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Discussion

Ideals and monisms: recent criticisms of the Strong Programme in the sociology of knowledge

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Abstract

I offer a reply to criticisms of the Strong Programme presented by Stephen Kemp who develops some new lines of argument that focus on the 'monism' of the programme. He says the programme should be rejected for three reasons. First, because it embodies 'weak idealism', that is, its supporters effectively sever the link between language and the world. Second, it challenges the reasons that scientists offer in explanation of their own beliefs. Third, it destroys the distinction between successful and unsuccessful instrumental action. Kemp is careful to produce quotations from the supporters of the programme as evidence to support his case. All three points deserve and are given a detailed response and the interpretation of the quoted material plays a significant role in the discussion. My hope is that careful exegesis will offset the numerous misinterpretations that are current in the philosophical literature. Particular attention is paid to what is said about the normative standards involved in the application of empirical concepts. The operation of these standards in the face of the negotiability of all concepts is explored and misapprehensions on the topic are corrected. The work of Wittgenstein, Popper, Kuhn and Hesse is used to illustrate these themes.

Keywords: Strong Programme; Social constructionism; Idealism; Monism; Finitism; Relativism

1. Introduction

In his paper 'Saving the Strong Programme?' Stephen Kemp argues that the Strong Programme is beset by two fatal flaws (Kemp, 2005). The implication is that it should be discarded rather than saved. Both of these flaws are held to derive from the programme being a

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species of social constructionism. First, Kemp says that the programme lapses into what he calls 'weak idealism'. If 'strong' idealists completely deny the existence of an external world, 'weak' idealists are defined as those who acknowledge an independent reality but (wittingly or unwittingly) sever any connection between that reality and our concepts. Kemp says that advocates of the Strong Programme are guilty of idealism in this weaker sense because, for them, scientific discourse is, 'free-floating and unrelated to the world of things' (ibid., p. 707). Secondly, Kemp argues that social constructionists 'challenge the credibility of the scientific concepts that they analyse' (ibid., p. 706). They are committed to discounting the role of the rational arguments that scientists offer in support of their views. For the constructionists, he says, the scientist's arguments are 'rationally unconvincing' (ibid., p. 716). According to Kemp, social constructionists must adopt this view in order to make room for their own account of credibility that is based, not on reasons, but on sociological causes that are incompatible with the arguments proposed by the scientist (ibid., p. 717).

While I appreciate the courtesy with which Kemp has presented his case, and the care he has taken to point out areas of agreement as well as disagreement, I nevertheless believe he is demonstrably wrong on both of the above points. I shall argue that his first claim is vitiated by a serious error about the operation of the normative standards involved in concept application. I shall then show that his second claim is inconsistent with the routine practices of historians and sociologists of science. Because these practices were built into the formulation of the Strong Programme, and are exemplified in the kinds of analysis developed by its advocates, Kemp's charge is misplaced. Kemp's paper 'Saving the Strong Programme?' needs to be read alongside an earlier paper called 'Toward a monistic theory of science' that was also devoted to the criticism of the Strong Programme (Kemp, 2003). This earlier work provides a revealing backdrop against which to set the later arguments and I shall devote two sections of this defence to its discussion and criticism.

Since it is evident from both of the papers that Kemp is neither an unsympathetic nor a careless critic, I must accept that the ideas I have been trying to convey were either badly expressed or were more difficult to grasp than I had imagined. It is also clear that Kemp is not alone in seeing these two weaknesses in the Strong Programme and he cites other writers who have expressed similar criticisms. I should therefore like to use this opportunity to re-explain and amplify some of the central ideas in the development of the Strong Programme, and to do so in the context of a dialogue with my critic. In the course of this discussion I hope to bring into salience certain recurrent, structural features that run through a number of apparently quite different accounts of scientific knowledge. I hope that this will give my defence of the programme some general epistemological interest. First, though, let me provide a little necessary background.

2. Getting finitism in focus

Rules and rule following have always been a central preoccupation of those associated with the Strong Programme and some of us took our lead from Wittgenstein's work in this field (Bloor, 1973).[1] Wittgenstein made it clear that his conclusions about rules applied to

[1] I made the case for reading Wittgenstein as a sociologist of knowledge in Bloor (1973, 1983). There are other, more popular, readings of Wittgenstein, for example those adopted by both the ethnomethodologists and the Oxford ordinary-language philosophers. See my exchange with Michael Lynch (Bloor, 1992; Lynch, 1992a, 1992b). The debate has recently been revived (Kusch 2004).

all acts of concept application. His central point was that rule following was a step-by-step process and the move to the next case of a rule or a concept was not predetermined by the finite number of past cases in which the rule has been followed or the concept applied—hence the label 'finitism' which has been given to his analysis.[2] Philosophers, however, have typically tried to identify some feature of these previous cases that makes it necessary that the next step in following the rule, or the next application of the concept, should be thus and so. Wittgenstein says they have failed to locate the source of this necessity because they have been looking in the wrong places. They have appealed to abstract 'meanings', or 'definitions', or 'interpretations' or 'conceptual content' or 'what the rule requires'. All such 'formal' specifications (as they might be called) will fail because they represent a justification that sets in train an infinite regress of further, formal specifications. Thus if a rule depends on an interpretation then the interpretation demands an interpretation. This, said Wittgenstein, could lead anywhere. Anything could be deemed an interpretation of anything else. He concluded that the real determinants of the next application, and the real sources of the discrimination between correct and incorrect steps and applications, were not to be found in the realm of formal specifications and justifications but amongst the totality of contingencies that impinge on the episode. He was not saying that the move to the next case was undetermined. Rather, the determinants lie around or behind the formal specifications but do not appear in or amongst them.

This result is important for sociologists of knowledge because, amongst the actual determinants and contingencies at work, there will, and must always be, social processes. These were explicitly identified by Wittgenstein himself as customs, conventions and institutions.[3] Clearly, Wittgenstein's argument insinuates the social into the most intimate and pervasive of all cognitive processes. Unfortunately the central thrust of this important argument is not handled with due sensitivity by my critic. What he says is not, perhaps, strictly wrong, but it is not fully in focus. It creates a confusing impression and points an unsuspecting reader in the wrong direction—to the detriment of the subsequent discussion. Kemp says that applications of a concept to a newly encountered entity are 'rationally underdetermined, such that it is equally reasonable to classify that entity under the concept or exclude it from the concept' (Kemp, 2005, p. 716). 'Rationally underdetermined' is acceptable here because, like 'formal', it is being used as a technical term rather than a piece of everyday speech. More care is needed, however, if Wittgenstein's position is to be described, as Kemp describes it, by saying that it is always 'equally reasonable' to apply or withhold a concept.

Why is this problematic? It is to be handled with care because it is a way of speaking that invokes an ordinary, everyday context. It sounds as if we could always get away with casually identifying the next dog as a cat. Of course we can't get away with this, and Wittgenstein isn't saying we can. Communication would be hindered and the response would be dismissed as incompetent or irresponsible If the words 'equally reasonable' are to be used then more care must be taken to put them in the right context so that they do not

[2] The label comes from M. Hesse (1974). Hesse's general model of a conceptual system, called the network model, has played a central role in the work on the sociology of knowledge. Because Kemp does not engage directly with Hesse's formulations these will not feature as prominently in the discussion as they perhaps should.

[3] For example: 'To obey a rule, to make a report, to give an order, to play a game of chess, are *customs* (uses, institutions)' (Wittgenstein, 1967, §199). Again: 'what is the reality that "right" accords with here? Presumably a *convention*, or a *use*, and perhaps our practical requirements' (Wittgenstein, 1978, I-9).

have this implication, for example by explaining that 'reasonable' here means something like 'as judged by formal, logical criteria'. In the ordinary sense of the word, 'reasonable' means 'acceptable when judged by everyday, practical standards'. These standards however, according to Wittgenstein, contain far more than formal specifications. They embody a rich totality of contingencies and it was central to his project to expose their presence and explain their importance. In this sense, his theory most certainly does not imply that all and any casual applications of a concept are 'equally reasonable'. Some will be deemed acceptable and some unacceptable. That, after all, is the whole point of having conventions and shared standards.

