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SOME INITIAL DIFFICULTIES WITH THE SOCIOLOGY OF KNOWLEDGE
a preliminary examination of "The Strong Programme"

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Introduction

We should start, perhaps, by making one thing clear. While what we have to say about the strong programme[1] in the sociology of knowledge may appear somewhat negative (indeed, it is somewhat negative), we are in no doubt at all about the value of the studies carried out by that programme. They stand comparison with any other investigations in the sociology of knowledge. In fact, they are comparable in almost every way. They have the same virtues and the same defects. And this is the point we wish to make. While the advocates of the strong programme offer it as a move forward, an improvement upon previous work, we can see it only as a move sideways. No progress has been made toward closing the gap between the real achievements of the studies and the claims which are made on their behalf. Part of our aim is to show that making such progress is more than can be reasonably expected just now of the strong programme, or any other movement in the sociology of knowledge. The reason for this is that, along with the rest of the sociology of knowedge, the strong programme has tended to minimise or even disregard the existence of the gap, and has offered itself as somehow enlarging, enhancing or supplementing the findings of other disciplines such as the history and philosophy of science, theology, or whatever. Naturally enough, when this happens such sentiments may be greeted with scepticism, not to say scorn. What the sociology of knowledge offers these disciplines is a description of the essentially social character of knowledge. Thus Mannheim, in one of the earliest formulations of its investigative tasks, defines the sociology of knowledge as:

> ....a research interest which leads to the raising of the question where and when social structures come to express themselves in the structure of

> assertions, and in what sense the former concretely determine the latter.
> (Mannheim. 1960 p239).

This social conception is developed in contra distinction to the view that knowledge can be analysed as if it were asocial, that is independent of the social cir cumstances in which it is found. The objective of much of the sociology of knowledge is to demonstrate just how limited this asocial conception is. It proposes that once the asocial conception is replaced by a social one, buttressed, of course, by the use of appropriate sociological method, the examination of human knowledge will be set on a proper footing. Thus the disputes arise.

There are, then, two very large interconnected questions here, namely the general issue of the extent to which sociological theories and findings can have direct and unequivocal application to work within other disciplines, and the more specific concern with how far the sociology of knowledge can contribute to epistemology. The scale of both of these questions is such that we cannot hope even to survey them within the confines of a single paper. Instead, we will take up a very specific set of arguments which bear upon both. In exploring what is involved in these arguments, we may well see how the general questions might be approached. As we have indicated, the case which we will take up is that of the strong programme in the sociology of science. There are many reasons for this. To begin with, proponents of the strong thesis have deliberately framed their findings in the most general terms possible. Their claims are strong ones. Second, their formulations have been designed to be provocative, to elicit responses both from fellow sociologists and from investigators in other disciplines. They have gone looking for arguments confident in the innovative character of their own programme. Third, it has been suggested that the studies done under the auspices of this programme indicate just what level of argumentative and evidential rigour the sociology of knowedge ought to aspire to. The strong programme is held to indicate how sociological descriptions of the relationships between social conditions, sets of institutionalised practices, and bodies of knowledge and belief are to be arrived at. And that is the problem, after all, which has bedevilled the sociology of knowledge from its inception.

In what has become a famous (or even notorious) aphorism, Shapin once asserted "One can either debate the possibility of a sociology of scientific knowledge, or one can do it." (Shapin. 1982, p 157) and thus sought to dismiss certain orders of criticism from serious consideration. We do not think these criticisms can be so easily disregarded. They are, in essence, conceptual and thus affect what we claim about the discoveries which Shapin and his colleagues are making. Because they are conceptual, getting on with the investigation of cases and the presentation of data offers no solutions. These will only be found, we would suggest, through systematic reflection on matters such as the <u>nature</u> of a body of knowledge or belief, on what it <u>means</u> to talk of the comnmensurability or incommensurability of bodies of knowledge, on what is involved in <u>sharing</u> a framework of knowledge and beliefs and, of course, on what orders of relationship concepts, practices and social life can be held to stand in.

To show how deep these isues run with regard to the strong programme, and just what might be involved in settling them, we turn our attention to just one, albeit the central claim which the strong programme makes. We will show that this claim is more than a little ambiguous. We will then extend the argument to indicate that this ambiguity is, in fact, shared by the rest of the sociology of knowledge. Third, we will introduce an expository example drawn from outside the sociology of science to show what kinds of reflections might be required to provide the clarifications we seek. Finally, we will sketch what, in our view, the likely outcome of an incorporation of these reflections into the sociology of knowlege might be.

<u>The central tenets of the strong programme</u>

According to two of its major proponents, Bloor and Hesse [2], the strong programme aims at the scientific investigation of bodies of scientific knowledge. What this is taken to mean is that any successful explanation of a scientific (or, presumably any other) body of knowledge will have to be couched in propositions which

(1) specify the causal connections between social conditions and states of belief, knowledge, understanding and so forth. These connections would identify how it was that the set of social conditions produced particular beliefs or understandings;

(2) would apply equally well to bodies of true knowledge as to false knowledge. This is termed the transivity requirement;

(3) would offer symmetrical explanations of true and false knowledge. That is, the same causal conditions will produce the same conse quences no matter whether these are the holding of true beliefs or false ones;

(4) would apply equally well to the sociology of knowledge itself. This reflexive requirement is necessary to prevent the sociology of knowledge being an exception to its own general explanations.

