Penultimate Draft

Scientific Realism and Social Epistemology

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Abstract

Scientific realism (=SR) is a view of scientific knowledge, and scientific knowledge obviously is the product of research groups, traditions, schools of thought, or paradigms. Nevertheless, these social dimensions of scientific knowledge to date have not been at the forefront of SR theorising. Work on these dimensions has however been prominent in various forms of social epistemology. This paper seeks to connect the two fields by continuing a debate over the relationship between SR and one important strands of social epistemology, to wit, the "Sociology of Scientific Knowledge" (=SSK). Some philosophical commentators take SSK to be incompatible with SR, others as fitting with SR. I shall concentrate here on the contributions of four authors that exemplify different possible stances. I will try to defend an irenic solution to the dispute.

Bio

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Introduction

Scientific realism (=SR) is a view of scientific knowledge, and scientific knowledge obviously is the product of research groups, traditions, schools of thought, or paradigms. And yet, these social dimensions of scientific knowledge have not been at the forefront of SR theorising. Work on these dimensions has however been prominent in various forms of social epistemology. This paper seeks to continue a conversation over the relationship between social epistemology and SR.

"Social epistemology" can be understood broadly or narrowly. On the broad understanding, it covers all systematic reflection on the social dimension or nature of

cognitive achievements such as knowledge, true belief, justified belief, understanding, or wisdom. The sociology of knowledge, the social history of science, or the philosophy of the social sciences, are among the key parts of social epistemology thus understood. On the narrow understanding, social epistemology is primarily a philosophical enterprise, and has its roots in Anglo-American epistemology, in feminist theory, as well as the philosophy of science (Kusch 2011: 873).

In this chapter I shall focus on one ingredient of the broad understanding, to wit, the "Sociology of Scientific Knowledge", or "SSK" for short. It is this ingredient that has stimulated most debate with, and amongst, philosophers interested in SR. Some philosophical commentators take SSK to be incompatible with SR, others as fitting with SR. I shall concentrate here on the contributions of four authors that exemplify different possible stances: Jeff Kochan (2008, 2010), Tim Lewens (2005), David Papineau (1988) and Nick Tosh (2006, 2007, 2008). Kochan, Lewens and Papineau take different conciliatory lines, while Tosh opts for irresolvable disagreement.

I shall follow the authors' selection as to which strand *within* SSK most interestingly engages with scientific realism. This strand is the "Strong Programme" in SSK, developed first and foremost by Barry Barnes, David Bloor, and Harry Collins. (Bloor and Barnes will figure more prominently since their writings are philosophically richer than Collins' works.) I will try to defend an irenic solution to the dispute over the relationship between SR and SSK thus understood. Against Kochan I shall argue that there is more SR in SSK than he allows for. Against Tosh I shall seek to establish that the realism of SSK is not in conflict with other elements of the programme. And finally, against Lewens and Papineau, I shall maintain that a reliabilist version of SR is unable to block the sociologists' relativism.

The Strong Programme

Philosophers and sociologists disagree over the question how SSK is best defined. But no-one disputes that Bloor's "four tenets" of the Strong Programme are central. They are:

1. It [i.e. the Strong Programme] would be causal, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.

2. It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.
3. It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.
4. It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. (Bloor 1991: 7)[1]

One famous historical study in the sociology of scientific knowledge that clearly follows at least the first three tenets is Steven Shapin's paper on phrenology in early-nineteenth-century Edinburgh, "Homo Phrenologicus" (1979). Shapin begins by noting that anthropologists have identified three kinds of social interests that motivate preliterate societies to gather and sustain knowledge about the natural world: an interest in predicting and controlling events in the natural world, an interest in managing and controlling social forces and hierarchies, and an interest in making sense of one's life situation. The first-mentioned interest hardly calls for further comment. But how does an interest in social control relate to knowledge about the natural world? The answer is that people everywhere use knowledge about the natural world to legitimate or challenge social order. It is almost invariably regarded as strong support for a given social arrangement if it can be made out to be 'natural', that is, in accord with the way the (natural) world is.

Shapin argues that the same three kinds of interests can also be found sustaining scientific knowledge – phrenological knowledge in early nineteenth-century Scottish culture for example. Phrenology was developed in late eighteenth-century Paris by Franz Josef Gall and Caspar Spurzheim. In Edinburgh these ideas were taken up and championed by various members of the rising bourgeoisie. Phrenologists believed that the mind consists of 27 to 35 distinct and innate mental faculties (e.g., amativeness and tune). Each faculty was assumed to be located in a distinct part, or 'organ', of the brain. Moreover, the degree of possession of a given faculty was thought to be correlated with the size of the respective organ. And, since the contours of the cerebral cortex were taken to be followed by the contours of the skull, phrenologists believed that they could 'read off' the skull of a person which faculties he or she possessed and to what degree. Phrenologists believed that the faculties were innate, but they allowed that the environment could have a stimulating or

[1] It would be a mistake to think that these four tenets sum up *all* of SSK. In the surface, they say nothing, for example, about SSK relativism, nominalism, or "meaning finitism". I shall not try to explain and sum up these complex commitments at this point. Central elements of these doctrines will surface in the discussion below.

inhibiting effect upon the growth of the brain organs. They also held that social values and feelings were the outcome of an interaction between individuals' innate faculties and the institutions of a particular society.