Unfortunately, the distinction between everyday talk, and the far-from-everyday business of analysing the workings of our concepts, is blurred throughout Kemp's analysis—as it is by other critics.[4] It may help to bring these important points into sharper focus if I deal with a simple example and then use this as the base-line with which to assess the main body of Kemp's criticisms.

3. Normativity and the preconditions of reference

Consider a child learning to use a simple, empirical concept such as the species name of a domestic animal. (The subsequent applications of the concept will be part of the user's engagement with the material world, so there is no question of idealism, strong or weak). What is involved in learning to use a word like 'dog'? The correct answer, developed by philosophers such as Wittgenstein and Hesse, is that it is based on ostensive training by a competent concept user.[5] Children are shown instances of the concept, their innate tendencies to generalisation are activated, and their subsequent performances are corrected and brought into alignment with the local usage of the term. This analysis suggests that, in achieving some mastery of the concept, the child has learned two things about its environment. It has learned something about the natural world and, simultaneously, something about the social world. This might be expressed by saying that the child has learned something about dogs and also something about the conventions for uttering the word 'dog'.

The process of ostension does not furnish a full account of reference and does not illuminate all of the diverse ways in which scientific discourse is grounded in reality. It is only offered as the necessary beginnings of such an account so, while both Wittgenstein and Hesse began their analysis in this way, neither of them ended it at this point. The same applies to those who have tried to develop their ideas in the context of the Strong Programme. The point to emphasise is that an account of scientific concept use must include a proper account of ostension. Any attempt to develop a picture of science without giving it due attention—and such attempts are made—would deservedly be called 'idealist', in the

[4] A version of Kemp's 'equally reasonable' error is made by Boghossian who attributes to supporters of the programme the idea that all theories, whether scientific or mythological, are 'equally valid' (Boghossian, 2006, p. 3). The attribution is false and the quotations Boghossian offers to justify it do not assert or imply what he says they do. The quoted material from Shapin and Schaffer is about equal responsibility. The material from Barnes and Bloor is about the equal distribution of curiosity. Neither entail 'equal validity'. Equal validity is explicitly denied in the source that is quoted from Barnes and Bloor but Boghossian appears not to have noticed.

[5] Wittgenstein's discussion runs through part one of the *Investigations*. He is at pains to point out that ostensive definitions can always be misunderstood but, for contingent reasons, ostensive training and definition does work. For a systematic discussion see Hesse (1974, pp. 11 ff.).

sense that talk would be detached from an independent reality and collapse into talk about talk.[6]

The simplified account of the necessary conditions for external reference may be broken down into its psychological and sociological components. The psychological component concerns the sensory experience and motor activity that is involved in recognising, say, candidate dogs. This component includes the innate cognitive tendencies to generalise that are implicit in their identification and re-identification. The sociological component concerns the sensitivity to encouragement and discouragement and the tendency to trust, imitate and emulate other concept users. It involves the capacities which are exercised to sustain communication and co-ordination within a group. Indeed, it implicates all the forms of interaction and reciprocity involved in learning and participating in the conventions that, locally, may be said to 'govern' concept use.

In the body of work criticised by Kemp these necessary components of cognition and language use were described, respectively, by means of two, deliberately idealised, models designed to bring out their most important features. In adopting this method of exposition I was following closely in the footsteps of my Edinburgh colleague Barry Barnes and making extensive use of his important paper 'Social life as bootstrapped induction' (Barnes, 1983). Thus, the psychological component was modelled in terms of a simple, hypothetical pattern-matching device. Sensory input was assumed to be automatically tested for its match, or failure to match, an interior stereotype. Was this putative dog close enough to the pattern constructed from exposure to the instances in the learning set? Each individual was supposed to have such a device at work in his or her brain and it was supposed to operate mechanically. It explains the initial and spontaneous response to an object, though it does not explain what makes that response correct or incorrect.[7] For the sociological part of the story the central idea was that, in applying concepts, the actor is oriented not only to the objects in the non-human environment but also to the orientation of the other actors towards those objects. This part of the story concerned the alignment of these orientations, for example the alignment of the responses that underpin the application of the label 'dog'. This dimension embodies the standards of right and wrong application of the label. It provides the preconditions for explaining the all important, normative part of the story.

The simplified model of the social dimension was called the 'self-referential' model of an institution. Why 'self-referential'? The answer is because, if we focus exclusively on the normative component, if we separate it out and try to discern its inner structure, it transpires that the 'rightness' of the orientation in question refers to nothing outside the shared conviction or acknowledgement of its 'rightness'. Normative concepts do not, on this

6 As an example of an account of scientific knowledge that opens itself to the charge of 'weak idealism' because of a neglect of ostension I would cite the interesting book by Hans-Jörg Rheinberger (1997). I should emphasise that my criticisms are not directed at the fascinating historical account of protein synthesis given in the book but only at the philosophical and methodological gloss that is offered on the history. Rheinberger argues that the resources of analytical philosophy and the sociology of knowledge are inadequate to bring out what is important in the historical case. He recommends and adopts approaches derived from French post-modernist thinkers. I argue the reverse case (Bloor, 2005) and seek to show (i) that Rheinberger needs these resources, and (ii) that his strategy inadvertently leads to an idealist picture. For Rheinberger's response see Rheinberger (2005).

7 The automatic character of the process is important. It allows the model to avoid the obvious objection that it is question begging. For a case where such an objection is rightly made see Ludwig Wittgenstein (1967, §604). The form of the model as developed by Barnes, however, is not vulnerable to this criticism.

approach, stand for some further element in the world alongside the particular objects towards which the actors are oriented. Talk of 'rightness' does not require the existence of some realm of values that is supposed to exist in, or as, a domain distinct from the references made to it. When separated out for special consideration, talk of 'correctness', as such, is not like talk of dogs, though it is a dimension of talk about dogs and a necessary part of it. Its semantic logic, when considered in isolation, does not mirror that of empirical discourse even though it is a necessary part of that discourse.

The logic of the normative dimension is shown to advantage by looking at concepts such as, say, that of 'leader'. The point is that 'leadership' is constituted by imputations of leadership. In a similar way, the rightness of rightly calling something a dog ultimately depends on its being deemed right. The group collectively 'decide' on the norms of proper usage of their concepts and classifications and they create the norms of their correct use in the course of invoking them. They do not collectively discover the norms, as if they were a further feature of the world, even if it may occasionally seem like this to the individual concept user.

Calling the specifically social dimension of the model 'self-referential' is another way of saying that normativity is a matter of convention. The label helps keep in mind the interactions that are involved in sustaining this, and any other, convention. Conventionality involves more than a set of individuals simply moving, as it were, in parallel: it involves an element of monitoring, cross-checking and an ever-present dimension of conditionality. It implies that the behaviour of any one follower of the convention is conditional on the continued conformity of a sufficient number of others.[8] The collective 'decision' to use a concept in a certain way is not arbitrary; it must be one that is perceived to have utility for the group of users and it must be consistent with, and sustainable by, their innate cognitive proclivities—such as the natural operation of their pattern-matching machinery. Although my critic thinks otherwise, this means that the account actually meets the requirement that, as he puts it, there must be some 'constraining effects of the interaction of concept and object' (Kemp, 2005, p. 716). There is such a constraint but it is always and necessarily a socially mediated constraint.

It is vital to appreciate the specific role played by the 'self-referential' component in the full story of empirical or 'other-referential' discourse. Three points deserve attention. First, it is necessary that some adequate account is taken of the normative dimension of concept use because, without it, there can be no coherent conceptual grasp of the world. If a would-be concept user cannot be spoken of as getting the application right rather than wrong, if there is no distinction between a right and a wrong use, and no way for the users themselves to create and apprehend this distinction, then the preconditions of meaning evaporate. Whatever the analyst might then call the verbal response of, say, 'dog', it is not an act of concept application and the mind or brain of the concept user cannot be attributed with any genuine conceptual content. Under these circumstances it would be appropriate to talk of 'weak idealism' and the 'free-floating' character of the attempt at discourse. In Wittgenstein's terminology it would be an attempt, and a necessarily doomed attempt, to use a 'private language' (Wittgenstein, 1967, §269).