It is the strength of the propositions specifying the connections between bodies of knowledge and social conditions which gives the strong programme its name. These are held to be explanatory and true, as well as causal. We will see, in a moment, what might be involved in offering propositions in this area which are both explanatory and true is not altogether clear. However, first we will look at what is undoubtedly the cornerstone of the whole programme, namely that the explanations are to be causal in form.

Hesse and Bloor[3] adopt what might be thought of as a non-determinist version of causality. At ay particular moment, there will co-exist multitudes of possible and identifiable contingent causes for the acceptance of some body of knowledge or any of its component theoretical schemes. This is well worth emphasising. In principle, the strong programme deliberately refrains from offering a uniform, determinist account of causal relations but opts, instead, for a much broader conception. Within this myriad of possible contingent causes, some sets of social conditions are discerned as helping to bring about belief in, or the acceptability of, certain bodies of knowledge. By a body of knowledge is meant a theoretical schema such as that associated with the theory of evolution, Newtonian mechanics, plate tectonics, and the like. Put in these terms, the programme seems to be fairly minimalist. In effect it says, here are some of the possible causes for the adoption of this particular body of knowledge. And that is just about all. However, in selecting out and emphasising the causes which it does as part of the promotion of its corrective to epistemology, the strong programme appears to make a more powerful claim. (Indeed, as we will discuss below, some of its supporters actually argue for this claim). By focussing attention on social conditions, it seems to allocate them a primary or foundational character. Explanations are not complete until reference has been made to social conditions. If this is the argument which is being made, then it incorporates an extremely strong thesis, one which we could, perhaps, look for ways of evaluating[4].

A lack of determinacy does not vitiate the strong programme´s findings, even though it does make it somewhat difficult to see where the strength might lie. Far more important is the conception which is being articulated in those findings, namely the positing of a causal relation between sets of social conditions and bodies of knowledge and belief. The notion of causation in these matters is notoriously intractable. Presumably, when it speaks of the causes of true and false beliefs, the strong programme is referring to sets of predisposing conditions. But what is being said about these conditions? Only that they are purely contingent? That they are necessary? That they are sufficient? Or what? If it the first, then how is the foundational character of some of them to be arrived at? We can see how it might be possible to say that it was the contingent co-incidence of an intellectual curiosity with regard to the constituents of the material world, the availability of a specific technology, and the combination of a particular set of political, economic and social interests which led to the rise of the atomic theory of matter and experimentalism in chemistry, or how it might be argued that the peculiar social milieux occupied by the Edinburgh bougeoisie might be sufficient to account for the fashion for phrenology in that city in the mid nineteenth century.[5] But how are we to tell when some conditions are only contingent (ie the broad use of causation), when some are sufficient and necessary (the narrower use of causation)[6], and which are more foundational than others, when all that we have available to us are bodies of beliefs which arose in the circumstances in which they did. Because sociology is, as yet, simply unable to compare sets of social conditions with any exactitude[7], we will be unable to say when conditions were merely contingent, when they were sufficient, or when they were clearly necessary. This inability might not appear to be much of a difficulty providing one is prepared to tolerate the indiscriminability of contingent, necessary, sufficient, and necessary and suffient conditions, but we would have thought it makes a lot of difference to the putative scientific character of the findings made as well as the strength of the claims which can be made on their behalf, particularly if we want to allocate a foundational role to some rather than others. The strong programme is designed to explain the causes for the rise and fall of both central and marginal bodies of knowledge in science, but if all it can offer are accounts of contigent conditions, then it is saying no more than in the conditions in which these bodies of knowledge grew up, they grew up. Again, as a broadened history of ideas this might be fascinating. As a piece of sociology, it is rather uninformative.

But it is important to see why it is uninformative. A large part of the problem lies with the goal of the programme itself, namely the explanation of bodies of knowledge and beliefs and their associated practices.[8] For the strong programme, as we have seen, an explanation of a body of beliefs will make connections between it and the constellations of social conditions which are supposed to have engendered it. Hesse (1980) conceives the relationship as one of the interlocking of two systems. The theoretical schemas which constitute the body of knowledge are systematically related to one another. They form a network. The social conditions which make up the surrounding culture are also in systematic relationship. What is being sought is a series of systematic interconnections between these two systems. These connections will illuminate how one system causes the other; or perhaps better, how changes in one system bring about changes in the other. In characteristically trenchant form, Shapin summarises the objective thus:

> All empirical sociology of knowledge has to do more than demonstrate the underdetermination of scientific accounts and judgements; it has to show why particular evaluations were rendered; and it has to do this by displaying the historically contingent connections between knowledge and the concerns of various social groups in their intellectual and social settings.
> (Shapin 1982 p164, emphasis in original)

It is not (or, at least, not only) the untrammelled pursuit of pure reason which accounts for developments in areas of science but changes in the social environment in which such scientific practices and knowledge are located. Naturally, one of the components of this social environment is the development and dissemination of knowledge from elsewhere in science. A body of knowledge, such as a scientific paradigm, stands in a complex lead/lag relationship to the social environment in which it is to be found.

