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Epistemology in the face of the strong sociology of knowledge: a reply to Maffie

MAURICIO SUÁREZ

ABSTRACT

James Maffie claims that *weak continuity reliabilism* is compatible with the principles, as well as the insights, of the Strong Programme in the Sociology of Knowledge (SPSK). There are three possible readings of *weak continuity reliabilism*: I argue that the first two are unsound, while the third is actually inconsistent with the principles of SPSK. SPSK is instead compatible with an *identicalist* epistemology, one that does not aim to distinguish scientific epistemology from our everyday epistemic practice.

1 MAFFIE'S DILEMMA

The birth and emergence of the Strong Programme in the Sociology of Knowledge (SPSK) in the 1970s have often been seen to carry a death certificate for traditional epistemology. In fact the funeral march had been played in philosophical quarters for years. Quine's naturalized epistemology programme often was presented as a golden death-bed for epistemology; golden because it aimed to retain the corpse, and re-inflate it with some new air – the scientific air of the psychology of perception; but deadly nonetheless. And well before Quine wrote, John Dewey rampaged against *the spectator theory of knowledge*, a term that he used to describe traditional British

empiricist epistemology. What seemed significantly different in Bloor's proposal was the strong programme's refusal to acknowledge any role for any form of epistemology, traditional or not, in the analysis of science. The presumption was that other disciplines, headed by sociology, could take over epistemology's role in providing the explanation of the emergence of scientific belief, perceptual or otherwise.

Were these dismissals of epistemology premature? Is traditional epistemology alive and well among us, in spite of the successes of recent sociology of knowledge in explaining the details of real cases of knowledge acquisition and justification in science? James Maffie provides a sustained defence of epistemology in the face of the strong programme. He investigates, and carefully lays out, the options that remain open to an epistemologist who would like to account for the explanatory successes of SPSK-inspired sociology of scientific knowledge, without surrendering the position entirely; i.e. without abandoning epistemology's traditional role in explaining the justification of belief. Broadly speaking, there are two ways to go about it, which Maffie refers to as *strong continuity* and *weak continuity* between epistemology and the natural and social sciences. He then argues for weak continuity as a form of naturalized epistemology that is able to accommodate not just the insights, but also the principles of SPSK, while remaining a firm, normative and detached form of inquiry.

In the aftermath of the science wars, Maffie's conciliatory attempt to bridge the gap between the disciplines is timely and very valuable. But much as I like the spirit of his work, I disagree with the conclusions. More specifically, I do not think that Maffie's preferred option is a genuine option. There are three different readings of weak continuity epistemology. I argue that the only one that is sound does not do the work that Maffie wants it to do: in at least one way it is not compatible with SPSK. Maffie's bridge turns out not to be a bridge towards SPSK-inspired studies of the practice of science, but one that leads instead back towards the type of traditional epistemology that he himself is so keen to avoid. A better bridge to SPSK would be provided by an *identicalist* epistemology.

2 EPISTEMOLOGY AS DISTINCT FROM SCIENCE

Maffie lists six different assumptions that separate science from traditional foundationalist epistemology (p. 23). These assumptions are intended to capture the traditional 'first philosophy' aspect of epistemology: the primacy of epistemology over science characteristic of foundationalist attempts to ground science. Maffie defines the notion of strong continuity between epistemology and science as the denial of all six assumptions. According to strong continuity there is no distinction between science and epistemology, either

because there is no epistemology at all (*'eliminativism'*), or because epistemology lies within science, in the manifold form of the various procedures and methods that scientists employ to justify their conclusions to each other (*'identicism'*). And hence there is no way that epistemology can ground science, any more than science itself can.

