

LANGUAGE, THOUGHT AND THE SOCIAL  
CONSTRUCTION OF REALITY.

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## I

*Claim to be*

Sociology ~~is~~ about how and why people do the things they do. Because of this, at the heart of sociology's development has been a concern to explain what people do and why they do it in terms of what they know about the social and natural worlds in which they live. Actions and beliefs are, for the sociologist, inextricably tied together. The consequence <sup>of this</sup> is that when confronted by clear differences in types of activities between and within societies, sociologists have preferred to base their explanations of ~~in~~ such differences on differences in beliefs and claims to knowledge. The Christian prays for a sick child because of beliefs that are held about God and His willingness to intercede in the world. An Azande consults an oracle because of what he knows about magic and the malevolence of witches. Given that differences in belief produce such variations in social activities, it is both an obvious and, perhaps, a necessary next step to ask how the variations in knowledge and beliefs are to be accounted for. The general principle which has been offered is that the structure of knowledge and beliefs about the social and natural worlds is a reflection of the structure of society itself. This, we might say, is the basic tenet of the sociology of knowledge.

Recently this principle has been taken up and developed in two distinct ways. On the one hand, there has been an attempt to use it to illuminate a disciplinary area usually labelled 'the history of ideas'. Here the sociology of knowledge is put to use to fill out or contextualise the social background of intellectual development. The most popular topic has been science in the period 1600 - 1900. <sup>!</sup> As we shall see, this focus has caused

this sociologically based history of ideas to run foul of some entrenched philosophical views on the nature of science and scientific rationality.

The second direction which the sociology of knowledge has taken is towards a rapprochement with literary and linguistic studies.<sup>2</sup> In the hands of a new generation of analysts and critics, the idea of the social determination of knowledge has been taken as indicating the broad 'political' nature of literary work~~s~~ and language use. That is to say, using a language or working in a literary tradition is taken as the conscious or unconscious reproduction of an ideology. In a very strong sense, this tendency marks a return to the concerns which exercised Mannheim and his students.<sup>3</sup> In its turn, the sociological history of ideas has claimed for itself Durkheimian roots.

In this discussion, we will review both of these recent tendencies. We will do so with two general aims in mind. First we will try to ascertain whether the work that is published and discussed represents a real and novel break-through in the sociology of knowledge or simply the application of well worn and well known ideas to fresh situations. Second, and this is more serious, we wish to determine whether as it is presently conducted the sociology of knowledge can hope to formulate a coherent analytic framework which will result in a firmly based investigative programme. We will then turn to a wholly distinctive set of proposals for the founding of a sociology of knowledge, namely those of Berger and Luckman.<sup>4</sup> We will ask whether this set of proposals is a real alternative and whether it offers a preferable style of work.

Before we move to the detail of particular arguments and cases, it might be as well to sketch out what generally the sociology of knowledge has conceived itself as doing. At one level, the sociological analysis of knowledge is of a piece with the interest shown in kinship systems, economic practices, religious rituals and so on. It is the reasons for the observable variations in the patterns of these activities which ~~is~~ <sup>are</sup> ~~the objectives.~~ ~~of interest.~~ And hence, as we said, it is the explanation of the variations in patterns of knowledge and beliefs which is taken up in the sociology of knowledge. But, unfortunately, the parallels break down at other levels, and things become very much more complicated. This is because the sociology of knowledge cannot simply be defined as 'just another branch of sociology'. If we want to compare patterns of child rearing practices or different forms of the division of labour, we will base our discriminations between one set of practices and another either wholly, or in part, upon the beliefs, attitudes and knowledge professed by members of the societies concerned and associated with the practices which we wish to examine. It is the distinctiveness of knowledge, attitudes and beliefs which we will use to mark the distinctiveness of cultures and hence to discriminate sets of practices. The distinctiveness of a society and the discreteness of its 'cognitive map' are not methodologically independent. It follows that in seeking explanations of the nature of beliefs and practices, we will have to tread a wary path to avoid falling back on explaining them by pointing to the differences between societies. For to do that would be to run in an explanatory circle.