How then did this theory serve the aforementioned three interests? There can be no doubt that the phrenologists were genuinely curious about the brain as a natural object. They amassed an enormous amount of detailed knowledge about the convolutions of the cortex; they were the first to recognize that the grey and white matter of the brain has distinct functions; and they noticed that the main mass of the brain consists of fibres. They clearly collected as much information about the brain as they could – with their limited means – hoping eventually to be able to explain more and more of the brain's structure and functioning. Thus the interest in prediction and control was obviously important.

As far as the other two interests are concerned, it is important to note that the advocates of phrenology came from bourgeois and petty bourgeois strata in the society. At the time, these strata were moving up in society. Traditional hierarchies and forms of social control were breaking down as commercial interests became more dominant. The economy was rapidly undergoing a shift from a traditional agricultural to a modern industrialist system. This shift weakened the old aristocracy and worked to the advantage of the middle classes. Phrenology was used as an argument in favour of the change. First, it considerably increased the number of mental faculties over the traditional six. An increased number of mental faculties provided a natural argument for a greater diversity of professions and division of labour. Second, the new faculty of 'conscientiousness' explained the new social reality of competition and contest: this was the faculty that allowed people to compare their standing with that of others. And third, phrenology also made sense of the experience of collapsing hierarchies. Traditional philosophy had put a heavy emphasis on the boundary between spirit and body – metaphorically, 'spirit' stood for the governing elite, 'body' or 'hand' for the workforce. Phrenologists stopped short of equating body and mind, but they made the brain the organ of the mind. In other words, phrenological theory was popular among the rising bourgeoisie since it allowed the latter both to feel at home in the changed socioeconomic situation and to argue against the dominance of the old aristocracy.

It is easy to see that the first three tenets of Bloor's programme – causality, impartiality and symmetry in style of explanation – are central to Shapin's analysis. Shapin's study proposes a causal explanation for the fact that the members of the Edinburgh bourgeoisie tended to favour phrenology over other theories of the mind. The relevant cause was their interest in making sense of their social situation in changing society in a way that benefits them. Shapin does not say or imply that this social interest was the *only*

cause of the belief in phrenology. Indeed, his reference to the role of the interest in prediction and control (of the natural world) and thus to the phrenologists' detailed brain mapping suggests that other causes, for instance the phrenologists' observations about the brain, also were causes of their belief. Furthermore, Shapin's analysis is impartial; he does not attempt to determine which parts of the phrenologists' or the traditional philosophers' theories were true or false, successes or failures. Shapin's mode of investigation is simply blind to these differences. And thus Shapin's style of explanation is also 'symmetrical': the same types of cause explain true and false beliefs. That is, the phrenologists' various social interests explain (in part) why they opted for their theory, both for the parts we now regard as true and for the parts that we now regard as false.

Papineau's Conciliatory Response

Having introduced the "Strong Programme" in general terms and with an example of one of its most celebrated case studies, I can now turn to the philosophical debate over its relationship to realism in general, and SR in particular. I begin with Papineau's contribution. In his paper "Does the Sociology of Science Discredit Science?" (1988), Papineau defends a negative answer to his title question. Papineau wishes to determine what follows for SR from the fact that, according to the SSK theorists' case studies, scientists often do not behave as traditional rationalist images of science would lead us to expect. That is to say, these studies often portray scientists as influenced by factors that the realist would not see as "good reasons" for the scientists' beliefs (1988: 37).

Papineau's central idea is borrowed from epistemology. He distinguishes between "Cartesian" and "naturalized" epistemology. Cartesian epistemology is a form of epistemic-internalist foundationalism. It holds that, to be appropriately epistemically justified, our beliefs must be based on good reasons accessible to our consciousness (1988: 39). Naturalized epistemology is an externalist form of reliabilism about epistemic justification. For a belief to be justified it is sufficient that it is produced by a reliable process. It does not matter whether the holder of the belief is aware of this process or not. Arguments for beliefs are not without interest and importance, but they are not always necessary. Even a non-conscious belief can be justified (1988: 41). Naturalized epistemology has a prescriptive side, too: individuals and groups should seek to develop ever more reliable belief-forming techniques (1988: 43).

Papineau maintains that it is Cartesian epistemology – and Cartesian epistemology alone – that cannot but take SSK to be discrediting science. The first step of the argument

supporting this conclusion is the idea that Cartesian epistemology is naturally thought of as a form of anti-realism. This is so because Cartesians conceive of reason as prior to truth: "'Truth' and 'reality' … are simply epithets attached to the picture of the world that reason leads us to" (1988: 46-47). Moreover, and this is the second step, "rationality is by definition the way that scientists think" (1988: 49). And, step three, there is the rub: if SSK is right, then the reasoning of even highly successful scientists contains elements that intuitively should not be there (such as social-political interests). This is a conclusion that the Cartesian is unable to accept. And hence she has to conclude that SSK discredits science (1988: 49).

