, beliefs as mental states might be socially constructed. This kind of view also can be motivated by concerns about the post-modern condition, but it can also be motivated by arguments for externalism about mental contents. If part of what it is for your mental state to have the content it has is for the world outside your head to be a certain way, then as long as parts of the world outside your head can depend on social forces, so can your mental contents. Fourth, warrant (or justification, or whatever) might be socially constituted. This sort of view can be motivated by relativist views of epistemic norms (e.g.., the view that "what's justified is what my culture says is justified"), but it might also be that justification is partly a contextual matter without it being totally relative or "up for grabs." It might also be that whereas it is an individual matter whether a given belief is justified, a social group is necessary to provide the individual with justifiers, in the form of background information, or other resources required for undertaking an investigation. There will also be a stunning variety of combinations of these views. In addition to that, any one of these views might be held in a restricted or an unrestricted form (most beliefs are social constructs, some objects are epiphenomena of social practices, some kinds of justification depend on social facts, etc.).

Criticisms of social epistemology frequently depend on not distinguishing among these possibilities.[3] Instead of recognizing the bewildering variety of views that count as social epistemologies (because they all make room for social facts as crucial in the acquisition of knowledge), critics frequently talk as if all social epis-

temologists are post-modernists, believers in decentered selves and socially constituted realities, and so are easily dismissed, along with their political observations. The lesson of this paper is that it is not so easy to get rid of social epistemology, and so not so easy to dismiss political critiques of knowledge production.

III.

The view I intend to defend here is the following: As a minimum, social groups and their activities provide a necessary condition for large and important segments of the knowledge of any member of that group; they do this by providing justifiers, information upon which knowledge is based. I do not claim that all knowledge has such social necessary conditions, or that those conditions are logically or metaphysically necessary, or that the same must be true of any finite intelligences. I am content to claim that, as a matter of fact, we are so constituted that we can't make any real headway in our knowledge acquisition efforts without the backing of a substantial social framework. The role of this social framework makes it possible for knowledge-acquisition enterprises to acquire a political slant.

I should also not be understood as denying any of the other views in the taxonomy above. For example, it is certainly true that some objects exist only because of the cognitive activities of social groups (institutions and games, for example). It is also certainly true that there are beliefs that are held by groups; in some cases, those group beliefs can be understood as some combination of individual beliefs, but probably not all can be so reduced. Moreover, it is not at all unlikely that externalism about mental content is true, and so the very content of beliefs that are candidates for knowledge may often be determined by the behavior of social groups. Each of these other kinds of social epistemology makes room for political critiques of epistemic practices, insofar as it is possible for political forces to bias precisely those parts of the enterprise where the social enters in. But rather than argue for all of that, I wish to pursue the smaller claim that most of our epistemic practices rely on social groups to provide justifiers in the form of background information.

The argument is really quite simple; it involves showing that without the help of other people, we couldn't come to know very much at all, so that our choice is between accepting the epistemic help of others and almost total skepticism. Consider how much you know that you discovered entirely by yourself. Allowing for normally functioning cognitive equipment, and a full stock of concepts, you know a great deal about your physical environment, your inner states, and your past experiences. But do you know that I am a professor of philosophy? Many people agree that I am, but you don't know that without relying on somebody else's word. Do you know that you are in one of fifty states governed by a federal government in Washington, D.C.? Do you know the phone book is not an elaborate hoax? Do

you know there is such a place as Bosnia? Not without believing what you have read or heard. Your most mundane beliefs are tied together in a complicated network with information that you have gathered from other people, trusting that they are giving you the truth.

It's not trust, you might reply, because I have good reason to trust other people, by and large. Relying on my own cognitive abilities, I discover that when people talk about geography, history, and the like, they are usually telling the truth. Since I have discovered them to be reliable sources of information, I may rely on them without further ado, just as I may rely on a proven thermometer without having to recheck it all the time. Unfortunately, that just can't be right. For one thing, if you're like me, you have done practically no checking to find out whether mapmakers lie. The same goes for history books, science texts, telephone books, news broadcasts, and a host of other sources of information I rely on routinely. If I were to set out to verify personally every claim in a phone book, or even a sizable sample, I would rightly be thought a lunatic. Imagine the task of confirming even a representative sampling of claims in a freshman chemistry text. Even for the scientists whose business it is to be able to confirm these things, they frequently rely on instruments calibrated by other people, and must make use of technicians and assistants, whose observations they rely on. Even for the smartest of us, the help of other people is inescapable.[4]

It would be a mistake to think that the social contribution to our individual knowledge stops there. To think so would be to accept a hopelessly inductivist view of science. It is true that we need observations besides our own to support theories, but it is also true that theories are supported by more than mere observation. Any set of observations is consistent with an infinite variety of different theories. Consequently, when we decide between empirically equivalent theories, we are always deciding on the basis of more than observational data. One way we narrow down the number of contender theories is by invoking nonepistemic values, like simplicity, predictive fruitfulness, and elegance. When scientists are faced with mutually inconsistent but equally empirically adequate theories, they frequently choose which to pursue in further research on grounds other than likelihood of truth. It may be, as Quine (in some moods) says, that two theories that are completely empirically equivalent – that is, entail all the same observations – are mere verbal variants on one another. But we are not at liberty to say that about theories that are equivalent only in that they entail the same evaluations for observation sentences we already know the truth-values of, or ones we are likely to know soon. This limited kind of empirical equivalence is the pressing problem, and equally intractable if we limit ourselves to differences between the theories that bear on how likely they are to be true. Similarly, and also because of the underdetermination of theory by data, scientists judge particular theories in the light of what they al-