8 The classic analysis of conventionality, which has never been improved upon, is by David Hume (1960, Bk. III, P. ii).

Second, there are no other viable accounts of normativity on offer in the literature apart from those which make appeal to convention and social consensus. It is society that provides the standards that are available to, and necessary for, the individual. There is, indeed, much confident deployment of normative notions by philosophers, and a ready dismissal of sociological accounts, but I know of no remotely acceptable analysis of normativity other than those of a sociological character.[9] All too often, rather than being analysed, normativity is simply taken for granted—and we shall see an example shortly.

Third, if a sociological model is on the right lines then all individualist and subjectivist accounts of concept application are clearly in trouble. Wittgenstein put his finger on the source of their weakness. Because, on these theories, there is no external standard outside the individual, then whatever seems right to the individual is right. But that, said Wittgenstein, means that one cannot talk about right in this case at all. There has got to be an external standard of right or wrong concept application and that standard is a social one.[10]

4. A reply to the first objection

Why does Kemp think that the approach I have just outlined lapses into 'weak idealism' and severs the link between discourse and the world? Given that the analysis was meant to be a description of this very link, the grounds of its alleged failure need to be examined carefully. Kemp offers three lines of critical commentary. First, he says that the two components of the overall model, the psychological, pattern-matching part and the sociological, self-referential part, seem to 'exclude' one another and seem to 'place opposing, and arguably, irreconcilable demands on concepts' (Kemp, 2005, p. 713). The argument to back up this feeling, however, never receives a clear formulation. Kemp's worries seem to depend on the assumption that people cannot do two things at once, that is, respond simultaneously to both their material and their social environment. But let us see in more detail how Kemp's worries are given expression.

Kemp accepts that my picture treats the presence of the self-referential component as a precondition of external reference but he resolutely reads this as a temporal sequence. He speaks as if, on this picture, one first learns the rules of the language game and then applies them to objects in the material world. He thinks of it as a process in which propositional content is given to the empirical concept and *then* a connection is made to instances to which the concept applies (see for example ibid., pp. 712–713). I hope my preliminary remarks will have made clear that this is not the story. The content of basic empirical

[9] It is both revealing and distressing to see how ineptly the matter of normativity is handled even by (Oxford) philosophers who are purporting to follow and expound Wittgenstein—but who read him as an individualist. Thus Baker and Hacker (1984) try to conjure up normativity out of some admixture of regularity and complexity. In rejecting a communitarian, that is, sociological, account of normativity they say: 'The claim does not involve insistence on community-aid for solitary rule-followers, but on *regularities* of action of sufficient *complexity* to yield normativity' (ibid., p. 42). How regularity and complexity come to equal rightness is never made clear. Similarly, Colin McGinn (1984), p. 163, considers it acceptable to say: 'If it be asked how this normativity works, then the answer . . . is that it is simply in the nature of meaning to have normative consequences (as it is in the nature of moral values to determine what is right conduct)'. Elsewhere (ibid., p. 138) he grounds meaning in 'natural propensities' but never explains how those propensities generate normativity.

[10] 'One would like to say: whatever is going to seem right to me is right. And that only means that here we can't talk about "right"' (Wittgenstein, 1967, §258).

concepts is not acquired prior to exposure to their instances and, indeed, could not be so acquired. The norms of the proper application of the concept are conveyed and negotiated in the course of its application to particular instances. This is why I spoke of the child simultaneously learning about the social and non-social aspects of the environment.[11]

In order to highlight the properties of the norms that relate to empirical concepts I used examples of what might be called 'purely' social concepts, that is, concepts that embody things like rules, rights, promises and statuses. To illuminate the processes at work in the social dimension I moved from empirical concepts to cases where the immediate reference was focused on a feature of social reality, for example to a status. Such examples were meant as extreme cases to bring home the nature of discourse about the *status* of being right or wrong with regard to an act of concept application, *even when the concept applies to the non-social, material world*. They were meant to provide a model that could be used when thinking about the normative dimension whose presence was necessary in all concepts. My critic contrives to creates a sense of puzzlement by using formulations which refer to these 'purely' social cases and juxtaposing them with sentences that refer to empirical concepts, without properly articulating their relationship to one another. Thus he says:

> In order to be self-referential, a concept usage has to refer *only* to other concept usages . . . By contrast, if a concept is to refer externally, it has to make reference to some outside object, not to other concept usages. (Ibid., p. 713)

I think my critic has got himself into a verbal muddle over the word 'only'. His difficulty might be illustrated by a simple analogy. Consider the following two questions. First: 'How can a company that *only* trades locally become part of a company that *only* trades abroad?' Here the question is a good one. Somebody has got to shut up shop or change their business plan. This might be contrasted with a second, somewhat different, question: 'How can a company that *only* trades locally become part of a company that will *also* trade abroad?' Here there is no problem. My critic thinks the merger I am discussing is like the first case. It isn't: it is like the second. Correctly reformulated the quoted passage would read: In order to be purely self-referential a concept usage has to refer only to other concept users. If, however, a concept is to be used to refer externally, it has to make reference to some outside object, and in order to do this norms of correct usage are involved, and it is these norms which have a self-referring character.

Given that the oppositions and exclusions of which Kemp complains are not truly present in the theory how could they have been read into the text? My suspicion is that my critic is simply presupposing that attention to the natural and social worlds are exclusive (as if nobody can *both* trade locally *and* abroad.) Speaking of the two components of the account of reference, that which makes a link to the non-social world and that which provides the norms of application, Kemp says that 'it is hard to see how they can be combined' (ibid., p. 713). It might have helped if he had approached the problem concretely by means of an example. Had he started, say, with a child learning a word, then he might

[11] As evidence for his sequential reading Kemp cites Bloor (1999), p. 109. There is, however, no reference to sequential processes on that page, and nor should there be. There is, however, the sentence: 'The link between self-reference and external reference is that the latter presupposes the former'. But to say that A is presupposed by B does not mean that first A happens and then B happens. Kemp has read a logical condition as a temporal sequence.

have experienced less difficulty working with a picture that combines inputs from both the material and the social environments.[12]

So much for the first line of criticism. Next, my critic argues that the self-referential component, which I say provides the necessary normative element, is actually dispensable. The pattern-matching part alone, he argues, could ground normativity. He does not really think that the mechanism of pattern-matching is a good model of the individual learner (ibid., p. 715) but uses this line of thought in an attempt to show the redundancy of the self-referential account of co-ordination and conventionality. He puts it like this:

> If it [the pattern-matching machinery] produces stable outcomes . . . then we can take the pattern-matching machinery to be functioning well. If it does not, then we can take it to be functioning badly. This being the case, we have grounds for assessing the discriminations of pattern-matching machinery, and therefore, it does make sense to say that the machinery's discriminations are normative, and have content. (Ibid., p. 714)

So the stability of the output of the machines is proposed as the criterion of correctness of the output and this, it is said, is a sufficient basis for normativity in itself.

It is important to see why this misses the point. To identify the target he wants to criticise, and to exemplify the position he rejects, Kemp quotes me as saying, 'The pattern-matching process has no measure outside itself to determine whether the matching is going well or ill'. The crucial words here are 'outside itself'. The point at issue is whether an individual's brain processes—or the pattern-matching machines which model these processes—can generate normativity out of their own, unaided resources, that is, without a surrounding society or some equivalent. This is what my quoted words say cannot be done and what Kemp believes can be done. Although Kemp quotes these words he shifts the ground of the discussion and actually addresses a different problem. Instead of sticking to the question of whether a pattern-matching machine can, of itself, generate norms, he asks whether the machine can be *subject to* norms or *used* as the basis for specifying norms. He proceeds as if I had asserted (in his words) that 'there can be no . . . judgement about mechanical pattern-matching' (ibid.). But of course there can be such judgements. The problem is not whether there can be judgements *about* mechanical pattern-matching (by some unspecified body of concept users), but whether there can be such judgements generated *within* the pattern-matching process and *by* the pattern-matching process.