Maffie prefers what he refers to as weak continuity, which denies only the latter five assumptions, while adhering to the first one. We may call it Assumption One: 'epistemology employs evidential concepts, norms and goals distinct from those of science' (p. 23). That is, weak continuity epistemology (or, anthroepistemology, as Maffie prefers to call it) is a distinct cognitive enterprise, with concepts, rules and goals different from those of science. So, for instance, while scientists' cognitive evaluations are 'relative to such ends as power, utility, empirical adequacy and simplicity', the cognitive ends of anthroepistemology are 'maximizing truth and minimizing falsehood' (p. 25). Hence Maffie admits that the aims of science are open, indeterminate and plural, and that more often than not the search for truth is not the driving force; but truth nonetheless is what anthroepistemologists aim for.

And yet, as Maffie claims, anthroepistemology is in no way 'privileged' or 'prior'. It cannot provide epistemic foundations for the sciences. We may then ask: what is the function of anthroepistemology? It isn't to provide us with knowledge of the natural world, for that would assimilate its concepts, norms and goals to those of the natural sciences. And it isn't to describe our social, political or economic relations to each other, for that would make anthroepistemology a social science. Although Maffie doesn't explicitly say, two alternatives fit his other pronouncements well (there is a third alternative that doesn't fit these pronouncements at all, but which I think is the right one, and it is discussed it in the last section): (1) anthroepistemology provides the rules of the game to achieve more mundane, ordinary knowledge, which is distinct from scientific knowledge; (2) anthroepistemology is a meta-level reflection on cognition, scientific or otherwise, that provides us with the general principles that underlie our epistemic practices at large (in science and elsewhere). These are, I think, just what Maffie has in mind when he refers to anthroepistemology as, respectively, a *substantive* doctrine, and a *meta-level* one.

However, neither alternative is a genuine possibility, in light of Maffie's own weak continuity constraints. The first ('anthroepistemology as folk epistemology') assumes a distinction in kind between our scientific and our everyday epistemic practices which, according to Assumption One, would have to have distinct norms, concepts and goals. So, for instance, the aims of science would be reliability, utility, simplicity, the exertion of human power over nature, etc.; while the unique aim of ordinary epistemology would, on this account, be the uninterested pursuit of truth.

But the distinction, I think, bears no close scrutiny: if anthroepistemology merely describes our everyday cognitive and epistemic practices, then it invariably describes science, for our everyday epistemic practices *are* part of science. Working scientists do not, in entering their offices and laboratories, leave their everyday epistemic practices at the doorstep; on the contrary they bring along the epistemology they practise at home, and they continue to build on it. In doing science, scientists employ the same epistemology that they employ in their many other everyday activities – including, not exhaustively, their political lives. This is one of the main philosophical insights to be learnt from SPSK-inspired sociology of knowledge. And it has indeed been learnt: the philosophers' search for the holy grail of the universal scientific method has subdued progressively, as sociologists and historians of science have continued to accumulate evidence in front of us that scientists do not possess privileged access to any special epistemology. Paul Feyerabend's attack on method signalled just one polemical and magisterial landmark in this progressive, cumulative shift.

Thus, to paraphrase Arthur Fine, the epistemology of science is a refinement of everyday epistemology.¹ If the aim of science is not truth, the search for truth cannot be driving our everyday epistemological practice either. An entire philosophical tradition, American pragmatism, underwrites this insight; but it is an insight that can be gained independently, by means of detailed studies of scientific practice. If Fine and the pragmatists are right, it is hardly surprising that on close inspection, our epistemic evaluation of scientific claims is no different in kind from that of our more mundane, everyday claims. *A fortiori*, anthroepistemology cannot be distinct from science.

The second alternative ('anthroepistemology as a meta-discipline') does not assume this problematic distinction between scientific and everyday epistemology, but it requires a level of generality in our epistemic practices (scientific or otherwise) that I think is unwarranted by a close inspection of the patterns of inquiry. This is a point that can already be made by considering folk cognition and inquiry across diverse cultural and social groups. There is no general set of epistemic norms that would be able to subsume the whole range of human epistemic practice at all times. What is taken as requiring evidence, and what is taken as providing it, depends on subject, community and epoch.