A second initial difficulty emerges from the examination of just what

a sociological conception of knowledge implies. For example, Mannheim describes the sociology of knowledge as

....a research interest which leads to the raising of the question when and where social structures come to express themselves in the structure of assertions, and in what sense the former concretely determine the latter.

( 1960. p 239)

Of course, he does distinguish what he calls an interest in the phenomenon for its own sake from the task of "correcting" traditional, as sociological epistemology, and he does say that the latter is not a necessary concomitant of the former, but nonetheless, it is clear that in Mannheim's view the most important contribution which the sociology of knowledge could make would be to demolish the philosophical conviction that knowledge of the world can be analysed as if it were independent of social circumstances. The very point of relating differing beliefs to their social backgrounds is to undercut any claims to the effect that epistemological superiority is guaranteed by ratiocination alone.

What Mannheim is doing, of course, is supposing that sociological evidence and arguments are relevant to philosophical disputes. This view is shared by many later workers in the field. We shall argue that this is a mistake. Once a clear separation is made between philosophical and sociological interests in knowledge and beliefs, we will suggest that it will become easier to clarify exactly what empirical problems the sociology of knowledge might take up and what type of concepts will be necessary to deal with them.

The two central ideas in the sociology of knowledge are these:

*The asocial  
knower.*

1. The body of knowledge constitutive of a society expresses a collection of propositions about the nature of reality. To some degree the components of this collection are <sup>non-</sup>indeterminate. The underdetermination of 'theory' by 'the facts' is a standard position in epistemology and offered as an explanation for the variations in knowledge and belief.
2. The body of propositions comprises a system of related assertions. Various organising principles can be offered to describe the system's internal logic, eg Mannheim uses 'ideology' and 'class interest', Hesse<sup>s</sup> uses an image of a network. Whatever principle is posited, the key features of the body of propositions are systemic (ie identifiable) boundaries, internal coherence and self-regulation, adaptivity.

Two things follow from these root ideas. The systems of propositions are not static; and change within them can be described in terms of whatever organising principle is posited. As a consequence, alternative accounts of the same phenomenon ( eg the emergence of relativity theory or the popularity of creationist biology) can be generated with ease. Hence, no appeal can be made to external grounds to determine which account might be preferable. Whatever change does take place within the system will take place unevenly<sup>^</sup>. This lead/lag character of the development of knowledge does not itself have to be explained. Rather it is the forms that it takes in particular circumstances that comprise the central topic for the sociology of knowledge to investigate.

What is clear from the two central ideas just set out is that the use of the notion of a body or system of propositions is a rationalising strategy. The assertions which make up the content of magic, or science, or religion are aligned with one another by means of the organising principle adopted. And hence understanding and explaining any particular body of knowledge involves <sup>the</sup> demonstration of the ease with which the internal logic proposed collects together the propositions identified. In a paper summarising the state of the art in one topic area, <sup>Sociology,</sup> Shapin suggests

All empirical sociology of knowledge has to do more than demonstrate the underdetermination of scientific accounts and judgements; it has to show why particular accounts were produced and 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.

( 1982, p 164)

*Says something about the  
of science and  
a social  
knowledge*

The upshot of this rationalising strategy, of course, has been a tendency to offer a version of the development of science, <sup>its relation to social interests</sup> religion, and so on utterly at odds with that conventionally proffered by philosophers of science. <sup>\*</sup> The sociologists look for parallels between the scientific rationality <sup>displayed</sup> and the logic to be discerned in social relations. The philosophers have, for the most part, assumed a procedural correspondence between the rationality of science and the logical structure of the natural world.