ready believe. If a scientist earlier in this century had a firm belief that all the laws of nature must be deterministic in character, not statistical, then she would be inclined to reject the standard understandings of quantum theory and accept hidden-variable versions of the theory. Other scientists accept the standard understandings of quantum theory as superior to hidden-variable theories (on the grounds of simplicity) and therefore reject the view that all the laws of nature must be deterministic. It is probably right to reject the deterministic view of law rather than the value of simplicity, but the point is, whether any given instance is right or wrong, these kinds of considerations have a role to play in theory choice. Insofar as we acquire from our communities our beliefs about what non-epistemic values are the right ones for our theories to embody, and also our auxiliary assumptions about how the world works, our social settings have an effect on what directions our science takes.[5] And there is more to science than theory selection. What science gets done is partly a function of what previous scientists have already done and what presently employed scientists would like to see done. Scientists are partly hired, promoted, and otherwise evaluated on the strength of how interesting the problems are that they are pursuing, so what we find out about the world is in part a function of what presently employed scientists find interesting. The same goes for publication and replication of results. If no scientific society or journal finds your work important or interesting, it won't get published, and so the general public will never find out about it. Evolutionary biology had to wait decades for Gregor Mendel's groundbreaking work because it languished in a second-rate journal that nobody was reading. Even if a paper on a problem considered marginal by the majority makes it to publication, if the scientific community doesn't pick up on it, discuss it, and expand on it, it vanishes into obscurity.

IV.

So many important areas of knowledge we acquire only with the help of our communities; they provide us with background information, non-epistemic theoretical values, auxiliary assumptions, resources, and public ratification. But what has all this got to do with politics? To put it simply, if our community has a political preference, it can express it in what science it supports with funding for research. The federal government, in the hands of conservatives, is much less likely to spend money on research in drug treatment as an alternative to punishment, studies of the harmful effects of religion on communities, fetal tissue research, birth control device trials, and the like. If our society considers one kind of person inferior to another kind of person, then scientists will bring that prejudice as an auxiliary assumption to their research. They will tend to accept theories that support that claim, in spite of flaws, and be skeptical of theories that challenge it, in spite of support by data.[6] If our society considers the ailments of women uninteresting, then scientists who insist on studying PMS will lose funding, respect, promotions, and employment opportunities. They will also find it hard to publish their results, no matter how well-designed their studies are. In all these ways, the communities of which we are part can help some projects and hinder others, all because we need those communities' help to do our research. If science has been biased in the past, then the research projects against which it is biased will have less of a history. In other words, there will be less previous work to draw on, so the work will be correspondingly more difficult. Research tools, like indices, can also be skewed against some research projects. If there is no category in *Psychological Abstracts* for Abused Woman Syndrome, then people who want to do research on that have a harder job tracking down what previous work there is.

What about non-epistemic theoretical values? It's hard to see how what values we use to judge theories could be employed to political ends. Nevertheless, it almost certainly happens that theories are chosen over other theories for reasons that have nothing to do with whether they are likely to be true, and people with different political aims would accept different theories. Suppose for example that the two theories under consideration are indistinguishable with respect to empirical consequences, simplicity, and whatever other uncontroversial theoretical virtue you like. If the first theory is true, then a commercial application that will lead to gigantic profits will follow. If the second theory is true, then there is no readily exploitable commercial result, but there is an easy application to some problem in the third world. People in science for the sake of technological application and profit will tend to accept and pursue the first theory, while people who see the role of science as one of improving quality of life for people will tend to accept the second theory. The theoretical value in this kind of case is tied to what the scientist takes to be the primary role of science in society, what "good science" is supposed to do. Of course, if those theories really are alternatives, then one or both will be wrong, but it may take quite a bit of time to find out which, and in the meantime, science will continue in the direction it has taken because of a non-epistemic value choice. The upshot of all this is that science, because of its essentially social nature, can be and frequently is bent to political purposes. Since this is so, political critiques of scientific enterprises can be apt.

Notes

1. See e.g. Sandra Harding, *The Science Question in Feminism*, Ithaca, NY: Cornell University Press, 1986; Helen Longino, *Science as Social Knowledge*, Princeton: Princeton University Press, 1990; and Hilary Rose and Steven Rose, *The Radicalization of Science*, London: Macmillan, 1976.

2. See my "Feminist Epistemology and the Extent of the Social," *Hypatia* 10 (1995), 85-98, for an account of this variety in feminist epistemology.

3. For example, see Susan Haack's "Epistemological Reflections of an Old Feminist," *Reason Papers* 18 (1993), 31-43. Haack argues that since the most radical proposals in feminist epistemology don't make sense, there is no such thing as feminist epistemology. She says this even while granting many of the points that less ambitious feminist epistemologists make.

4. The preceding two paragraphs are a rough precis of the argument in my "Why I Know About as Much as You: a Reply to Hardwig," in *the Journal of Philosophy* 90 (1993), 260-270.

5. Longino, *op. cit.*, pp 40-48, describes the role of auxiliary assumptions very nicely.

6. For two examples of cases in which this sort of thing seems to have happened, see Nancy Tuana's "The Weaker Seed: The Sexist Bias of Reproductive Theory," in her *Feminism and Science* Indiana University Press, 1989, and Stephen Jay Gould, *The MIsmeasure of Man*, New York: Norton and Co., 1981. Longino, *op. cit.*, is also full of examples.