If we examine the quotation from Kemp given above it becomes clear how the shift from 'within' to 'about' is exploited in his argument. It is, of course, easy for him to prove that there can be judgements *about* the pattern-matching. All that is necessary is to introduce some group of social actors who decide to apply their own criterion of sameness to the outputs or who declare that they will use the deliverances of the machine to be their

[12] The late Prof. Anscombe, Wittgenstein's pupil and translator, developed a precursor to the self-referential model that she called by the perhaps unfortunate name of 'linguistic idealism'. It was designed to illuminate rules, rights and promises and to link Wittgenstein's work with that of Hume on convention. See Anscombe (1976), Vol. 1, Ch. 13. The claim that norms have a nature which calls for an analysis in terms of 'linguistic idealism' may have been misread by critics as compromising the independent reality of the objects of discourse. Anscombe herself warns against this misreading.

norm of correct response. And this is exactly what Kemp does, hence we read in the quote given above that 'we' can take the machinery to be functioning well, and 'we' have grounds for assessing the discriminations. But who is this 'we'? If the terms of the original problem are to be retained it is necessary to hold on to the fact that the 'we' and the pattern-matching machinery *are supposed to be one and the same thing*. Kemp trivialises the problem by letting this crucial point slip through his fingers.

Proceeding as he does, Kemp has not explained at all how the pattern-matching machinery of the individual brain, in and of itself, can generate real and non-subjective standards. This was his goal but it has not been achieved. He has simply jumped outside the machine and postulated a group who accord a certain status to the machine's outputs. This sheds no light on where this status comes from or what its nature is. What sorts of interaction, within the group called 'we', can constitute or sustain the normative standards that 'we' apply? I have given an answer to this question by appeal to the self-referential model, but Kemp has not. Kemp's appeal to the stability of the pattern-matching machinery has begged the important questions and revealed nothing about the source or nature of normativity.

In his third line of criticism Kemp draws attention to my use of Wittgenstein's slogan that where there is no external standard, and where 'whatever seems right is right', then we can no longer, justifiably, speak of right. He seeks to turn this against me by noting that even if, as he thinks, Wittgenstein's claim does not apply to a pattern-matching machine, it must apply to an isolated group (ibid.). Why, he asks, is a group supposed to be in a better position than an individual when it comes to generating standards? Why is a sociological theory supposed to be better than a purely psychological and individualistic theory? As Kemp puts it: 'Whatever seems right to the community is right, which means that the word "right" has no real application here' (ibid., pp. 714–715). Thus the sociological account of normativity has, supposedly, destroyed itself.

This is a line of argument that has frequently been used by philosophers against sociological accounts of normativity.[13] The objection is, however, easily answered. The group, taken as a whole and viewed, as it were, from the outside, is indeed in the same position as the isolated individual or machine. It cannot overcome the ultimate subjectivity of its collective achievements or justify them in terms of any higher objectivity. None of this, however, undermines the arguments that I have been developing. The argument up to this point, as I have been at pains to stress, has been about the need to see the individual operating within a group. The group is external to each of its individual members and thus its collectively sustained standards are external to the subjectivity of each of its members. The claim has been, simply, that a group can furnish a standard for the individuals within it. The inability of the group to perform this same service at the higher level (for itself, taken collectively) is simply a fact that has to be accepted. Fortunately it can be accepted without relinquishing the earlier conclusion.

Wittgenstein was right when he spoke of rules and concept use having their only justification within a 'language game' while the 'games' themselves can have no deeper or ultimate justification. In the end it simply has to be accepted that this is what we do and this is

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[13] For example, it is used by Baker & Hacker (1984), p. 37, and McGinn (1984), p. 188, and a version of it is used by Kripke (1982), p. 111.

how things work (Wittgenstein, 1967, §217).[14] Language games, for Wittgenstein, are clearly determined by a combination of our animal nature and our social institutions though he characteristically stopped his investigation before confronting any of the obvious causal questions this poses. But he never shirked the point that justifications must come to an end. It stops with the conventions of the language game and the circumstances that give them their substance and structure. This was Wittgenstein's way of formulating the ultimate commitment to relativism that necessarily pervades his work, and which finds its echo in the relativism of the Strong Programme.

It is important to understand what relativism means in this context. Relativism is the negation of absolutism; it is not the opposite of materialism. A commitment to relativism is not a commitment to idealism. It is possible to be both a relativist and a materialist. In the past, materialist–relativist thinkers have played a significant and honourable role in our culture, though they have been effectively written out of the history of philosophy.[15] But the overall message is: relativism should never be confused with idealism though, deplorably, the confusion is endemic in the philosophical literature.[16]

5. A reply to the second criticism

What is the relation between scientific reasoning and the account given of it by sociologist and historians in the course of their own investigations? Scientists typically advance reasons for, say, accepting or rejecting a certain theory and the question at issue is how these reasons should be dealt with. If I correctly understand Kemp, his own view is that the reasons proffered by scientists should be taken at more or less face value. He believes, however, that this is not the view of those who follow the Strong Programme. He speaks of social constructionists 'challenging' and (ultimately) rejecting the scientist's reasons in favour of their own different, and incompatible, sociological account of why a theory is accepted or rejected. For the constructionist the scientist's evidence is treated as 'rationally unconvincing' (ibid., p. 716).

To introduce my response I want to say something about the general orientation behind the Strong Programme. When it was formulated in the early 1970s it was not offered as a novel approach or a way of telling other scholars what they ought to be doing. Rather than being prescriptive it was largely descriptive. The aim was to codify the assumptions and practices of the exciting work that was then being done on science, especially by historians. This work was all the more admirable for being done in the face of a barrage of

[14] Wittgenstein wanted to treat language games as basic and told his readers not to seek to explain them: for example, §654–655. The 'explanations' that Wittgenstein was particularly keen to stop were rationalistic justifications as much as causal explanations, though there is no denying that, on occasion, he adopted a negative attitudes towards causality as well. It must not be forgotten, though, that Wittgenstein's references to customs, conventions and institutions are themselves embryonic sociological explanations.

[15] For the role of both the government and the pre-Darwinian scientific establishment in suppressing the radical, nineteenth century representatives of this tradition see: Desmond (1987).

[16] Sadly the confusion is even embedded in the reference books where it is reported but allowed to pass without comment, thus: 'Since relativism denies an objective, independent reality in virtue of which beliefs are true or false, it has been held a disguised form of idealism' (Newton-Smith, 1981, p. 369). See also the entry on relativism by L. P. Pojman in *The Cambridge dictionary of philosophy* (Pojman, 1999, p. 790) where it is stated that, for relativists, 'the world has no intrinsic characteristics', that is, all such characteristics are projections of the beliefs of the knowing subject.

bullying attacks from philosophers who wanted to reify and ring-fence 'reason' and who effectively treated the 'internal logic' of science as if it were an ahistorical, self-propelling and autonomous force.[17] For these philosophers, society came into the story merely as the precondition of science, not as a constituent of knowledge. Society facilitated or impeded the autonomous growth of rationality. Fortunately, the historians did not allow themselves to be browbeaten. They resolutely traced out all that was contingent, local and historically variable and scrutinised all aspects of science, without fear or favour.[18]

How did the historians handle the reasons put forward by the scientists they were studying? Rather than challenging or rejecting them it was clear that the reasons were taken seriously. There was no doubt that they had the power to convince because, in fact, they did convince at least some of the historical actors. Nor was there any doubt that, in general, the actors, convinced or unconvinced, proceeded with exemplary rationality. The actors' reasons were thus accepted by the historians as the starting point of an analysis that was intended to delve deeply into what lay behind them. But why were the reasons not taken to be self-sufficient and self-explanatory? Why were they only the starting point and not, as the philosophers wanted, the finishing point of the enquiry?

To understand the historian's procedure, consider the reasoning that might take place in the context of a scientific dispute. All the opposing scientists would typically advance evidence and reasons but: (a) the opposed parties would frequently make different selections from the range of facts that might have been cited, and (b) they often put different interpretations on the same experimental or observational outcomes and often attached different degrees of significance to facts even when their interpretation was the same. Furthermore (c) the terms of the debate could themselves be seen as historically contingent and neither compelling nor necessary.[19] For the historians, then, the deployment of reason by the scientists posed a problem, and the answer to the problem was neither obvious nor provided by the scientists themselves. The problem was: why do the proffered reasons typically convince some scientist but not other scientists?