Put in this way, the explanations on offer look very similar to rather old fashioned functionalism. This should cause little surprise given the degree of indebtedness to the ideas of Durkheim and Mauss, and the reiteration, almost as a slogan, of Durkheim's maxim "the classification of things reflects the classification of men". The precise character of this debt will become apparent when we look at an example of the explanatory strategy in action in a moment. For now, we are concerned with what is required to re-specify functional explanations as causal ones[9]. As it stands, an explanatory proposition of the following form:

> There is a fit between bodies of knowledge and the social conditions in which they are found.

would have to be replaced by something like the following:

> Those bodies of knowledge which are relatively better adapted to the conditions of their time are the ones which survive.

The trouble with this is that it changes the task which has to be completed before the truthfulness of the explanation can be ascertained. At the very least, it requires the examination in detail of why competing schemes proved to be unsuccessful in order to show what the causes of their failure were. Investigators would have to draw up schemes of knowledge which were in competition with, say, Newtonian mechanics, and indicate how it is possible from the data now available to distinguish between the "real" as opposed to the "publically avowed" or "politically acceptable" reasons why some schemes were never endorsed and others censored out of existence. As Arthur Child (1944) pointed out many years ago, the sociology of knowedge suffers acute difficulties over the imputation of beliefs, knowledge and attitudes. These problems are only soluble in a very restricted range of cases, those wherein a self-conscious, ideologically organised grouping is visible, and where a coterie of ideologues is given the task of codifying the ideology. Such phenomena as political parties and interest groups might qualify. It is much more difficult to see how the same could be said of groups, schools and cliques of scientific researchers[10]. But, even in the clear cases, the imputation of ideology requires the development of sound investigative techniques for the grounding of interpretations, the formulation of readings and the attribution of attitudes. It is certainly not enough to gloss some socio-political programmatic statement and pronounce it not incompatible with a scientific or other viewpoint. Shapin himself makes the point.

> ....there is a marked lack of rigour in much social history of science; work is often thought to be completed when it can be concluded that ´science is not autonomous´, or that ´science is an integral part of culture´, or even that there are interesting parallels or homologies between scientific theory and social structure. But these are not conclusions, they are the starting points for more searching analyses of scientific knowledge as a social product.
> (Shapin 1982 p 176)

Unfortunately, to judge from the studies available to date, what the strong programme means by "rigour" or "more searching" is not altogether clear. For example, Shapin cites a study by MacKenzie[11] of the controversy between Pearson and Yule concerning estimations of the association of data in contingency tables. MacKenzie traces the differences of interpretation back to the social purposes which Yule and Peasrson had for their statistical procedures. Pearson was a firm supporter of the eugenic programme. Yule was not. Shapin summarises MacKenzie´s work by suggesting:

> Thus esoteric work in mathematical statistics within the statistical community is explained by referring different views to divergent purposes within the statistical community, and also to diverging roles in wider society. Historical work of this sort therefore illustrates........beyond any doubt that even the most technical and esoteric scientific studies <u>may</u> need to be referred to

wider social interests.
(Shapin 1982 pp 190-191, emphasis in original)

Providing what is being said here is not simply the banal observation that even statisticians have their own reasons for taking up the lines of enquiry they do, all that can be said about MacKenzie´s study is precisely that it does demonstrate an homology between social interests and scientific theories, and certainly not that such knowledge is a social product in anything other than a trivial sense. This is not to quarrel with MacKenzie´s account, for in its own way it is a model of the investigation of intellectual history, but to say that its conclusions remain plausible, intriguing rationalisations of the bases of the controvery and the vehemence with which it was carried on. The evidence which would be required to show "beyond any doubt" that statistical innovations may "need to be referred to wider social interests" for their explanation, simply is not produced. Indeed it probably could not be. We do not have the data required; nor do we have the techniques for transforming what data we do have into that which would accomplish the task.

MacKenzie himself is well aware of this. After summarising the controversy and teasing out its technicalities, he states the problem.

> The differing goals of Pearson´s and Yule´s work led to their two positions being incommensurable. Logic and mathematical demonstration alone were insufficient to decide between them, we might say. Their concepts of ´measuring association´ were different:
> (Mackenzie. 1981, p 167).

He locates the differences in their conception in the purposes to which they wished to put their statistical discoveries. Pearson was, as MacKenzie makes clear, a convinced eugenicist, and sought to put eugenic theory on a scientific basis through his work. As a piece of historical information, this is as fascinating as, <u>but no more important than</u>, the fact that many modern physicists see the discoveries of quantum physics as a proof of the existence of God, or that Game Theory in mathematics was invented by poker players. It is not a demonstration that statistical discoveries are social products in the sense implied. To do that, a clear causal connection between eugenics and sets of political, social and economic interests has to be laid out. This is what MacKenzie says about such connections.