The **sharpness** of the differences over this can be brought out by considering two further ideas associated with the sociology of knowledge. The first is the suggestion that forms of thought appeal to individuals because they are symmetrical with forms of social organisation. To quote Durkheim, the classification of things reflects the classification of men. <sup>6</sup>

*\* for the debate between Leach + S.P. + Leach's recent statement synthesis.*

Second, because individuals in societies share the same sets of beliefs and knowledge and because these are reflections of social organisation, the certainty with which individuals hold on to the knowledge and beliefs they have provides a powerful mechanism by which the 'objective social facts' of social relations can be internalised as generalised norms and values. Expressed as the collective representations of social relations in bodies of knowledge, they are shared by all of the members of a society. Exactly the same kind of account is given of the nature of knowledge even when the concepts in play are 'ideology' and 'class consciousness'. In both cases, the explanation of the place that a body of knowledge has and the shape it takes is to be found in the contribution it makes to the continuation of existing social relations. Given the predisposition of the sociology of knowledge to conceive itself to be correcting traditional views in philosophy, what all this leads to is the promoting of a functional epistemology and a functional theory of history in direct competition with the rationalism espoused by epistemologists and historians of ideas. It comes as no surprise, therefore, to find philosophers and historians responding <sup>with acerbity</sup> ~~quite acerbically~~ to the explanations given by sociologists.

The sociology of knowledge takes as its topic, then, the complex set of relationships which exist between the ways that we think and speak about the natural and social worlds and the ways that they are 'in fact'. As we said at the outset, these complexities are approached in different ways, at different levels and with different emphases. For this reason alone, it will be best to proceed from now on by the examination of particular cases and particular arguments. We will start with the much debated case of the rationality of science.



## II

Broadly what is at issue in the debate over the rationality of science is the propriety of <sup>the</sup> sociology of scientific knowledge which refuses to give science a privileged status, but insists on examining it as if it were just another body of knowledge and beliefs. The rationale for this stance is the explanatory strategy outlined above. Science is just like any other corpus of beliefs and knowledge and to be explained by reference to the social circumstances in which it is to be found. This refusal to treat science differently is an expression of a clearly set out premiss; there is nothing of sociological interest to be gained by holding to the distinction between true and false knowledge. The same sort of sociological explanations will hold for both. This premiss we might dub sociological conventionalism and is hotly denied by <sup>these</sup> sociologists and philosophers who wish to argue that the difference between truth and falsity with regard to scientific knowledge is fundamental. For them, the rationality of science provides a guarantee of its truth. Since sociological conventionalism is largely an argument against this rationalist stance, we will begin by outlining the rationalist position.

There are three major components in the rationalist argument.<sup>8</sup> They can be summarised as:

1. Relativism with regard to truth is self-refuting;
2. Relativism with regard to reason (ie the denial of the existence of logical universals) is untenable;
3. In the 17th century, the development of modern post Galilean scientific

procedures had the consequence of raising scientific knowledge to a quite different plane of objectivity. The technology it has made available and the understanding it has given, mark the great divide between modern, scientifically based societies and others based on various forms of superstition or embryo sciences.

*is self-refuting*

The argument that relativism with regard to truth<sub>is</sub> is a complex one which draws upon ideas from logic, semantics and anthropology. In its simplest form it takes the guise of a consideration of the suggestion that there might be two languages side by side between which translations could not be made. If this were to be the case, the concepts expressed in one language could not be translated into the concepts of the other. This, it is said, is the central tenet of relativism. The concepts and languages would be incommensurable. Among examples of such supposed incommensurable bodies of concepts are the theories of procreation held by Tully River Blacks and ourselves; quantum physics and Newtonian mechanics; Apache metaphysics and western metaphysics; Azande magic and modern medicine. The rationalists deal with the incommensurability thesis in the following way. When we find ourselves confronted with the assertion that women get pregnant because they have sat over a fire, or because a man has told them to get into an interesting condition (!), we are at a loss to know what can be meant unless we attribute an erroneous theory to the speaker. We do this on the basis of a principle of humanity or charity?. Both of these principles suggest that when we are faced with the possibility of a failure in translation, we impute to the speaker we are attempting translate a pattern of beliefs and so forth which

is as similar as possible to our own. And, of course, we know that our own are true. This puts us in the position of making the pragmatic move when translating of assuming that the vast majority of things that others say about the world are true. In proceeding in this way, we isolate what Tully River Blacks might have to say about the habits of migrating birds and animals, the ways to cook herbs and roots and most of the rest that they can tell us, from their theories of procreation. By making this separation, the radical translation problem of incommensurability is obviated. All that is left to deal with is a partial assymetry which can be ~~dealt~~ with by attributing to the Tully River Blacks an erroneous theory. This minimisation of difference makes translation into a ~~prblem~~ problem of idiom. With regard to this maxim of 'count them right in most things' or 'count them intelligible' Steven Lukes has suggested:

It has the singular virtue of being the principle we do in practice apply in the interpretation and translation of beliefs.

( 1992. p 264 )

Clearly what will count as intelligible become critical here. Lukes, following Grandy and others, proposes that we operate by assuming 'behavioural rationality' , that is a disposition to act as if the beliefs we hold were true. What we do, then, is to presume ratiocinative procedures ~~s~~imilar to our own to underpin the symmetry between the overwhelming proportion of beliefs that we and they have. The peculiarity of the strange beliefs is then explicated by reference to local circumstances, a lack of interest in copulation, a failure to develop biological science, or whatever.

The rationalist charge is, then, that relativism cannot describe any empirical instance we might come across. We have adopted systematic procedures

designed to undercut the possibility of incommensurability. They further add, that in any case relativism is self-refuting. In order to be able to collect and cite sets of strange beliefs about procreation or illness or physical events, translations of some sort will have to be made. These translations will rest upon the principles just described. Built into the principle of humanity is, as we saw, a presumption of intelligibility. To offer examples of incommensurability, we would have to presume intelligibility. But incommensurability claims that concepts do not translate and are mutually unintelligible. Hence relativism is self-refuting.

The fact that the overwhelming proportion of beliefs will be held in common means that there are 'bridgehead beliefs' which can be used when lining up apparently incommensurable conceptual schemes or bodies of knowledge." Such bridgeheads will refer to how to plant seed, the spatio-temporal permanence of material objects and the recognition of the passing of time. "Secondary theory" embodied in science, magic and so on rests upon these primary bridgehead beliefs and is related to them in a variety of complex ways. It is precisely this variety and these complexities which generate the difficulties with translation.

Part of the principle of humanity is the presupposition of intelligibility. For the rationalist, this involves the further presumption of the universal observation of specific logical rules such as those of negation and the excluded middle. Intelligible translation, Newton Smith, for example, argues, simply would be impossible without such a presumption," or perhaps better, without the use of the invariance of logic as a working hypothesis. On his view, if we came across the following case of a speaker who said

"all misfortunes are due to witchcraft"; "Joseph is suffering a misfortune"; "Joseph is not being bewitched though", we would have to say that this speaker was not using the word 'not' <sup>and 'all'</sup> as we do, and hence the propositions as a whole would be intelligible. In such a case, Newton Smith argues, we would be forced to review our translation procedures and not to abandon the m because ~~the possibility of~~ invariance in reasoning is impossible for us to hold onto. No matter what the reasons for holding some belief will be, it will be presumed by the holder of the beliefs that they are true. It is the truth status of a set of beliefs which is the reason for holding them. The fact that someone dies from lung cancer is good reason for holding that their death was, in part at least, caused by their heavy smoking, because it is true that heavy smoking can cause lung cancer. That other societies might offer different reasons for believing the same proposition true ( malevolent magic or whatever) does not itself imply that they reason differently, only that they put different sets of conditions in the place that we might place the web of beliefs we have about the causal relation between smoking and lung cancer. There is no difference in reasoning, only different sets of beliefs. In the end, Newton Smith argues, full blown relativism ends up by being uncharitable and illiberal . It must either say that we find other beliefs unintelligible and non-rational or that other societies do not have beliefs, logic and reason.