Clearly this problem did not arise because the analysts were challenging the scientist's argumentation but for the opposite reason. The task arose from the analysts' close and sympathetic engagement with the scientist's argumentation, accompanied by an awareness that the argumentation itself does not contain the answers they were looking for. The historians' response was to contextualise the ideas and arguments under examination. They looked for the intellectual traditions into which the competing parties fell, the institutions with which they were associated, and the goals and interests that might be behind the argument. In this way they could illuminate the unspoken assumptions and the taken-for-granted tendencies that were at work. The historians routinely exercised their curiosity in a wholly general way, seeking to contextualise the behaviour of all the parties to the dispute. This was registered in the idiom of the Strong Programme as the principle of 'symmetry'.

Given this picture, what are we to make of Kemp's claims? Referring to the reasons advanced by scientists to increase or decrease the credibility of a theory, he says: 'social constructionists deny that these kinds of reasoning process provide an adequate explanation of

[17] As an example of the attacks see Lakatos & Musgrave (1970). For an exposure of the reactionary and theological character of the assumptions behind the attacks see Bloor (1988).

[18] For a still very useful bibliographical study of the historical work in question see Shapin (1982).

[19] In framing the issue in this particular way I am again following Barnes (1984).

how differences in credibility are achieved' (ibid., p. 715). He is quite right—but only if one keeps in mind that the inadequacy in question is relative to the purposes of the historian. The historian wants to know why, say, a given experimental result seems important for some scientists but not for others. Both groups may give reasons for their stance, but that only displaces the question. The historian now needs to know why the proffered reasons move some scientists but not others. The difficulty with Kemp's account is that he appears to overlook the way in which the inadequacy of which he speaks is an inadequacy for specific purposes, namely those of the historical analyst. He argues as if this inadequacy is seen and presented by the historian as a shortcoming in the science itself.

Kemp may have been led to his conclusions, at least in part, because he blurs the distinction between the perspectives of the actor and the analyst. He does this by repeatedly treating both of them as dealing with one and the same commodity, namely 'credibility'. For example, he treats the social constructionist as being at odds with the scientists who advanced arguments in favour of Madame Curie's claim to have discovered a new element called radium. He expresses the point as follows:

> The clash between scientific and social constructionist perspectives can be illustrated by . . . the case of Marie Curie, and [by] analysing competing explanations of the high scientific credibility of her claims to have discovered a new element. (Ibid., p. 716)

But have competing explanations of credibility really been put forward? Did supporters of Curie really seek to *explain* the credibility of her theory or did they seek to increase its credibility? To my ear Kemp's manner of speaking is strained and invites confusion. If it is not to lead us astray it is vital to keep in mind that there are quite different ways of orienting to credibility. Scientists certainly want to modify the credibility of a theory in the eyes of other scientists, but they are not typically in the business of investigating or theorising about the phenomenon of credibility itself, in the way that the historian or the sociologist is.

There are certainly complexities that would have to be introduced into my picture of the divergent perspectives of the actor and analyst if a fully general treatment were in question. The line between actor and analyst might be blurred, for example, by a sociologist who deliberately intervened in the first-order, scientific debate under study. There might also be occasions when, say, the historian could not resist the conclusion that a reason advanced by an historical actor was not the real reason that was at work. Again, scientific actors sometimes proffer their own quasi-sociological accounts, usually to explain the behaviour of those who disagree with them. Nevertheless these cases are best understood when set against the backdrop of the typical cases in which there is a clear difference between, for example, the physicist interested in electrons or the engineer interested in bending beams, and the historian or sociologist who is interested in research schools, institutional differences, disciplinary orientations, pedagogic traditions, national styles, patterns of training, and the influences of war and ideology. The objects of their respective curiosity are clearly disjoint.

6. Instrumental success and negotiability

In the last section my reply was based on the assumption that the Strong Programme effectively embodies the excellent practices of historians of science. This argument is vulnerable to the objection that, despite the attempt to ape the historian, extra assumptions might have been introduced and it is these assumptions, peculiar to the programme, that give cause for complaint. Kemp could field such an argument by appealing to the claims he made in the earlier paper called 'Towards a monistic theory of science' (Kemp, 2003). I shall now address the central argument of that paper, beginning with an explanation of what, in this context, is meant by the term 'monism'.

Supporters of the Strong Programme have always opposed the assumption that the rational and the social are fundamentally different in their nature so that, for example, social and psychological causes explain deviations from rationality rather than being internal to the operation of rationality itself. The programme was formulated in opposition to those who would erect a fundamental dualism of the rational and the social. In this sense it expresses a form of monism and, in principle, Kemp agrees with this stance. He does not, however, think that the desired monism is correctly stated in the Strong Programme. He wants a monism in which the interests in prediction and control, that is, the instrumental and practical aspects of knowledge, are unified with social processes and interests in a way that, in his view, is absent in the Strong Programme.

What is supposed to be missing? Kemp draws attention to the obvious effects of success and failure in dealings with the material world. They can have a profound effect on behaviour, for example in providing incentives to join successful projects and disincentives to join failing projects. Success rewards and encourages certain lines of development in our thinking and failure punishes and discourages other lines. According to Kemp these pragmatic, feedback effects do not receive due recognition by writers such as Barnes and myself. Kemp accepts that we claim to take them into account but, he says, in reality we do not and we cannot square them with the overall thrust of our analysis. The pragmatic dimension is acknowledged on the level of generals principle but betrayed by the main analysis of knowledge. It is contradicted by the stress we place on the negotiability of the categories and classifications that make up the conceptual network of science. The monistic pretensions of the Strong Programme are thus compromised. Kemp says:

> It seems pretty clear that on such an approach, instrumental success and instrumental failure are no longer distinguishable. (Ibid., p. 323)

The reason why it seems clear is because, according to Kemp, on the Strong Programme analysis:

> Whatever the interests pursued by actors, this can be said to be fulfilled by their current beliefs and classifications. (Ibid., p. 324)

As evidence Kemp produces a number of quotations from Barnes of which the following two are representative. Barnes says:

> A whole conceptual fabric can always be *made out* as in perfect accord with experience . . . (Quoted ibid., p. 323)

and

> Whenever anything of nuisance value arises out of experience, it can always be deemed a new kind of thing or event, and assimilated under a new concept, leaving the existing structure unaltered. (Quoted ibid., p. 323)

Kemp concludes that

> SPers [that is, advocates of the Strong Programme] ultimately remove issues of instrumental success and failure from their account . . . (Ibid., p. 321)

I do not think Kemp's conclusion follows because I do not believe that he has accurately rendered the meaning of the words quoted from Barnes. The error in Kemp's gloss turns on his failure to respect the important difference between what *can* be done and what *is* or *will* be done. He has, I believe, conflated the logical or formal parameters of the analysis with its causal parameters. Barnes is not removing issues of instrumental success or failure from the account, he is articulating the formal framework within which such issues have to be addressed, by both the actor and the analyst. He was, by implication, locating the pragmatic aspects of success and failure where they should be located—that is, *not* in the formal properties of some conceptual system, but in the sociology of the concept user's collective commerce with the material world.

Because the point is so central I want to address it in two quite different ways, going over the ground, as it were, from different directions. First, I want to come at it in simple, general terms and then, secondly, go over Kemp's text with some care to identify the shifts and equivocations in his account of the matter. The justification for traversing the terrain a second time is that Kemp's errors are representative of a recurrent failure in the critical literature.