> So I would argue that the eugenic theory of society <u>corresponded</u> in its main features to certain important aspects of the social interests and typical social experience of the professional middle class.
> (MacKenzie 1981 p 31 emphasis added)

To be sure, MacKenzie does say that he will go on to "move from this overall correspondence to detailed connections" (MacKenzie, 1981,p 31) that is, in Shapin´s terms, from homology to causality presumably, but as we have suggested, at least in the case of the Pearson, this does not happen. MacKenzie offers what he terms the following "crude summary"of the argument between Pearson and Yule:

> The biometric approach to association was the result of the needs of eugenics, and eugenics can, I have argued above, be seen as ultimately sustained by professional middle class interests. So, in crude summary, I would suggest the biometric mathematics of association <u>reflected</u> the influence of social interests on statistical theory, <u>as mediated through</u> the connections between statistics and eugenics.
> (MacKenzie 1981 p 180 emphasis added)

If we set aside his own interpretation of the force of his previous argument (after all before he said it was a correspondence now he claims the one <u>sustains</u> the other), then we are left once again with straightforward, structural homologies, the staple diet of functional analysis.

With Yule, the argument is even weaker. Having suggested that Yule was not at all involved in the eugenics movement, Mackenzie begins from the following position.

> It is difficult to specify very specific goals informing this work, and the most one can clearly point to is the absence of the crucial eugenics/statistics connection. It is just possible - I claim no more - that this absence may reflect a similar dynamic to that discussed above............traditionalist opposition to eugenics.
> (MacKenzie 1981 p 180).

and concludes

> It is possible that the Royal Statistical Society, with its strong ´establishment´ connections, was particularly attractive to men like Yule, Hooker and Edgeworth - that they may have formed a ´reactionary´ statistical sub-culture that would have seen positivist, meritocratic eugenics as vulgar. But this is merely speculation, and it must be remembered that there other grounds for opposition to eugenic policies...... Until further evidence can be uncovered, we may simply note the possibility that specific social interests sustained the non-eugenic statistics of Yule and his supporters.
> (MacKenzie 1981 p 182, emphasis added)

It does not seem to us that even MacKenzie wants to claim that this constitutes a demonstration beyond any doubt that the explanation for the differences between Yule and Pearson may need to be referred beyond its technical bounds to the wider social interests of the parties involved. Furthermore, it seems quite evident to us, that the most one could say about MacKenzie´s study is that it brings out in a most imaginative and thoroughgoing way, the structural homology of a set of statistical ideas and a set of social policies, and indicates how these policies might not be uncongenial to a specific sector of society. It is an almost ideal typical example of conventional sociology of knowledge.[12]

The influence of Durkheim

Our argument so far has tried to substantiate two interconnected propositions. First, we have suggested that one of the central elements in the strong programme, that it offers causal explanations, is ambiguous in important ways. Second, we have suggested that even if this were not so, the explanations which are offered are functional rather than causal in form. We have argued for these propositions somewhat negatively, seeking rather to question the claims made by adherents to the strong programme with regard to their findings than to examine the framework of investigation itself. It is this latter task which we now take up.

The essential element in the strong programme's investigative strategy might be summarised in Durkheim's famous proposal that the classification of things reflects the classification of men. That this is the central element has been attested to by both Bloor and Hesse[13]. Bloor, in particular has championed its adoption as a departure point. If it is true, he argues, it should make a considerable difference to how we might approach the study of science and scientific disciplines.

> In proposing that....classificatory activities reproduce the pattern of social inclusions and exclusions Durkheim and Mauss were offering us a bold, unifying principle. For if the claim is true it would be of the utmost importance for a whole range of disciplines: not only anthropology and sociology, but also the history of science and philosophical speculations on the nature of knowledge.
> (Bloor 1982 p 287)14

Exactly what difference this is likely to be, Bloor tries to show by means of an extended illustration, that of Robert Boyle and the development of corpuscular philosophy. As indicated in our previous discussion, the aim is to show how

> .....the preference that developed in certain quarters, rather suddenly, for an inert and passive, rather than active and self moving, matter....
> (Bloor 1982, p 285)

can be explained by reference to the social circumstances of those who were promoting the corpuscular philosophy. The conception of nature as animate and intelligent had its origins in Greek metaphysics, or at least, it was first codified there. This philosophical view was given a wide currency during the Civil War in England where it underpinned a pantheism or pan-animism which held that all matter displayed the Divine Principle to a greater of lesser extent. In the hands of the radical sects, this metaphysics became transformed into a revolutionary ideology which stressed, among other things, the equality of all men before God , and hence moral, religious and political autonomy. Bloor notes that Boyle was a member of ´the establishment´, a leader of ´the intelligentia´ and a man of considerable social standing. He had suffered financially during the recent war and so may be deemed likely to have been antipathetic to antinomian ideals. His philosophical and scientific views on the nature of matter are taken to be a direct expression of this antipathy and a firm ideological counter to the revolutionary parties.

> In place of this animated and intelligent universe Boyle put the mechanical philosophy, with its inanimate and irrational matter. This was then used to bolster up the social and political policies that he and his circle advocated. It was called ´latitudinarianism´. The aim was neither complete toleration of dissent nor outright repression. The latter policy would fail and hence be as disastrous as the former. A middle way was required that would contain dissent and comprehend it within the church. Enthusiasm was to be discouraged by an ethic of diligent, time consuming work; while inspiration was to give way to the slow

> accumulation of knowledge through study and experiment. In this way the initiative would be taken out of the hands of the sectaries and put back where it belonged.
> (Bloor 1982 p 287)

In case the point had been missed, Bloor draws the conclusion out even more clearly.