Papineau thinks that naturalized epistemology can respond to SSK differently. To begin with, naturalized epistemology is a form of realism rather than antirealism. This means, according to Papineau, that truth is prior to reason. Moreover, naturalized epistemology does not seek to justify standards of rationality with reference to how scientists think. Epistemic standards are justified if and only if they are *in fact* reliable techniques for reaching a high proportion of true over false beliefs. It follows from this, Papineau alleges, that naturalized epistemology is not forced to assume that SSK case studies discredit science (1988: 51). More precisely, Papineau holds that the overall structure of scientific practice would not be reliable for truth if the processes bringing about scientific beliefs included "*only* social factors". But the results of SSK do not establish this conclusion (1988: 52).

Lewens' Ambivalent Response

Lewens' "Realism and the Strong Program" (2005) picks up the thread where Papineau left it seventeen years earlier. Lewens pushes the argument further by attending not just to SSK case studies but also to Barnes' and Bloor's theoretical pronouncements. Moreover Lewens focuses on agreement as well as disagreements between SSK and SR. Whereas Papineau had merely insisted that the naturalized epistemologist is not forced to think of SSK as discrediting science and SR, Lewens even finds some statements of Barnes and Bloor congenial to SR. He applauds Bloor's statement that "(non-social) nature plays a central role in the formation of belief" (Bloor 1999: 102) and Barnes' pronouncement that "talk of 'external reality' is thoroughly justified and sensible" (Barnes 1992: 135; Lewens 2005: 560). Indeed Lewens even finds little to disagree with in the four tenets of the Strong Programme. The realist too seeks to give causal explanations for beliefs, and although social causes will

often be distal rather than proximate[2], even the distal role "seems enough to ground empirical sociology of knowledge". The requirements of impartiality and reflexivity are likewise realist common sense: the realist too thinks of all beliefs as caused, and he has no objections to the idea that the beliefs of sociologists require causal sociological explanations as well (2005: 562-3). Lewens spends more time analysing the symmetry tenet, but his primary concern is to shield it from widespread misunderstandings. For instance, Bloor's symmetry requirement is not that true and false beliefs have exactly the same explanations. It is the requirement that true and false beliefs are accounted for using "the same *family* of explanatory concepts" (2005: 563).

Lewens thinks that reliabilist externalism often fits with the symmetry tenet. Take two individuals with the same reliable system of vision, one of whom is looking into a normal cubic room while the other is looking into a trapezoid *Ames room*. The first will acquire justified and true beliefs, the second unjustified and false beliefs. And yet, as far as neurological level is concerned, both beliefs receive the same causal explanation (2005: 565). Lewens also reminds his readers that reliability is context-dependent and sometimes even community-dependent. It is the latter when the reliability of one's testimonial beliefs depends upon a sufficient number of truth-tellers in one's environment (2005: 566).

Turning from agreement to disagreement, Lewens finds fault with Bloor's use of explanatory contrasts. The key passage in this context is one of Bloor's methodological comments when discussing the dispute between Robert Millikan and Felix Ehrenhaft over the possibility of sub-electronic charges (cf. Holton 1998: 25-83; Franklin 1986: 140-164; Barnes, Bloor, Henry: 1996: 18-45). Bloor grants that today "we believe … that Millikan got it basically right" and that thus "electrons … did play a causal role in making him believe in … electrons". So far, so good, as Lewens is concerned. The problem is with the way Bloor continues:

> But then we have to remember that (on such scenario) electrons will also have played their part in making sure that Millikan's contemporary Felix Ehrenhaft didn't believe in electrons. Once we realize this, then there is a sense in which the electron 'itself' drops out of the story because it is a common factor behind two different responses, and it is the cause of the difference that interests us. (Bloor 1999: 93; Lewens 2005: 572).

[2] By "distal cause" here is a cause "upstream" from the belief, that is, further away in the causal chain leading to the belief. A proximate cause is close.

This is the part of Strong Programme methodology Lewens find unpalatable, at least if it is generalized to cover all SSK explanations. His counterexample involves "Bigfoot" hiding in a cave. Jim enters the cave and sees the creature; John stays outside to sleep. Lewens insists that if we are to explain why Jim believes that Bigfoot is in the cave, and why John does not, Bigfoot cannot drop out of the story. Lewens thinks this case generalizes to science. Often the best explanation for a difference in belief between two disagreeing scientific communities is that one was exposed to a different part of the world than the other. Lewens alleges that Barnes and Bloor, in earlier work, had in fact "conceded" this very point, when they wrote that "certainly any differences in the sampling of experience, and any differential exposure to reality must be allowed for" (Barnes and Bloor 1982: 35; Lewens 2005: 573).

Lewens also objects to Bloor's writing that "there are no absolute proofs to be had that one scientific theory is superior to another: there are only locally credible reasons" (1999: 102; Lewens 2005: 574). He detects here the Cartesian internalist epistemology that we saw Papineau contrasting with externalist reliabilism. It is true that we cannot prove to others that our theories or standards are superior. But from this it does not follow that there are only locally credible reasons. As Lewens has it, we need *not be able to show* that our rational standards are reliable for them *to be* reliable (2005: 576).

Tosh's Uncompromising Response

Tosh agrees with Lewens' critical part but not with the latter's conciliatory comments. Tosh's main goal is to argue that it "is impossible coherently to espouse the claims of the Strong Program while recognising the existence of scientific knowledge" (2006: 686).