First, the more general treatment. This can be provided by using the familiar, symbolic form of Duhem's argument about crucial experiments (Duhem, 1954, Ch. VI). The symbols can be taken to represent a conceptual system, but one with just two or three main propositions in it called A, H and O respectively. Suppose H is an empirical hypothesis while A represents the background assumptions and O represents an observation statement, or prediction, derivable from the conjunction of A and H. Duhem argued, correctly and powerfully, that no hypothesis can be tested in isolation from a set of background assumptions. The prediction depends, logically, on both. This means that if the prediction comes out wrong then the blame could be placed on either one of them. It does not attach, unequivocally, to one or the other. Using $\sim$ to represent negation, and . for conjunction, and $\rightarrow$ for implication, the standard expression of the argument is:

$$\begin{bmatrix} \mathrm{A.H} \rightarrow \mathrm{O} \\ \sim \mathrm{O} \\ \sim \mathrm{A} \text{ or } \sim \mathrm{H} \end{bmatrix}$$

Logically the failed prediction permits the conclusion $\sim$H or the conclusion $\sim$A. Placing the blame for the failed prediction on H means rejecting the hypothesis. Placing the blame on A opens up the possibility of rescuing the hypothesis from refutation. A modified version of the background assumptions may always be found, though perhaps at a high cost, which brings the prediction back into alignment with observation. The argument can, of course, be run in the reverse direction too. An initial agreement between prediction and observation, where A.H $\rightarrow$O, and O is observed, can be disrupted by modifying A. Read in the usual direction, this argument identifies the procedures for restoring a form of agreement between a body of propositions and reality. It does not guarantee that people will be clever enough to think up the required rescue moves nor that the moves will be acceptable to the community of scientists (given all the other demands to be met by their theories) but it does show what is logically possible.

This textbook argument allows us to restate the points that are at issue between Kemp and the supporters of the Strong Programme. First, notice that it exemplifies the point made in the quotations from Barnes: the conceptual system can always be made out to be in accord with experience. But, secondly, it seems (at first glance) to support Kemp's account of the situation. Does not Duhem's argument show that, in Kemp's terms, corroboration and refutation 'are no longer distinguishable'? If Duhem was right would it not mean that issues of success and failure have been 'removed from the account'? Has not Duhem effectively rendered the conceptual system 'free-floating' thus committing himself to a form of 'weak idealism'?

The answer is no. This is not the implication of Duhem's argument. To see why, it is worth recalling how Karl Popper reacted to Duhem's argument in his classic *Logic of scientific discovery* (Popper, 1959, Ch. 4). Given that Popper was emphasising the importance of exposing theories to test and refutation, and Duhem was, from the Popperian standpoint, exhibiting the mechanics for 'evading' refutation, it might be expected that he would challenge and reject Duhem's logical analysis. In fact, and quite rightly, Popper did nothing of the kind. He accepted the logical point. The response that he made hinged on the difference that I pointed out above, between what can be done and what is done or, in Popper's normative version, between what can be done and what ought to be done. Popper said that (logically) Duhem was right, we can always evade refutation; it is just that (methodologically) we should not do so. He shifted the discussion to the normative plane and made what he called a 'methodological decision'. He declared that scientists should operate by resolving not to rescue H by adjusting A. In effect Popper supplemented Duhem's logical analysis by adding a policy statement for the proper conduct of what he called the game of science.

Whatever one may think of the policy there is no doubt that the two parts of this picture, the policy and the logic, engage with one another. The overall pragmatic level which embodies the feedback between experience and theory (represented here by Popper's methodological policy) is not disconnected from the details of the conceptual mechanics (represented here by Duhem's argument). There is nothing, in principle, about either of them that precludes their being put into contact and formed into a unified, procedural whole. Corroboration and refutation have not, in Kemp's words, become 'indistinguishable' and success and failure have not been 'removed from the account'. There is no problematic 'dualism' of logic and method so the account is appropriately 'monistic'.

Now let me draw out the parallels with the argument over the Strong Programme. Kemp asked, of the supporters of the Strong Programme, 'whether their theoretical system of analysis can properly incorporate such a concern' (Kemp, 2003, p. 322), that is, can their account of the negotiability of concepts properly incorporate the pragmatic and instrumental concerns that so obviously inform science. He returned a negative answer. But again one must ask: what is supposed to be the problem? The example above, using Duhem and Popper, shares precisely the same structure and shows that Kemp is wrong to think the two things exclude one another. Pragmatic and instrumental concerns can be, and are, incorporated despite their being applied to a conceptual system that can be manipulated and negotiated in all manner of ways. These two ingredients are not antithetical.

One may think that Popper's conception of the pragmatic dimension is inadequate and that the concern with falsification has produced a one-sided account. But even if this were right, such a response would miss the structural point of my argument. Nothing in my reply to Kemp actually hinges on using Popper's preferred policy of refutation, that is, always blaming H rather than A. The same conclusion would follow if we imagined another, quite different, policy at work, for example that of protecting H and always blaming A. To sharpen the contrast this might be called the 'Kuhnian' policy. The symbol H may be taken to represent a 'paradigmatic' achievement, though the logical schema hardly does justice to Kuhn's actual picture of science (Kuhn, 1962).[20] Nevertheless, the policy of retaining H captures something of the conservative tendency to protect central insights and symbolises distrust of the idea that 'new' means 'better'. From this point of view, adjusting A is not 'evading refutation', it is protecting an idea from premature rejection. It gives advocates of H an opportunity to exploit its potential to the full and embodies the patience and tenacity that informs good scientific practice.

It is noteworthy that my critic aligns himself with the Popperian policy. For Kemp, adjusting the background assumptions to sustain a theory or an approach is identified as wrong. He believes that it has the effect of detaching the users of a conceptual scheme from any real engagement with reality. He presents it as a policy which diminishes the instrumental efficacy of a body of knowledge. Thus:

> I would argue that when the genuine instrumental concern of action is taken into account, the kind of reclassifications advocated by Barnes cannot make sense to actors. (Kemp, 2003, p. 325)

The reference here is to Barnes's use of Duhem's argument to describe the scope for conceptual adjustment. I think Kemp's claim is factually false, and false on a massive scale. Duhem-style reclassification makes very good sense to scientists who have a genuine concern with instrumental efficacy. Indeed, it is one of the central ways in which they give expression to that concern. All theories that endure over time do so because their users deploy Duhem-style adjustment and reclassification. Such adjustments are amongst the most pervasive of the cognitive methods of science and their description provided the factual substance of Kuhn's classic work.

Not only do such manoeuvres make sense to pure scientists, they also play a central role for technologists and applied scientists. My own current work on the history of aerodynamics allows me to provide some striking counter examples to Kemp's claim. The mathematical and experimental basis of modern aerodynamics was developed by Ludwig Prandtl and his school in Göttingen in the years before and during the First World War. Their theory of the flow of air over a wing was formulated using the mathematics of an 'ideal' or 'perfect' fluid, that is, an imaginary fluid with no viscosity. This conception of the nature of air is empirically false, and was known to be false, but it proved instrumentally valuable to the highest degree. The account worked (over a certain range of conditions) precisely because it was sustained by a series of ad hoc assumptions and categorisations, for example the Joukowsky hypothesis that the flow of fluid must be smooth at the trailing edge of the wing, a requirement that was wholly inexplicable on the theory. Those workers in the field who insisted in bringing viscosity into their account, rather than finding ways to sideline it, that is, those who did *not* protect their mathematical

[20] The reason why the symbolism does not do justice to Kuhn's vision is because a paradigm is not a system of propositions: it is an exemplary achievement which embodies all aspects of scientific practice, both propositional and non-propositional.

fiction from refutation, were the ones who paid the price. They became bogged down in unmanageable complexity and intractable mathematics.[21]

I now want to set the 'Popperian policy' alongside the 'Kuhnian policy' in order to make three, more general, points. First, the existence of the two policies serves to remind us that there is no single way to express an instrumental or pragmatic orientation to the world or to give it expression in a system of concepts. There is no unique way to learn from experience. Both the 'Popperian' sequence of conjectures H_1, H_2, H_3, etc. and the 'Kuhnian' sequence of adjustments A_1, A_2, A_3, etc. can be seen as schematic learning curves.[22] Furthermore, the simplified contrast of the policies, and the systematically different bodies of empirical knowledge that they would generate, immediately highlights the factors of interest to the sociologist of knowledge. Policies are collectively sustained, social phenomena. They are realities whose nature is illuminated by the self-referential model. Something is the policy of a group only because, and in so far as, it is accepted as a policy. Orienting to it as 'the policy' is what makes it 'the policy'. Similarly with 'paradigms'. Something is a paradigm because it is collectively used as a paradigm.