~~Another~~ The "big ditch" argument against relativism is associated with Ernest Gellner.<sup>13</sup> With the Galilean break-through in science in the 17th century is an associated conception of knowledge and scientific procedure, one which is objective and absolutist. The idea that reality is independent of our

cognitive processes and to be fully understood only by a set of objective procedures associated with science is the cornerstone of our civilisation and has enabled our massive technological superiority over other societies. Judgements about the superiority of recent science over early science, or medical practice over witchcraft can only be made from within the bounds of this absolutist and objectivist framework. The correctness of the framework is attested to by the success of the engineering and technology it has spawned. And it is these which mark the 'big ditch' between our civilisation and others. Relativism means a denial of this superiority and can only lead to a new barbarism and a return to the scientific dark ages.

insert as Ba-h.

(insert pp 35-42 here as an extensive version of rationalism)

### III

The sociology of science as that has recently been carried out in some quarters is a direct argument with the rationalists <sup>like just described.</sup> ~~of Lukes, Newton Smith and Gellner.~~ As a first move, it seeks to deploy its sociological conventionalism in the territory which Gellner claims marks off our society from all others, namely the Galilean transformation of science that took place from 1600 to 1900. If it can be shown that causal explanations of scientific developments can be made which do not refer to the relative approximation of theories to truth nor to the objectivity of methods and reasoning, then space will be made available for the sociology of knowledge to locate its own explanatory accounts and for the disposition of relativist arguments. If the empirical claims which the rationalists make about the origins and nature of science can be shaken, then a campaign can be mounted against their philosophical justifications. We will take both of these aspects in turn.

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~~The responses to the relativism of Bloor, Barnes and Shapin with regard to the sociology of science has been of two kinds. On the one~~  
~~hand there has been~~ <sup>The</sup> a complete rejection of any hint whatsoever of the idea of relativism on the grounds that to give such a notion serious consideration is likely to open the floodgates of irrationalism and lead rapidly to the collapse of western civilisation as we know it. This vein of argument has been most forcefully put by Ernest Gellner and Ian Jarvie. <sup>25</sup>  
~~On the other hand, there is the more moderate tone adopted by philosophers of science such as Laudan who would suggest that while the claims of the relativist sociology of science remain unproven nonetheless they should not be dismissed out of hand. We think it important to notice that these are very different responses ( something that Bloor and Shapin appear at times not to have noticed) because they are expressive of different attitudes and~~  
~~marshall very different types of arguments.~~ <sup>125</sup> One could say that ~~the former~~ is an exercise in moral condemnation ~~while the latter is a more subdued methodological evaluation. Because the former is intrinsically less interesting, we will begin with that in order to get it out of the way.~~

Ian Jarvie's recent paper 'Rationality and Relativism' will provide as good a text as any, even though it is not cast as a discussion of the sociology of science. Nevertheless, from the lists of citations given in the footnotes, it is clear that Jarvie's targets are just those sociologists of knowledge to whom Bloor, Barnes and Shapin claim affinity and those philosophers whose positions they say that they adopt. The extension of Jarvie's critique to the sociology of science could be justified by a further

piece of circumstantial evidence. Nearly all of the discussions of the Bloor, Barnes, Shapin line claim at some point that it must lead to a negative evaluation of the achievements of modern science, a claim which Bloor, Barnes and Shapin hotly deny. Jarvie seems to imply that any kind of relativism is, perforce, dependent on this type of evaluation. He would, no doubt, endorse the comment that Lukes makes about Bloor:

.....Bloor nowhere explains why one should accept that certain interests - those concerned with social control, involving personal fortunes and political aims - are decisive in operating as 'coherence conditions', in determining theory choice, specifying boundaries of classifications and the selection of laws which are basic. Why should these interests always trump the distinctive interests of scientists such as Boyle and Newton, in getting their explanations right?

(1982. p 317)

What Jarvie would take issue with would be any attempt to explain science away as just the epiphenomenon of social conditions and hence to underplay the role of rationality in its achievements.