The argument in essence is this. A *true belief* that p can sometimes be explained by *the fact* that p. But a *false belief* that q cannot, in any way or form, be explained by *the fact* that q. After all, there is no such fact. Applied to scientific knowledge: if advocates of SSK recognize the existence of scientific knowledge, then they must allow that scientific knowledge that p is causally connected to the fact that p, and that false beliefs that q are not so connected.[3] And this breaks the symmetry between the explanation of true and false beliefs.

[3] I am writing "causally connected" rather than "caused" to avoid the impression that for Tosh knowledge must always be causally downstream from the fact. Jim might know that it's going to rain tomorrow. Tomorrow's rain does not cause his belief; rather, the present atmospheric conditions cause both his present belief and tomorrow's rain. (Tosh, in correspondence)

Put differently, Tosh makes two points, one trivial, the other substantive. The trivial point is that if there is no fact that q, then *a fortiori* q cannot be used to explain the belief that q. The substantive claim is that the trivial observation leaves open the possibility that the fact that p might relevantly be cited to explain someone's belief that p. For instance:

> … if we believe both that electrons have (and always have had) a charge-to-mass ratio of 1.76×10^{11} C kg^{-1}, and that J. J. Thomson believed that electrons have a charge-to-mass ratio of about 10^{11} C kg^{-1}, then we are very likely to want to tell a causal story relating the latter to the former' (2007: 687).

Bloor grants that "we believe … that Millikan got it basically right" and that thus "electrons … did play a causal role in making him believe in … electrons". Tosh reads this as a commitment to SR. But Tosh finds this commitment in contradiction with the symmetry principle. As we saw, Bloor thinks that Ehrenhaft also interacted with electrons and that "the electron itself 'drops out' of the story because it is a common factor behind two responses". As Tosh has it, Millikan's true belief that there are electrons is at least in part explained by the existence of electrons, whereas Ehrenhaft's false beliefs that there are subelectronic charges is not explained by the existence of either electrons or subelectronic charges (Tosh 2006: 687). More precisely, it is the difference in how Millikan and Ehrenhaft set up their experiments that explains why Millikan came to believe in their existence and why Ehrenhaft did not. But this difference is explanatory only on the assumption that electrons exist and have a charge of about 1.6×10^{-19} C (2007: 190). Tosh is happy to concede that the true charge of the electron is not the "complete" causal explanation for Millikan's belief in electrons. But he deems it likely that a proper causal account of how Millikan and Ehrenhaft arrived at their respective views will end up invoking (what we take to be) the correct charge of electrons. And this use of electron physics will not be symmetrical (2007: 191):

Tosh considers a number of possible objections to his argument. The most important of these objections builds on Ian Hacking's claim that "we should not *explain* why some people believe p by saying that p is true". Hacking asks us to consider how we explain why some scientist came to believe in the existence of a "Big Bang" in cosmology. We might provide a long list of reasons, Hacking assumes, but the actual truth of the Big-Bang theory will not be one of them. Tosh is not convinced. He accuses Hacking of conflating explanation with justification. The truth of the Big Bang theory cannot be one of the actor's reasons for

believing it, but it might still explain why the actor came to believe it (Hacking 1999: 81; Tosh 2006: 691).

Kochan's Defence of the Strong Programme

Kochan seeks to defend the Strong Programme against Lewens and Tosh. Like Lewens, he is concerned to find common ground between SSK and SR; and unlike Tosh, Kochan denies that Strong Programmers are, or should be, committed to SR.

Kochan repeatedly emphasizes that SSK is happy with at least a *weak form of realism*, that is, the view that there exists a mind-independent world (2008: 25). But this conception is not as ambitious as SR. What then is the problem with the latter? As Kochan sees it, SR treats scientific knowledge as a "resource" rather than as a "topic" for historical explanations of scientific beliefs. This means that scientific realists "explain the credibility of scientific beliefs on the basis of their correspondence to an inherently structured reality" (2010: 131). In so doing, scientific realists assume that correspondence is a causal relation, and that there is "a special form of perception" that puts us touch with "absolute feature[s] of the world" (2010: 137).

By contrast, SSK assumes nothing of the sort. To make this plausible Kochan follows Tosh in focusing on Bloor's statement that "we take for granted that trees and rocks, as well as electrons and bacilli, have long been stable items amongst the furniture of the world" (Bloor 1999: 86; Kochan 2010: 130). As Kochan has it, this is not a commitment to SR: Bloor merely states what "we" in ordinary life take for granted. Our local tradition does "compel us to judge in favour of Millikan's theory" (2010: 137). But Bloor does not thereby commit himself to thinking that, in positing that electrons and bacilli exist, science has hit upon the one inherent structure of the world. For Bloor the world has no such structure. In fact, nature does not determine the one correct theory about it, and it allows for a multitude of descriptions and classifications (2010: 131). This does not mean that Bloor is sceptical about scientific knowledge. On the contrary, SSK even uses scientific knowledge "as a resource in sociological explanation" (2010: 132).