> For inert matter read 'people'; for active principle and force read 'Anglican Church'; for natural hierarchy of matter and spirit read 'social hierarchy'. To deny that matter can move and organise itself is to deny that (certain) men can organise themselves.
> (Bloor 1982 p 288)

Thus corpuscular philosophy is the ideology of an establishment class and corresponds with the requirements of their social, political and economic interests. Its development and promulgation is to be explained by direct reference to those interests. The same story is being told as was told about Pearson and Yule. The classification of things recapitulates the classification of men.

If we had to put our argument in a nutshell, we would want to say that Bloor has uncritically taken over Durkheim's maxim and hence while actually demonstrating functional symmetry talks as if he has identified causal connections. This then gives him the difficulty of specifying what those connections might be and at what level they might operate. In The Elementary Forms of Religious Life, Durkheim avoids this because he ellides the causal and the functional parts of his argument and , by and large, disregards the causal elements. The connection between the totemic system and aboriginal kinship networks is one of functional fit. If one wants to say that the classification of men is causally connected to the classification of things, then one has got to be prepared to say at what level these connections hold and how they work. This means that one has got to say how certain states of knowing and believing are brought about both as individual and collective phenomena by the social interests and conditions that groups of individuals might have. In other words, one has got to be able to show how, as a causal matter, individuals come to know and believe the things they do. Talking about interests and consciousness, whether in class or any other terms, of ideological hegemony, or even of such vague processes as internalisation, merely pushes the problem further back. If the strong programme really wants to do more than demonstrate the isomorphism of sets of facts about the distribution of interests, power and so on in society with sets of ideas current in science, philosophy and religion, it will have to incorporate a fairly clear cut unproblematic **causalist social psychology**.[15] Bloor offers a sketch of what this might be when discussing the elementary laws which constitute the lineaments of a classification system.

> In one respect the laws may be said to assert the co-presence of those features of the world to which we have selectively attended. They could be arrived at by the brain keeping tally of the (conventionally classified) stimuli that impinge on it.
> (Bloor 1982 p 271)

Whatever else this is, it is hardly a clearcut and unproblematic social psychology.

Of course, the strong programme need not do this. It could content itself, as Hesse has noted,[16] with being a particularly elaborated form of the history of ideas. This would be quite consistent with the intention of identifying and exploring the ranges of contingent circumstances surrounding the development of scientific or other ideas. But, of course, that would be to give up on the strong programme's claim to novelty and distinctiveness.

The problem of imputation, again

A little earlier we mentioned the much neglected work of Arthur Child. It was Child who raised the really difficult question of how to give an empirical grounding to the claims of the sociology of knowledge, namely how to ascribe correctly outlooks, doctrines, knowledge, beliefs, and, we would add, interests to a collection of people. In general these issues have been discussed as if the primary task was to determine the character of the causal connections between modes of thought and social interests. However, such discussion presupposes that the more basic problem has been solved and that we can correctly make such assignments to groups and persons. Only if we can do that can we then debate whether,first, it is possible to make connections between such knowledge and beliefs and social structure, and second whether such connections are causal in form. It is not apparent to us that we can unreservedly make such assignments and that the problems raised by Child have been wholly solved. While there has been some empirical work carried out identifying the similarities and differences between putatively distinct ways of thought , it seems to us that the identification of the social interests of some group or class remains apriori. Exactly what would be entailed in demonstrating empirically that a class or group held a set of social interests is unclear, not the least because no-one, as far as we know, has ever attempted it. The identification of a way of thought, although more often attempted, is not much less problematic, as the following example will show.

The case we have in mind is Rorty´s[17] suggestion that the Greeks could not have posed the philosophical problem of the relationship between mind and body because they were unable to make that distinction with their vocabulary. The conceptual separation was not possible for them. What is important about this suggestion is that it became quite clear right from the start just what investigations would have to be carried out to see if it was valid. Its relevance for our discussion here is quite simply that one of the guiding concerns of the sociology of science has been to demonstrate how and why novel concepts and conceptual distinctions developed and were promulgated. Naturally, this involves the exposition of just why the concepts and distinctions can be taken to be novel.

Rorty´s claim is a factual one. In sum it is that we now have the vocabulary to make the distinction between mind and body; the ancient Greeks did not. This looks to be clear cut enough. However, before we mount a comparison of Greek society and culture and our own in order to determine whether they did or did not make a distinction which we can, we would have thought it is first necessary to see whether we make the distinction in the unproblematic way that Rorty´s proposal seems to imply. This, of course, will involve examining what we have to say about minds and bodies and their possible relationships.[18]

An analogy might help here. All of us would claim to be able to distinguish the object, a car, from another object, its driver. But this does not mean that we do not, now and again, confuse what cars do with what their drivers do, and sometimes are unable to tell how much of what happened is due to the car and how much to the driver. Although we might say and show on some occasions that we can make the distinction quite unproblematically, on others a philosopher concerned with the logic of our categories might find it difficult to mark. Certainly, it is unlikely that a single property will emerge which is on display in all usage and on all occasions around which the distinction could concretise. The same goes for the distinction between mind and body.[19] Bede Rundell once made the ironic point that it was a strange use of ´mental´ which had a pain in the big toe as a mental event.[20] The point is a light hearted one, but well taken. Much of the discussion in philosophy, psychology, linguistics is not really about the grounds of the distinction at all but about classification of what are taken to be two clearly segregatable types. And yet, the extent of the debate over the relationship and its persistence despite all efforts to draw it to a conclusion, both seem to testify that, <u>as a distinction in our culture</u>, it is far from straightforward or perspicuous.