What explains the preference for one or the other policy or paradigm? We cannot just say: well, perhaps one policy or paradigm is successful and the other is a failure, so the issue is purely 'rational' and not 'social'. This formulation fails for a number of reasons. There is no single, privileged set of criteria of success but a multitude of possible criteria. What counts as success depends on the goals and goals very, from group to group and time to time. The choice and employment of the preferred set needs explanation. Again, there is frequent disagreement about the degree of success or failure, the meaning to be given to advances and setbacks, and the proper response to make to them. Clearly, all of these issues take the discussion back to the analysis of scientific disputes introduced earlier in my account. The deployment of rational and pragmatic criteria are simply part of the overall sociological story.

Second, the symbolic form of the Duhem argument reminds us that not all of the important epistemic features of a body of knowledge are represented in the network of concepts to which it gives rise. Notice, for example, that the properties of the pattern-matching machinery (the psychology of the individual concept user) stands outside the network. Again, notice how the methodological policies that I have dubbed 'Popperian' and 'Kuhnian' also exist outside the symbolic picture of the conceptual network that I used to present Duhem's argument. The policies are not represented in the symbolism but are external to it. In her more realistic (but still schematic) 'network model' Hesse did not invoke either of the two simplified policies just described but called the class of such policies the 'coherence conditions' of the conceptual network, and correctly located them outside the network itself. They are the determinants of the network, not part of the network.[23] The same point applies to any of the social interests that inform a body of knowledge. These interests will, after a fashion, come to be inscribed in the network of concepts and laws but they will not necessarily find any explicit representation within it. Even when they do receive some expression within the network, that representation is

[21] For the background to these claims see: Glauert (1926), Ch. ix, and Eckert (2006), Chs. 1 & 2.

[22] Popper's methodological policy requires that when H_1 is rejected and replaced by H_2 then H_2 must explain everything that H_1 explains, plus the content of the observation report $\sim$O that led to the refutation of H_1. This prevents the sequence of conjectures from lurching from one random guess to another.

[23] On coherence conditions see Hesse (1974), pp. 51–54.

unlikely to give an accurate account of their operation. In general these policies, methodologies, goals and interests will only be revealed, and competently described, in the researches of historians and sociologists. It is their job, and an essential part of their contribution to the understanding of science, to identify the methodological policies and decisions that are at work, assess the merits of various attempts to describe them, and to explain their causes and distribution.

Third, I want to identify a common, structural theme running through the discussion. Wittgenstein argued that if nothing but the formal properties of rules were operative then it would destroy rule-following as an institution. The institution exists therefore, he concluded, ultimately we follow rules in a routine and customary fashion. Popper, Kuhn and Hesse argued, in effect, that if nothing but the logical relations between scientific concepts were taken into account then knowledge would be impossible and incoherent. They therefore injected coherence into the account by introducing a variety of devices under a variety of names: decisions, conventions, precedents, paradigms, models, metaphors and coherence conditions. Duhem, himself, argued that the practical limitations on the negotiability and fluidity of the network came from a mixture of what he called 'good sense' and 'faith'. For all of these thinkers the dualism of the formal and the informal is overcome, at least in principle, by the way in which the informal mechanisms act as norms for the utilisation of the formal machinery. The different character of their accounts results from the different levels of abstraction and historical sensitivity, as well as the different ideological purposes, informing their descriptions.

Supporters of the Strong Programme have been able to learn from all these thinkers precisely because, at a central point in their analysis, they have concentrated on this common structural theme and have not been unduly distracted by the differences that undoubtedly exist between them. They have learned from them by seeing them all as offering a more or less transfigured account of the necessary social dimension of knowledge. This is why attention has been paid to Wittgenstein's gesture towards customs and institutions and why he has been read as a sociologist of knowledge (Bloor, 1983, 1997). This is why Hesse's coherence conditions have been reconfigured as social interests (Bloor, 1982). It is also why Popper's methodological preferences have been seen as expressive of the ideology of individualism and the competitive market while Kuhn's have been seen as representative of the tradition of conservative thought as identified by Karl Mannheim (Bloor, 1991, Ch. 4; Barnes, 1982). All these thinkers, in their different ways, show that without the social dimension there would be no coherence and no knowledge other than an anarchic subjectivism. That they have drawn on quite divergent social models to express this insight only serves to highlight the common factor that remains salient and visible from these very different vantage points.

The common factor in all cases is that specific acts of concept application can be, and need to be, illuminated by identifying the social context in which they take place. This contextualisation does not render the act of concept application any the less about the real, independent world but helps identify the specific way the concept user engages with the world—there being no uniquely rational or non-social way available. This is where the account of concept application which began with ostension finds its full expression and development. What begins as a discussion of child learning ends with an exploration of the wide range of interests, policies and preoccupations that impinge on the process. The claim is not a sequential one, as if the first steps are innocent of broader cultural concerns, whether identified as, say, pragmatic or political. These things are always present.

The child is, after all, being introduced into a culture which is preformed by some currently operative versions of these things.

Working in conjunction with one another the historical, sociological and philosophical supporters of the Strong Programme have made a systematic effort to develop these themes both empirically and theoretically. There is much that is yet to be done, but it would be difficult to find a more sustained attempt to demonstrate both in principle and in detail of how interests, conventions, policies, customs and goals are woven into the fabric of scientific concepts and classifications. Perhaps the most unfortunate feature of Kemp's response to the Strong Programme is that he fails to see the common thread of argument picked up from writers as diverse as Duhem, Wittgenstein, Popper, Kuhn and Hesse. The common factor is the attempt, by different means, to overcome a certain sort of dualism and hence to create a certain sort of monism.

Having drawn out these general themes I now come back to Kemp. We shall see how they fail to register in my critic's reading of the Strong Programme. He rightly looks for a form of monism in the Strong Programme but, bafflingly, manages to see only dualism. This failure produces some extraordinary results. A close engagement with Kemp's text will also allow me to confront what is, polemically, the most striking part of his attack.

7. Fantasy and reality

The constraining effect of reality has already been mentioned. Its correct analysis has long been a point of dispute in discussions of the Strong Programme and it plays a central role in Kemp's attack. Consider the following three characterisations all taken from the earlier of the two papers. First, Kemp (2003) says that, for the programme's supporters:

> the pursuit of social interests and the pursuit of instrumentally adequate knowledge are not antithetical to one another. (Ibid., p. 312)

The correct formulation, however, is that these interests are not *necessarily* antithetical to one another, not that they can *never* be antithetical. This is not a minor slip; it is central to the entire picture that Kemp paints of the programme. Thus:

> To put this thesis another way the SPers claim that the logics of interest and instrumental adequacy are not separate and potentially conflicting but are one and the same. (Ibid.)

No. We do not claim this. Political and instrumental interests are, indeed, *potentially* in conflict, that is, *can* conflict. To see this one need look no further than the current conflict in the USA between the religious right and scientists working in stem-cell research and evolutionary biology. The point is that there does not have to be conflict between something identifiable as a political ideology and the pursuit of instrumentally adequate knowledge. The discovery, by SS doctors in Hitler's Germany, that smoking is a cause of cancer is a disconcerting case in point. They reached this conclusion long before the post-war work by Sir Richard Doll in the UK to whom the result is usually attributed. The earlier research was sustained by the ideology of racial hygiene, but it generated competent epidemiology and cogent causal conclusions (Proctor, 1999).

My critic's confusion over conflicting interests stands out with particular clarity in the following passage. Kemp starts by saying that according to supporters of the programme there is 'no essential difference' between socio-political interests and those related to prediction and control. If this means that they do not *have* to conflict then, so far, so good. Their essential nature does not necessitate a conflict. But he then goes on to say:

> In other words, socio-political interests are always also to be understood as predictive and oriented to dealing successfully with the environment. (Kemp, 2003, p. 318)

But this is not the initial claim expressed in other words, it is a different claim. To say that the two things do not belong to kinds which have to conflict is not rephrased by saying that they will always operate in a successful, mutually reinforcing manner. Kemp has slid from one proposition to a quite different proposition without, apparently, noticing the difference. But the first is true and the second is false.