Kochan suggests that the ideas of the last paragraph must be seen as operative when Bloor speaks of electrons as playing a causal role in Millikan's and Ehrenhaft's experiments. Here Bloor is not using the term "electron" in the sense in which it is used by Millikan. Nor does "electron" stand for something that a scientific belief can "track" (2010: 131). Instead the term stands for "the natural causes, or 'states-of-affairs' in the world, which produced the experimental data of both Millikan and Ehrenhaft" and which can be interpreted in

different ways (2010: 132). Put differently, "the natural attitude" which takes the existence of electrons for granted, is "inappropriate for sociologists and historians" who seek to explain how our belief in electrons came about. The same applies also to Lewens' Bigfoot scenario: if we want to explain why the person entering the cave took himself to be seeing Bigfoot, we cannot use Bigfoot himself as a cause. To do so would be to use Bigfoot as resource and as a topic in one and the same explanation. And this Kochan regards as unacceptable (2010: 134-135).

An Irenic Resolution

The viewpoints of Papineau, Lewens, Tosh and Kochan are difficult to reconcile. There is no alternative to deciding on the correctness of the respective readings of SSK.

A useful place to start is the distinction between different three kinds of realism:

(A) minimal realism: the view that there exists a mind-independent world;
(B) the unreflective realist talk of everyday life, that is, the "natural attitude" of talking about rocks and trees, electrons and bacilli as things;
(C) scientific realism vis-à-vis the natural and / or social sciences. This involves three claims:
    (a) the metaphysical view that "the world has a definite and mind-independent natural-kind structure";
    (b) the semantic view that scientific theories in the mature sciences are approximatively true, and that the relevant theory of truth is the correspondence theory; and
    (c) the epistemic view that the predictively successful scientific theories of the mature sciences are well confirmed. (Psillos 1999: xix)

It should be uncontentious that SSK theorists are committed to (A). They are not Berkeleyan or Hegelian idealists. To jump from (A) to (C.c), note that Stathis Psillos' definition and book-length defence of SR does not involve two of the features Kochan attributes to SR; to wit, that SR makes correspondence a causal notion, and that it involves a special kind of perception. The first claim is moreover explicitly denied by Lewens (2005: 570). Insofar as SR is not committed to these claims, the distance between SR and SSK is reduced.

Turning to (B), there seem to be good grounds for attributing to SSK theorists central ingredients of a *SR about the social sciences*: practitioners of SSK have no scruples about making explanatory use of the theoretical and unobservable posits of a wide range of social theories. Examples are classes and their interests, groups, or social structures. Of course, social kinds differ from natural kinds in not being mind-independent.[4] And practitioners of SSK do not believe that the theories of SSK are predictively successful – at least not over and above the general prediction that all scientific knowledge has social variables. But these two provisos to one side, at least when it comes to *basic* categories of social life, such as "group", "interest", "common knowledge" and the like, SSK theorists never use the idea that reality has no definite structure or that it allows for numerous and equally acceptable alternative conceptualizations. Furthermore, the theorists insist that SSK case studies can be, and often are, true to the historical facts. SSK's straightforward and bold realism about the social world has occasionally been challenged from the outside of the field, that is, by practitioners of ethnomethodology (e.g. Lynch 1992). But this challenge has not been able to weaken the social-SR of authors like Barnes, Bloor, Harry Collins or Shapin.

The key question in the present context is of course how SSK stands vis-à-vis SR about the natural sciences. This topic is complicated. Let me begin with a couple of comments on how SSK theorists use natural-scientific knowledge.

When SSK scholars investigate a specific scientific claim p, they rely on the first three tenets of the Strong Programme in order to see the credibility of p as being in principle as problematic as imaginable or real alternatives. But note that this method of making social processes salient is only ever applied to one specific claim or theory at a time. And while this one claim or theory is turned into a topic of research, the rest of science remains in the position of a taken-for-granted resource. For instance, when studying how Millikan's claims about electrons became credible in physics, the SSK scholar freely speaks about atoms, electric currents, gravitation, and much else (unobservable) besides (Bloor 1991: 177; cf. Collins 2004: 758, 793-794).

There is a further way, too, in which the SSK theorist relies on scientific knowledge as a resource. Theorists like Barnes or Bloor have always been keen to be "naturalists" about the social (Bloor 1999: 87). That is to say, against philosophers like Peter Winch, or

[4] Here it is important to recall a common-sense distinction between two kinds of mind-independence. Psychological and social kinds are mind-dependent insofar as their existence depends on the existence of minds: they are kinds of properties of minds, and kinds of relations between minds. But psychological and social kinds need not be mind-dependent in the following sense: their existence need not owe nothing to the specific thinking mind that uses them for purposes of psychological and sociological explanation and prediction. (A) speaks to the second, not the first sense. (At least if we treat social kinds as one kind of natural kind.)

fellow sociologists like Harry Collins, they have tried to integrate their sociological theses with other scientific fields focused on human capacities. Particularly important in this respect has been the psychology of perception and its philosophical interpretation at the hands of Jerry Fodor and Paul Churchland. Thus in their joint book (written together with John Henry), Barnes and Bloor discuss a range of theories of perception and side with Fodor's modularity thesis against Churchland's insistence on plasticity (Barnes, Bloor, Henry 1996: 1-17). Here too SSK theorists are not troubled by their own talk about unobservable entities and structures.

In order to penetrate more deeply into SSK theorists' attitude towards SR, we need to take notice of some of their more general views regarding the nature of natural knowledge, Mary Hesse's "network-model". After a brief summary of Bloor's rendering of the model (Bloor 1982; cf. Barnes 1981) I will discuss its compatibility with SSK.