We may just as well restrict ourselves to science, for the same point applies. It can hardly be said that there is any stable set of concepts, goals or rules in common between any pair of scientific disciplines. For a start disciplines come and go, in contingent fashion, and the boundaries of science are vague and ever changing. Even at any particular time, and given any pair of well-delimited scientific disciplines, no matter how closely related, it is difficult to find a set of epistemic concepts, rules and goals that are shared. For example,

I cannot think of a single evidential procedure that astrophysical cosmology and high temperature superconductivity share in common. Evidence in cosmology takes the form of spectroscopical analyses of starlight, statistical records of the positions of objects on photographic plates, the recording of measurements of the microwave background radiation by satellites like COBE, etc. In high temperature superconductivity, evidence is provided by measurements of electric currents and magnetic field strengths in the regions surrounding the superconducting material, measurements made by means of inductors, voltmeters, and other magnetoelectric devices. These procedures are as different as they could be, and a technician superbly skilled at assessing evidence by means of the one would be, without adequate training, entirely incompetent in the other.

Maffie's claim would be more sophisticated. It would be something like this: there is a set of abstract epistemic principles that underlie and validate all of science's evidential procedures. But there seems to be no good reason to believe that such abstract principles exist; on the contrary, there is increasing evidence pointing in the opposite direction. No philosopher has yet been able to show to everyone's (or perhaps anyone's) satisfaction that their favourite set of abstract evaluative principles can accommodate and explain a large enough variety of (not to mention the entirety of) the evidential procedures found in science. Arguably the best scheme we have so far is Bayesianism, which turns out not to be particularly felicitous in this regard.² With each failure, the suspicion grows that there are no such evaluative principles, and that the search for a unique set of universal abstract epistemic principles is a chimera. Finding evidential connections between disciplines is an arduous task; one that can be achieved only by those who possess the proper training and skills, i.e. the scientists themselves, operating *within* science. Willis Lamb found that the new techniques of microwave spectroscopy could be employed to provide evidence for unexpected claims about the fine-structure of the hydrogen atom. Such rare and precious links are hard to come by – they are the stuff that wins Nobel prizes. Scientists' epistemological practices appear much richer, more complex and plural than would be allowed by any monistic philosophical theory of evidence.

Can philosophers derive any epistemological lessons from such plural science? The answer is yes – as long as we keep our gaze close to the actual practice of science. Even then, the lessons are likely to be local and contextual; but this may be as much as the evidence grants us. On the other hand anthroepistemology operates from outside science, and is keen to make general recommendations: 'when assessing the epistemic credentials of belief, the relevant question is whether the process causally responsible for belief is reliable' (p. 32). This is a rather empty recommendation, when the specific judgements of reliability can be made only by the scientists themselves, and are likely to vary greatly from discipline to discipline. I take it that this

plurality and irreducibility of scientific judgement are yet another lesson to be drawn, at least in part, from the SPSK-inspired social history of science of the last two decades.

3 CONTEXTUALISM

What about the traditional enterprise of philosophical epistemology? Is there room within this detailed study of the nature of scientific practice for such philosophical concepts as justification and warrant, internalism and externalism, truth-conditions and assertibility-conditions, etc.? Bloor's SPSK denied the need to make any such room for mainstream philosophical epistemology; and this seems confirmed by the many detailed and careful studies of the practice of scientific belief formation that SPSK has inspired – studies where the concepts and theories of philosophical epistemology rarely ever make any significant appearance, let alone do any of the relevant explanatory work.³ On the other hand Maffie adjudicates descriptive and normative roles in the study of science for epistemology, albeit in the non-foundationalist guise of *weak continuity reliabilism*.