After such a start it is perhaps no surprise that Kemp then goes on to impute to supporters of the Strong Programme some truly extraordinary views, for example the idea that there is no difference between success and failure in practical action. If Kemp is to be believed, those who subscribe to the programme present scientists as inhabiting a veritable world of fantasy, a world devoid of failure and frustration. Thus:

> instrumental success does not need to be striven for by investigation: it can be defined by fiat, regardless of experience. (Ibid., p. 324)

Kemp takes as an example the cure for cancer. Do scientists want to cure cancer? Apparently (on Kemp's rendering to the Strong Programme) there is no problem. Whatever the evidence, it can be made out to mean that a cure is at hand and is being successfully applied. Our interventions, whatever they may be, can be understood as cures and no one need be understood as dying from the disease. If this is what supporters of the Strong Programme have committed themselves to then it is easy to see why the charge of 'weak idealism' has been laid at their door.

Why does Kemp read the situation in this way? Perhaps there is a misunderstanding about the role played in the Strong Programme analysis by sensory experience. In the present case this means the role played by the sights and sounds of people afflicted with cancer and dying of cancer and the experience of dealing with them, both when alive and when dead. Where does such experience come in to the account? Going back to the discussion of ostension, it is clear that our vocabulary for describing the disease and its victims will be grounded in exposure to real and paradigmatic instances. Our classifications, such as 'well', or 'ill' or 'alive' or 'dead', 'cancerous tissue', 'healthy tissue' etc., will be based on our dealings with past instances in just the way the child's use of the word 'dog' was so grounded. These past applications of the concept will be our first and natural resort when confronted with claims about the disease having a cure. Any claim to this effect will be understood in the light of the existing conventions of usage of the descriptive terms. The credibility of the claim will have to be established, at least in the first instance, in these terms. It is easy to see, on this basis, how a claim to have found a cure can fail. Patients have been given the 'cure' but still died. Large scale tests have been done and no significant difference has been found between those given the 'cure' and those given a placebo. The natural response of anxious persons, desirous of a cure, will be bitter disappointment.

Kemp thinks that supporters of the Strong Programme cannot legitimately talk like this. He thinks they have pulled this rug from beneath their own feet. He says that the programme's supporters 'argue that, formally speaking, existing categorizations of similarity do not provide any guidance whatsoever for continuing acts of classification . . .' (ibid., p. 314). This is correct, but notice the crucial qualifying word 'formal'. Kemp overlooks it.

He proceeds as if the position was that given only by the final words in his sentence. He thinks the claim is that previous applications of a word provide no guidance *of any kind* when it comes to the next application. But that is not the claim and it should be clear that it is not from the account given of ostensive training. To think that past applications contribute absolutely nothing is to neglect the entire psychological dimension of the learning process. It is to ignore the obvious fact that, for example, past applications will begin to lay down habits. Rather than 'no guidance whatsoever' there will be the guidance generated by routine and by the creation, in the individual concept users, of response tendencies and dispositions. Here we have the domain of the taken-for-granted, something that appears to play no role in my critic's understanding of the programme. Obviously, habits and response tendencies do not explain normativity, they do not provide the definitive standard of correctness. They only determine how people will tend to respond, not whether they respond rightly, but they still influence the response. Our responses may need to be overridden or adjusted, nevertheless these pervasive tendencies throughout a group of concept users give the system its momentum and continuity.

It is only by ignoring this background of routine responses that the Strong Programme can be made to look as if it yields the fantasy idealism that Kemp believes he detects. It is only under these wholly unreal conditions that arbitrary claims to have a cure for cancer would pass muster. In reality, anyone making such a claim would be flying in the face of accepted usage, a usage geared to a body of desires and expectations, including the experience of failure and frustration in finding the sought-for cure. But doesn't the overall picture of concept application as a collective achievement allow that a group *could* begin to alter their practices of concept application so that past failure came to be called success? The idea might meet initial resistance because of existing habits of concept application, but habits can change. Whilst an outside observer, such as a visiting anthropologist, might say that cancer remained without a cure and continued to claim victims, could not insiders begin to see the world through different spectacles and declare the disease cured? They could explain away seemingly obvious counter-examples to their claim by shifting the meaning of their words and categories (or doing what the outsider would call changing their meaning—while they, themselves, might declare that there had been no change). Supporters of the Strong Programme think this is possible. But perhaps they should see it as impossible. Isn't this the real point Kemp is making?

If so it is still wrong. A group surely could make the kind of change in world view sketched above. There are no logically compelling reasons why a radical shift of this character could not take place though, from our current standpoint it may have nothing to recommend it. There are classic works in anthropology which describe life in societies where, for example, there are held to be no natural deaths, only murders by witches (Evans-Pritchard, 1937). Our imaginary new society, where there are no deaths from cancer, would be just as peculiar. The important point is that what is logically possible may not be causally possible. From a Strong Programme stance it is necessary to address the possible causes of such changes and the causes which would lead to their being resisted. Logically the failure to find a cure for cancer could lead to a radical revision of our concepts of disease and cure. The question that a sociologist would ask is: how would support for such a change be mobilised? What interests would be served? Who would gain and who would lose? If plausible answers can be found then such a pathway of change is itself plausible; if not then such changes will be blocked. Given the commitment of supporters of the Strong Programme to causal explanation there is no justification for attributing to them a picture of the workings of culture in which this dimension receives no proper role. To impute the fantasy picture to supporters of the programme is to ignore central and explicitly stated tenets of the programme.

8. Desiderata

In the concluding remarks of both his papers Kemp calls for a sociological approach to science that: (i) does justice to the scientist's involvement with the material world and (ii) does not undermine the validity of scientific reasoning. In reply I have argued that the Strong Programme actually fits these desiderata and was developed with the express intent of meeting them. As someone committed to a materialistic and scientistic approach, respectful both of the material world and the scientific mode of engagement with it, I should not want it to be otherwise. All that I would ask is that the rationality imputed to the scientific enterprise be understood in a naturalistic and relativist manner rather than being given an absolutistic gloss—that is, a gloss which, in its implicit supernaturalism, would itself be a betrayal of science as it is now practised.

I find it something of a mystery that my critic does not see that what he is asking for is the same as that which has long been on offer. This suggests to me that there is a further premise which has been kept in the background and which, all along, has been causing the trouble. Right at the end of Kemp's paper there is a clue which may shed light on this. He briefly indicates the sort of analysis of which he approves. For Kemp, the lesson to be learned is that sociological accounts could explore:

> the institutional and organisational context of scientific developments, looking at the way this provides the conditions for particular kinds of scientific debates and advances, making connections with wider developments in society at the time, and so on. (Kemp, 2005, pp. 718–719)

For Kemp the focus is to be the institutional context as a condition for scientific advance. This has a familiar ring to it. It sounds like the programme recommended by the philosophers, mentioned above, who were harassing the historians of science in the 1970s. They treated society as something that, if it entered into the content of knowledge, impaired the operation of reason and attenuated the link with the material environment. Society was therefore given the unspecific role of a mere facilitator or inhibitor. It was, doubtless, a precondition of knowledge but it had no constitutive role. This is what Kemp has argued in the two parts of his paper 'Saving the Strong Programme?'. First, we have seen that he wants to push the conventional and normative dimension out of the analysis of concept application (arguing that the pattern-matching machine can do it all by itself) and second he claims that the process of identifying the social elements in knowledge must amount to challenging the operation of reason. In the earlier paper on 'Monism' all the critical attention was on the logical properties of the network of concepts and the role of the social was persistently misidentified or left out of account.

The unintended consequences of Kemp's argument will be to rehabilitate some variant of this old, anti-sociological position. It will give aid and comfort to the rationalistic, philosophical enemies of history and sociology—the very people with whom supporters of the Strong Programme had to do battle all those years ago. In crucial respects Kemp's conclusions even represent a retreat from Kuhn back towards Popper. Although, as I have argued, there are things to be learned from the writings of the earlier critics of the sociology of knowledge, I do not think that such approaches deserve to be re-habilitated. We should not have to fight these battles all over again; it would put the field back thirty years.

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