The basis of the model is an idealized account of naming. Language learners are taught to associate specific words with conventionally discriminated things or features of the environment. Call the latter "exemplars". Humans have the ability to generalize. They apply their terms in new circumstances on the basis of similarity judgements. New cases may or may not be bracketed with the exemplars. Bloor suggests that the same basic ability to recognize similarities is still operative in science. He mentions the use of models and analogies as clear cases of this phenomenon (1982: 270).

Our primitive sense of similarity often allows for more than one way of developing our classificatory scheme. Some of these developments are acceptable to other speakers of the language, some are unacceptable. In other words, our similarity judgements are frequently overruled. Bloor puts much emphasis on the fact that the model, as outlined so far, points to the importance of both a psychological and a sociological factor; the former comprises our perceptual capacities and primitive sense of similarity, the latter concerns the interaction between speakers and the role of convention (1982: 271).

Within a system of classification different kinds of entities are connected by "elementary laws", for instance "fire is hot". These laws involve probability estimates of the form: the occurrence of stimulus A makes the occurrence of stimulus B probable. Bloor emphasises a sociological perspective on such laws, suggesting that many of them have the "status of conventional typifications" and are learnt from accepted authorities. This makes them "collective representations" (as Durkheim would have called them). Bloor notes three features of such laws. First, they extend the area in which a classification can be confidently applied. Second, the laws need not be true for technologies informed by them to be successful. After all, the steam engine was a technology initially based on the caloric theory

of heat. And third, laws always form networks (of two or more laws). An example of such a system of laws is our knowledge concerning mammals (including how they differ from fish). (1982: 272-273)

Networks of (empirical) laws often face problems with new cases. Consider what happened to our knowledge of mammals and fish when we discovered whales. These odd creatures are like fish in spending their whole life in the oceans, and like mammals in suckling their young. Are whales fish that suckle their young, or are they mammals that spend their life in the oceans? The answer is underdetermined by perceptual similarities. This brings home the point that our verbal rendering of experience is a matter of both our sense perception and our responsiveness to networks of laws. Moreover, every element of the network is in principle open to negotiation. Each and every element can be given up, as long as the appropriate changes are made elsewhere in the network. (1982: 274)

At the same time, the network cannot be changed arbitrarily: classificatory decisions must be made in light of experience. Hesse spoke of this feature as the "correspondence postulate". Bloor suggests "adaptation postulate" instead since he regards the allusion to the correspondence theory of truth as misleading. As Bloor has it, the correspondence theory of truth implies "structural identity" of a fact and a belief, or "the perfect reflection of reality in knowledge". But this is not what Hesse was after – or should have been after. The network model comes with the assumption that "reality is indefinitely complex" and that all networks of laws simplify the experience they are rendering intelligible. No one network can hence be the whole truth. (1982: 278)

The adaptation postulate is not the only factor which explains the relative stability of our networks of laws and concepts. At least equally important are the efforts by their users to protect certain parts of the network from change, using the rest of the network in doing so. In so doing they assume protected parts to be true or self-evident, "but this will be a justification for the special treatment rather than the cause of it". The sociologist will seek the causes amongst the beliefs and interests of the users, not amongst the properties of laws. Parts of networks that attract efforts to protect them are of two main kinds: models, metaphors and analogies on the one hand, and boundaries or distinctions on the other hand. Hesse sought to capture these protective strategies with the concept of "coherence conditions": these are factors which govern a whole network of laws. Hesse suggested that "culturally conditional metaphysical principles" might qualify. Bloor prefers to take the idea in a different direction. He follows the anthropologist Mary Douglas' proposal according to which metaphysical principles are part and parcel of our attempts to control others around

us. We so construct our knowledge of nature that we can use it to justify our preferred social arrangements. (1982: 283)

All this is not to suggest that what we call "knowledge" is just a fairy tale invented for political purposes. It is to maintain instead that "knowledge" is the resultant of two vectors: the vector of experience and the vector of convention and social interests (cf. Bloor 1991: 32). The social dimension can never be filtered out. Moreover, and to repeat, nature is indefinitely complex and every network is a simplification. It follows that no network can ever cut nature at its joints. There thus is no one unique set of natural kinds. Elsewhere Bloor and Barnes claim that the model also suggest three further ideas. To begin with, "all cultures are equally near to nature" insofar as all networks "engage with nature according to the same general principles" (Bloor 1999: 88; cf. Barnes 1981: 316). Furthermore, the model "has no place for the myth, much beloved by many realists, that science progresses by converging on the truth" (Barnes 1992: 143). And finally, it is a mistake to think that if a theory is predictively successful, then "its terms must stand in a one-to-one link to the things mentioned": the predictive success of a theory is always the predictive success of the theory *as a whole*. This makes it illegitimate to attribute this success *atomistically* to the alleged reference of some terms of the theory (1999: 94).

Let us take stock. On a first reading the "network model" might seem incompatible with SR: no convergence on the truth, no inference from predictive success to truth or reference, no correspondence theory of truth, no unique set of natural kinds, reality indefinitely complex, and the all cultures equally near to nature. Can you get further from SR?