So, even if we can make the distinction in our culture, philosophers will testify that generalising that distinction is not easy. In order to see how such generalisation might be achieved, we would have to look at the very least at a range of instances where it appears that the distinction is being made, and see how they are related. We might, for example, consider the contrasts we draw between mental and physical disease and ask if there is a parallel here with mental and physical arithmetic or mental and physical fatigue. Again, we might consider explanations of a person´s actions which referred to their mental capacities, that they were sensitive, quick witted or boorish, and ask whether such explanations were symmetrical with explanations which rest on physical gracefulness, co-ordination or leaden footedness.

In any event, it is extremely likely that the net outcome of piecemeal considerations of this kind would be the rejection of any one stipulative definition of the distinction between mind and body, the mental and the physical, and so forth. It is highly probable that what will emerge from the examination of the ways that we talk about minds, bodies, thoughts, activities, anxieties, sensations and so on is the sheer multiplicity of connections to be drawn between them. We use these concepts in a multitude of different ways and no single logical reconstruction can hope to capture them all. What this seems to imply is that any examination of the character of the mind/body distinction for our culture is likely to be a slow and pains-taking business. It will involve building up a compendium of different usages, perhaps along the lines of that indicated in by Ryle.[21] Although even here it should be remembered that Ryle is far from making a positive case. He does not say definitively what the distinction must be, only that we talk about bodies and minds in a sufficiently disparate number of ways to prevent the universal applicability of a single prescription such as that he dubs "the Ghost in the Machine".

Now, the point is, of course, that if this is the situation for our culture, how much more difficult will it be in the case of the culture of ancient Greece?[22] Simply saying that they did or did not possess a vocabulary similar to ours, does not mean they could not make the distinction at all. Showing that, as a matter of fact, they did not and could not will require a wide ranging and sensitive investigation of the ways that they did talk about what we now call mental and physical events, and the parallels between their talk and our own. The determination of whether the distinction was or was not present or possible cannot be made merely by looking to the lexicon or grammar. Such a synoptic judgement can only be made by the consideration of instances. But that would require the prior solution of problems of translatability and equivalence. We cannot simply presume that grammar and lexicon correspond to one another directly. It follows that even if we were able to look long and hard at Greek usage and yet could find no trace of the distinction, and even if we were fairly confident that we make the distinction in clear cut and umproblematic ways, we would still be wise to be more than a little hesitant about moving to a judgement since our whole case would depend upon how we had lined up our own and Greek usage. That, of course, in turn depends upon decisions as to the comparability of idioms and practices which, on the surface, might seem wholly dissimilar.[23]

What does this all imply? It seems to us that the lesson to be drawn from the argument over Rorty's claim is that the decision whether or not a conceptual distinction or a classification system is or is not novel, commensurable with another, or even summarisable in a certain way, is likely to be premature <u>unless it is grounded in the sorts of investigations we have just been discussing</u>. Certainly, the citation of snippets culled from public and private documents is no substitute for them. We can tease this out a bit more if we take it up in the context of the central concern of the sociology of knowledge. This, it will be remembered, was the relationship between particular systems of beliefs, knowledge and explanations and sets of social conditions and interests. To take the classic case; how does the development of a bourgeois class, the emergence of an individualistic natural philosophy and the invention of the printing press give rise to a particular explanatory schema, let us say puritanism? The claim is usually that the social conditions obtaining prior to those in which puritanism arose were such that the characteristic attitudes of puritanism (a personalised relationship to and interpretation of scripture, individually obtained salvation, worldly asceticism) were not widely generalised or held. This claim seems, to us at any rate, to be very little different in scope to the one which Rorty made about the ancient Greeks. To say that a set of explanations or body of beliefs is not widely generalised or held is to say that, having examined a variety of contexts and practices, a range of things that people say and so, the stories they tell, the excuses they offer, the admonitions they hand one another, these concepts, these ideas, these distinctions are almost never found or found only in restricted quarters. Naturally, a claim such as this could only be made when one had surveyed instances where people might have had recourse to the ideas and concepts in question, and then only if we fail to find them. As the Rorty example indicates, it is hardly enough to look at contemporary vocabulary and on the basis of differences to be found there allocate causal efficacy to differences in social conditions.[24]

The heart of the matter

We have, at long last, come to the nub, to the heart of the matter. Up until now, we have been concerned with what sorts of claims might be made by the strong programme as an instance of the sociology of knowledge and how they might be supported. It is now time to ask whether there is any sense in supposing that the connections which the sociology of knowledge seeks could be causal in form.