On this issue, I side with the social historians. I see no role for philosophical epistemology to play in a study of the epistemic practices of science. (This is not to say that there is no contribution for philosophers to make in such studies; but then, by and large, philosophers of science these days do not practise philosophical epistemology.) I am inclined instead towards what Maffie calls '*strong continuity*'; and in particular '*identicism*': the view that there is an epistemology that we exercise in science as well as in everyday life, but that it is as varied and diverse as the evidential methods of science and of our everyday lives. For there aren't any epistemic principles of any generality that anthropologists may uncover that science itself would not uncover in the course of its development. So, if we want to do the work, we have to get in there.⁴

What then have traditional epistemologists been doing for the last 300 years or so? What is the function of traditional philosophical epistemology? This is always a troubling question for anyone who holds '*identicism*' or '*eliminativism*', and I cannot claim to have a comprehensive answer. Let me here try to sketch an attempt, inspired by Michael Williams' contextualist critique of traditional philosophical epistemology.⁵ Williams takes philosophical epistemology to be operating within the very special context of inquiry where there are no *hinge* propositions: there are no propositions that lie beyond the reach of epistemic evaluation, and hence no presuppositions that can articulate a framework relative to which evidence may be assessed. In other words, in epistemology, since Descartes, 'everything is up for grabs': every possible

proposition is seen as requiring evidential justification. (Not surprisingly perhaps, epistemology leads to global scepticism.)

One could then think of *anthroepistemology* as operating within this context. This would make it distinct from science in goals, norms and concepts: Assumption One would be satisfied. And there is no denying that this type of epistemology is a worthy and valuable enterprise in its own right. But notice that it would cease to be a study of scientific cognition. For in Williams' hands, philosophical epistemology is not a scientific investigation of the epistemic credentials of scientific belief at all, but becomes an investigation of the epistemic credentials of *all* propositions, and what's more, of all of them *at once*. Whatever this discipline is, it isn't science.

It then becomes very hard to see how anthroepistemology squares at all with SPSK. There are two main worries. The principles of SPSK, in particular the causality and reflexivity principles, affirm that it is possible to study science by means of science.⁶ As originally construed by Bloor, SPSK constitutes a scientific investigation of belief that can in principle profitably be turned onto itself: the tools of SPSK can be used to explain how SPSK itself came about. On the other hand, anthroepistemology understood *à la* Williams is not in the province of science, and the following asymmetry emerges: anthroepistemology would want to study science, but science would be unable to study *it*, thus violating the principles of SPSK.

The second worry is even more basic. It is difficult to see how a discipline where 'everything is up for grabs' can explain anything. In explanation, we typically take a set of claims that we are unfamiliar with, and subsume them under a further set of claims that we are more confident about. But traditional philosophical epistemology offers no scope for ascribing different 'degrees of confidence' to different propositions. On the contrary, every claim is suspect and all of them are *equally* suspect. It would seem then that anthroepistemology cannot shed any light on science – for it lies on much shakier grounds than science itself. It is rather anthroepistemology itself that stands in need of socio-historical explanation.

NOTES

- 1 Fine is in turn, quoting Einstein: 'The whole of science is nothing but a refinement of everyday thinking' (Fine, 1998).
- 2 See John Earman's (1996) admirable and balanced discussion of the limitations of Bayesianism as a model of scientific epistemology.
- 3 This point demands a careful formulation. SPSK did not deny that scientists justify their assumptions to each other on the basis of the available evidence. Nor did SPSK deny that the language of justification, evidence, etc., plays a role in scientists'

- evaluations. What SPSK questioned is the ability of the extant philosophical accounts of evidence and justification to do justice to the scientists' own justificatory and evidential practices.
- 4 As regards science, philosophers like Nancy Cartwright, Arthur Fine and Ian Hacking have been 'getting in there' (see, for instance, Cartwright [forthcoming], Fine [1997], Hacking [1983]). I get in there too, in my discussion of early accounts of superconductivity (1999).
- 5 See Williams (1996).
- 6 It is often assumed that Bloor stipulated sociology as the only explanatory tool. But, as Maffie points out (p. 000), this is an unfair construal of SPSK based on an inaccurate reading of the text. Bloor insisted only that sociology had to contribute *at least part* of the explanation of scientific belief.

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