On a second, closer, look things are a little less clear-cut. First, the scientific realist too can accept that reality is indefinitely complex and that all classifications and theories of natural processes are therefore simplifications. This claim seems to be just realist common sense. Of course the realist insists that some simplifications are better than others, given certain purposes. But it is hard to see how the SSK theorists could disagree. Second, reconciliation might look less likely concerning the issue of natural kinds. If SR insists on one unique set of natural kinds, then SSK and SR are incompatible. Note however that this assumption of unique natural kinds is not accepted by some authors who call themselves realist (e.g. Dupré 1993, Hacking 1991). Third, the claim that all cultures are equally near to nature is innocuous if it merely means – as it seems to mean – that all cultures develop their classifications on the basis of the same basic psychological and social mechanisms, and that long-living cultures have successfully adapted their beliefs and belief-forming techniques to nature. Fourth, Bloor's opposition to truth as correspondence seems to be an opposition to

a rather specific version of this theory: to wit, the view that a belief or theory could be "a perfect reflection of reality" – without simplification or idealisation – and that it is in principle possible for us to arrive at the whole truth about the world. These are claims that most sensible scientific realists will reject, too. Fifth, when SSK theorists attack "naïve realism", their targets are philosophers, not scientists. I suspect Barnes and Bloor would agree with Collins who writes: "I endorse realism as an attitude both for scientists at their work and for sociologists at theirs" (2004: 15). There is no suggestion in the context of this endorsement that would limit the realism to observables. Barnes and Bloor attack what they regard as the philosophers' "naïve realism" because they see the latter as focused on unification and inevitability. The SSK theorists insist that there is no reason to assume that science will converge on a single unified theory of everything. They see this as a central implication of the network model. There may well have been a time when SSK and SR differed on this score. Today the situation is less clear. After all, one of SR's most prominent defenders, Howard Sankey, has recently written explicitly against saddling SR with claims concerning the inevitability of scientific progress (Sankey 2008; cf. Kinzel, forthcoming).

Above I have argued that SSK theorists are scientific realists of sorts regarding some areas of the social and cognitive sciences. And I have suggested that some of their opposition to SR has become obsolete in light of recent developments in SR. But I do not want to downplay those passages in which Bloor and Barnes directly challenge SR's core assumptions. For instance, Barnes insists that the SSK theorists' interpretation of Hesse's network is "uncompromisingly 'instrumentalist'" (1981: 307). And Bloor attacks the "naïve" assumption of a "one-to-one link" between terms of a predictively successful theory and natural kinds in the world. ("If the talk is about electrons or microbes, then there must be electrons or microbes …" 1999: 94.) Of course, SR is not committed to simple-minded "one-to-one links" between *all* theoretical terms and natural kinds, and today *structural* SR is the preferred option in many quarters anyway. But I doubt that Bloor would withdraw his criticism in light of these corrections and modifications. There is some hope of a rapprochement, however, in Bloor's further comment in the same context:

> Obviously individual terms in the theory will have individual occasions of use. We talk about *these* electrons, *these* microbes, *these* lines of force, and so on. On those occasions particular experiential episodes will prompt the application of our terms, but that doesn't mean some uniquely direct or successful reference has been achieved. The entire system of classification is implicated and, before long, this may change. (1999: 94)

Thus talk of electrons and microbes is alright as long as we recognize that the referential links to features in the world are indirect, partial, and mediated by the entire fallible system of classification. This is not quite what a hard-core SR has in mind, but neither is it scepticism about referential links tout court.

Note also that Bloor does not present anything like a pessimistic metainduction. It is true that, at one point, Bloor uses the idea that all scientific theories sooner or later face competitors or alternatives (1999: 106). But he does not conclude from this that we should suspend judgment with respect to our current theories – or at least their theoretical entities. Bloor's goal is rather to deal with a difficulty that emerges when three ideas meet: that an SSK analysis of scientific knowledge seeks to identify its conventional character; that we can speak of conventions only where it makes sense to speak of *alternative* conventions; and that for our current best science it is often difficult to identify such alternatives. Bloor's response to this conundrum is an "optimistic induction" based on SSK case studies concerning past science: SSK scholarship has always found, in the historical record, competitors to past scientific theories; hence such competitors are likely to emerge for our current best science as well. It follows that no scientific theory is in principle beyond sociological analysis.

Above we saw that the relationship between the unreflective realism of everyday life (=B) and SR (=C) is one of the contentious issues between Tosh and Kochan. Tosh sees SSK as committed to SR on the grounds that Bloor speaks of electrons and bacilli as things that "we" accept. Kochan is right to insist that things are not that simple. And yet, it is not easy to accept Kochan's interpretation either. He says both that as members of our culture we are "compelled" to believe in electrons, and that sociologists can escape this compulsion when they resist the impulse to explain its source in terms of mind-independent features of reality. But that raises more questions than it answers. On the one hand, scientists and philosophers of instrumental or constructive-empiricist persuasions have also been sceptical about (some) theoretical entities that have made it into the standard curriculum. On the other hand, we might wonder whether SSK is meant to free the rest of us from this compulsion or whether we are free to return to our naïve talk after we have taken on board the role of tradition and training in the dispute between Millikan and Ehrenhaft. These comments are not meant as a criticism of Kochan. He seems to me to correctly pick up on one strand in the texts of Bloor and Barnes. But I also see value in Tosh's insistence that our two SSK theorists waver in their response to SR: sometimes they reject it outright; sometimes they come surprisingly close.