For the sociology of knowledge to be constituted under a causalist rubric, it has to be possible to say that holding a belief, knowing a proposition, understanding a proof, making a discovery, is to be in a 'mental state' of some kind or engaged in a 'mental activity' of some sort or other. Furthermore, it is also to say that such a state can be caused, either directly or at some remove by a constellation of social interests and conditions. Earlier we discussed the ambiguity of what is meant by 'caused' here and how the explanations offered seemed more functional than causal. We want now to look at what is held to stand in this causal relation.

First off, the idea of 'causing a mental state' or 'causing a mental activity' strikes us as more than a little odd. Can we say that mental states or mental activities are caused? To respond affirmatively would surely only be possible once one had engaged in just the kinds of surveys of usage which we indicated would be relevant to the verification of Rorty's claim about the difference between the philosophy of the ancient Greeks and ourselves. After we have come to accept that knowing, believing, understanding are mental events or activities, and that these mental phenomena can be caused, then and only then does an investigative programme which searches for such causes become defensible. Even so, such a programme would still have the problem of demarcating and connecting up such material causes as have been isolated by psychology and neurophysiology with the proposed social causes being offered by the sociology of knowledge. What that would involve, of course, is the integration of psychological, neurophysiological and sociological theories. Such an integration would provide the causalist social psychology which we said earlier Bloor intimated was possible and which was necessary to transform the functional explanations available into causal ones. But, the elaboration of this social psychology is not a luxury for the sociology of knowledge construed in the causalist mould, an afterthought to be developed as and when the data allow. It is a requirement for the securing of its goals. Without it, all that enables us to pass from descriptions of social conditions, social circumstances, bodies of interests to individuals´ discovery of facts, holding of beliefs and understanding of explanations is an intuitive leap of faith, a leap encapsulated in the familiar assertion: "It must be that way; how else could it be?"

The requirement to have a causalist social psychology is the consequence of talking about knowing, believing, understanding and so on in certain ways. In particular, it is associated with the use of an extended metaphor. Talking of bodies of knowledge or of frameworks of concepts invokes the notion of concepts as mental entities organised in some one way which can be alligned with sets of social conditions and interests. As Donald Davidson (1974) has pointed out there is only any point in talking like this if we wish to show that the comparison of such bodies or frameworks is or is not possible. And to do that is to presume the existence of a common co-ordinating system. But, the possibility of co-ordination is precisely one of the objectives of comparison. For this reason, talking about bodies of knowledge, or frameworks of belief, will not get us very far.

What this leads back to, of course, is the sheer complexity of the ways that we do talk about knowing and knowledge, and hence an awareness of the myriad of connections and interrelations that there are between this and other ´mental activities´ such as understanding, grasping, perceiving and the like. We have done no more than allude to what would be involved in the consideration of these cases.[25]

certainly we cannot be convinced that a single reconstruction of the logical grammar of knowing or understanding will be satisfactory, one which talks of bodies of knowledge or frameworks of belief, or that we can have such talk without the prior examination of its grounds. Most of all, we cannot presume the propriety of talking about bodies of knowldge and frameworks of beliefs simply so that we can then go on to relate these bodies and frameworks to other cultural monoliths such as sets of social conditions and constellations of socio-politico-economic interests.[26]

Conclusion

Let us draw to a conclusion by reiterating what we said at the begining. We have no quarrel with the investigations carried out under the auspices of the strong programme. The best of them are every bit as good as any done elsewhere in the sociology of knowledge. It is the claims made on their behalf which we wish to question. We do not even want to object to the Durkheimian proposal that one can take a sociological interest in ´the categories of thought´ since they are social in character. In many ways that proposition is both obvious and trivial. In so far as we can say that in our society we have a conception of space, time, number, substance, or whatever, it is fairly trite to say that these conceptions are social, if all we mean by that is that they are developed in society, are held collectively, and hence are socially institutionalised. Such a formulation is not so much cautious as indisputable.

The difficulties we have begin when this cautious formulation is used as the basis for a much larger claim to the effect that somehow or other our conceptions of space, time and so on are modelled on the structure of society or are developed in the service of social interests, claims which in turn are used as the premisses of epistemological arguments of various kinds.[27] It is precisely this strategy which gives rise to the generally negative reaction accorded the sociology of knowledge since it appears to lead ineluctably to the conclusion that the categories of thought are primarily , or even exclusively, social in character. To accept that, or so it is often felt, is to accept too much, and would involve giving up a great deal more.

In sum, we are every bit as confident as the advocates of the strong programme that the taking seriously of the socially institutionalised character of cognition and conceptualisation as a sociological project is likely to have far reaching consequences. However, such consequences would, in our view, be much more insidious and far more substantial than those claimed for but not really achieved by existing work in the sociology of knowledge.[28]

FOOTNOTES

1. Apart from the references given elsewhere in this paper, the empirical, theoretical and critical literature on the strong programme is now enormous. A summary of some of it is to be found in Shapin (1982). We would particulary draw attention to the studies of Pickering (1984), Brannigan (1981), MacKenzie (1981) , Shapin (1980) , Shapin and Schaffer (1985), the observations of Woolgar (1982) and the critiques of Laudan (1981), (1982) .