Over the past few paragraphs I have provided grist to Kochan's and Lewens' mill by suggesting that the SSK theorist and the scientific realist need not be total enemies. But this leaves me with three loose ends: to answer Tosh's claim according to which SR and the symmetry principle are incompatible; to situate SSK vis-à-vis reliabilism; and to explain what SSK's relativism boils down to. I shall address these three issues in that order.

A fair discussion of Bloor's claim that "the electron 'itself' drops out of the story" should begin by acknowledging Barnes and Bloor's statement that "certainly any differences in the sampling of experience, and any differential exposure to reality must be allowed for" (Barnes and Bloor 1982: 35). Lewens is fair enough to quote this passage but then goes on to treat it as a "concession". This rendering seems a little unkind. A more charitable reading would be to say that when differential exposure to reality is a key cause of opposed beliefs, then reality does not drop out of SSK stories. The Bigfoot case, in Lewens' rendering, is thus not fit to cause trouble for Bloor's general position.

We might still ask: how would we have to modify the Bigfoot scenario for Bigfoot to drop out of the story? Assume that around 1900 Jim and John disagreed over the existence of Bigfoot. Allow further that we today have excellent evidence for Bigfoot's existence. Jim and John have both travelled far and wide in the region where Bigfoot was supposed to dwell. They have never seen Bigfoot, but Jim has found droppings that he judged to originate with Bigfoot. John has inspected these droppings as well, but has a different theory as to their source. If we want to explain why Jim believed in Bigfoot, and John did not, then it is unclear what causal role Bigfoot himself can play. Millikan and Ehrenhaft have the same evidence but interpret it differently in light of their respective background beliefs and research traditions. Of course Bigfoot is part of the wider causal story; but he does not help to answer our contrastive question. The same is true of the electron case. It is not the case that Millikan had a device that trapped electrons, whereas Ehrenhaft lacked such an experimental set-up electrons. If that were the case, then the electron could not drop out. But Millikan and Ehrenhaft shared their experimental data; they even recalculated each other's results. But they disagreed over how these data were to be interpreted. And at this decision node the existence of electrons is not a cause. We can all agree that there are electrons, but still use the symmetry principle to home in on the social dimensions of the controversy.

What is the relationship between SSK and externalist reliabilism? To begin with Papineau, he is entirely right to stress that SSK case studies do not automatically discredit science. Science may well be a reliable way of finding out about the world, even though, or precisely because, social interests, negotiations, and conventions play a central role.

Lewens' use of reliabilist externalism is more contentious. He uses it to block Bloor's move from "there are no absolute proofs to be had that one scientific theory is superior to another" to "there are only locally credible reasons". Lewens' objects that although there may be no absolute proofs for the greater reliability of theory t_1 over theory t_2, it may still be a non-relative, *natural*, fact that t_1 *is* more reliable than t_2. I disagree. What Lewens overlooks is that the reliability of some belief-forming mechanism is not a natural fact, but dependent on the choice of reference-class (Brandom 2000: 97-122). Let Jones form the belief that he stands in front of a barn even though he stands in front of a barn-façade. Is his belief-forming method – taking a good look from a distance – reliable? It depends. Let there be ten barns and one barn-façade in the county where Jones lives. Then his belief-forming method is reliable. Assume the county is part of a province, and that in the province there are ten barns and ninety barn-façades. Now Jones' method is unreliable. And then consider the state with the ratios again reversed. The upshot is clear: reliability is not a natural fact; it is a measure that needs a human calibration. It isn't there anyway.[5]

Throughout this paper I have said little explicitly about the relativism of SSK. At its heart are the methodological ideas of impartiality and symmetry. I agree with Lewens that these do not threaten an enlightened SR. Against Lewens I have insisted that externalist reliabilism does not block the route to the relativist insight that there are only locally credible reasons. But my objection to Lewens' use of externalism is not an argument against SR. The scientific realist too can accept that for X to be a reason, there needs to be a context that gives X its meaning and point. This does not mean that X cannot travel or become enshrined in traditions of research. It could even become universally accepted (Bloor 2011: 435).

Conclusion

I have explored the relationship between one strand of social epistemology, namely SSK, and SR, broadly construed. I have sought to bring out that this relationship is not as clear-

[5] Kochan (2008: 34) argues for the possibility of a "sociologistic form of reliabilism", using Brandom as well as Kusch (2002: 109) as amongst his starting points. I am not entirely sure how close our positions are. We both emphasize the importance of social conventions for understanding judgements of reliability. But his insistence that there are no "natural facts", that is, that "all facts" are "the outcome of a combination of natural and social causes", seems to me a form of sociological idealism (2008: 26). It seems to me to run counter to SSK theorists' insistence on the distinction between "the world" and "our knowledge of it" (Kochan 2010: 130). A plausible social-epistemological form of reliabilism should honor this distinction. Put differently, Kochan and I are agreed that facts about reliability are not natural facts. But we disagree over the question whether there are natural facts at all.

cut as either defenders or opponents of SSK, or SR, have assumed. SSK theorists are not just committed to a minimal realism about the existence of a mind-independent world. They go beyond this minimal position in their uses of both social-scientific and natural-scientific theories. At the same time we have seen that SSK cannot be saddled with being anti-scientific in its mode of explanations, or vulnerable to externalist considerations.[6]

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