2. Bloor (1976), (1981) and Hesse (1980).

3. Cf especially Hesse (1980) p 51.

4. It is this possibility, of course, which provokes the philosophers and historians of science. As we shall see, when we look at its explanations of particular cases, the claims which are made for the data turn out to be much weaker, and quite properly so.

5. Cf Bloor (1982), Shapin (1979a), (1979b)

6 Presuming, of course, that we do want to be able to make that distinction. If we don´t, then what is there to distinguish our project from what might be called "a reformed social history of ideas"? This is a thought to which we will return.

7. Blalock (1982) for example, has a particularly scathing view of attempts to make precise comparisons between sets of social conditions

8. We have discussed this at length elsewhere. Cf Anderson, Hughes and Sharrock (1984)

9. Also, given the dependence of so much of the sociology of knowledge on Marxist philosophical anthropology of one sort or another, this conflation of functional explanations with causal ones should not surprise us. In his major work, Cohen (1978) has argued that the only plausible defence of historical materialism as a scientific theory of history could well be dependent upon re-specifying the causal explanations in historical materialism in functional terms. Such an assertion has, of course, not gone uncontested.

10 Woolgar´s (1981) and (1983) papers draw out some of the debilitating consequences of seeking to do so.

11. MacKenzie´s (1981) study of statistics.

12. Just in case this might be misinterpreted, let us say once again that MacKenzie´s study bears comparison with many of what are now regarded as "classics" in the genre. One has only to look at the

grounds at issue between Merton and his critics (Merton 1984, Becker 1984) over the famous "Merton thesis" with regard to "Pietism and Science", to see this.

13. Bloor (1982), Hesse (1982). Associated with this methodological dictum is a relativist epistemology which we do not here have space to examine. It is promoted by Barnes and Bloor in their contribution to Hollis and Lukes (1982) and contested by numerous co-contributors.

14. Note just three things in passing. First, the status of Durkheim´s proposal remains that of an interesting but unsubstantiated proposition. Certainly, neither the data which Durkheim adduces nor that provided by later studies are sufficient to do so. All of which might, of course, give us pause to wonder whether, even though Durkheim treats his proposal as an empirical hypothesis, we might not be better to view it as a methodological principle. Second, the trouble with Durkheim´s formulation is that it opens up the possibility of a totally unregulated search for correspondences between natural and social categories. Thus when such correspondences are pointed to, they are of a most heterogenous kind with little indication of an accumulation of systematic correspondences across studies. Thus the homologies identified by Bloor and MacKenzie, while themselves different from each other, are in turn very different from those which Durkheim drew between the categories of thought and the distributional and rhythmical properties of social structure. Third, whatever its status, this sociological conception is being exported as directly relevant to other disciplines. All of these are aspects of the general question which we indicated at the begining we would be unable to explore here.

15. Durkheim gave a nod in the direction of such a social psychology in the Introduction to The Elementary Forms. Cf Durkheim (1976) p13 fn 1.

16. Hesse (1980) p 54.

17. Rorty (1980) and the discussion which followed Skinner´s (1981) review.

18. Obviously, we are not putting forward here what used to be known as the argument from ordinary language. Rather, we are simply sketching some of the issues involved simply because our concepts are embedded in language use.

19. John Cook (1969), for example, has shown that not all uses of the term "body" are contrastive with "mind". Sometimes, "body" contrasts with "live person".

20. Rundle (1972) p 1.

21. Ryle (1963). The gains of a similar strategy are on view in White (1980).

22. We might even hesitate to say anything about such a broad topic since no less an authority than Moses Finley has intimated our knowledge of ancient Greece is really confined to a knowledge of the remnants of five or six cities. Cf Findley (1985).

23. Think, for instance, of all the trouble caused by Evans Pritchard´s (tongue in cheek?) comparison of Zande witchcraft and science, or as de Heusch (1985) has recently pointed out, by his translatiobn of the Nuer concept of nueer as "sin".

24. None of which should be taken as indicating that the issue of why puritanism or any other set of beliefs arose when it did is not important, or worthy of study. To repeat, yet again, we are concerned with what can be said about the findings of such studies. What can and what cannot be claimed on their behalf.

25. Wittgenstein was much preoccupied with such questions, see his (1958a) and (1958b).

26. We have discussed some of the other implications of treating knowledge and belief in this monolithic way before. Cf Sharrock and Anderson (1982) and Sharrock (1974) .

27. As we have indicated, the most popular of these is that our conceptions of how things are depend upon, are patterned or held in place by some external set of forces. It is just this is argument which Wittgenstein (1979) examines his commentary on Frazer´s Golden Bough. He also gives it extended discussion. in his (1976) discussion of causality.

28. This is where the story really starts, of course. Some indication of how insidious and how substantial these consequences might be can be gleaned from several recent studies of science. Cf Garfinkel, Lynch and Livingston (1981) and Lynch, Livingston and Garfinkel (1983), Lynch (1982),(1985a), (1985b) , Livingston (1986), Woolgar and Latour (1979).

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