Jarvie's position is staked out through a simplification, if not an oversimplification, of Pepper's well known views on the moral reach of epistemologies and the consequences which are likely to follow from the adoption of one kind rather than another.<sup>26</sup> In his reflections on philosophical and social matters, Popper was continually concerned to find ways of relating these orders of theorising. As we say, he was particularly agitated by the possible social and political consequences of particular epistemologies. In Conjectures and Refutations he provides a very neat summary of the connection which he sought to identify.<sup>27</sup> He calls these epistemologies optimistic and pessimistic. The optimistic viewpoint holds to the universality of human rationality and to the potential in all of us to attain the truth (ie true



knowledge) by means of our innate ratiocinative power. The pessimistic epistemology emphasises the difficulties standing in the way of the attaining of such knowledge and the widespread acceptance by mankind of deception and distortion. For the pessimist, the role of sage is necessarily an exclusive one, because only a few will have ~~the~~ required time and talent coupled with the will to overcome the difficulties. If the image of the optimist is Bacon's open book of nature, the pessimist prefers to talk of our existence as one enchained in Plato's cave world of shadows. The moral reach of these epistemologies is found in the general predispositions they carry towards the social infrastructure which provides the conditions for the attainment of knowledge. The optimistic attitude stresses the moral autonomy of the individual, <sup>a</sup> ~~this~~ freedom and anarchy <sup>which</sup> is argued to on the basis of a utopian rationalism. In contrast, Popper argues, the pessimistic view leads to the moral subservience of the individual to the community, and the preservation of the rights and interests of the social whole against those of the individual. This, he says, is traditional authoritarianism. Popper, of course, took issue with and rejected both of these standard positions and wished to <sup>propagate</sup> ~~propagate~~ <sup>his</sup> ~~critical~~ epistemology which he saw epitomised in science and which he felt was the cornerstone of western rationalist civilisation. Both the pessimistic and the optimistic epistemologies would lead to the erosion of rationality and the triumph of irrationalism, and hence to the collapse of civilisation as we know it.

Jarvie's rejection of relativism is a direct descendant of Popper's attitude towards the optimistic strain in epistemology. Jarvie defines it as stressing the priority of individual opinion in that it suggests that

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knowledge is only individual opinion, and hence will lead to nihilism in the long run. This rejection, like Popper's, is not merely based on an argument that relativism is incoherent but on the claim that it is bound to lead to deleterious social consequences. The argument with relativism is moved out of the strictly epistemology<sup>ical</sup> and methodology<sup>ical</sup>, it is not just that it is bad argument and bad sociology, but because it is politically and morally distasteful. The retreat into traditional authoritarianism is not available to Jarvie because of his acceptance of Popper's critique of this. Instead, he opts for the middle course, namely a weak absolutism. The strong absolutism of traditional authoritarianism says that all truths<sup>which</sup> are known are absolute. Weak absolutism asserts on the basis of the critical epistemology that we know some absolute truths and some approximate truths. Science and its critical approach enable us gradually to move towards truths which approximate more and more to the absolute.

The idea of approximate truths - not, notice, approximations to the truth - is a startling one. The examples which Jarvie offers to support his case do not clarify matters much. If  $2 \times 2 = 4$  is an absolute truth than the proposition  $2 \times 2 = 3\frac{1}{2}$  is more of an approximate truth ~~than~~ than  $2 \times 2 = 3$ . The idea that the earth circles the sun is more of an approximate truth than the idea that the sun circles the earth. Appealing and clear as these examples might be, they are so dissimilar to one another that they do not illuminate anything, Neither do they bear on the case being made.

The proposition  $2 \times 2 = 4$  is dependent on two sets of conventions being in place, the use of base 10 for notation and the multiplication

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rules of arithmetic. If we were using the base 3, then  $2 \times 2 = 11$ . Given that these conventions are in place, then the proposition  $2 \times 2 = 4$  is not a discovery we might make about the world, a natural fact, but the working out of some of the consequences of our calculus. This being so, calling  $2 \times 2 = 4$  a truth adds nothing to it. What it certainly does not mean is that it is necessarily true. Since we can all think of occasions when  $2 \times 2 \neq 4$  (other bases, other multiplication conventions) how can we contemplate a procedure by which the approximate truthfulness of  $2 \times 2 = 3$  could be estimated? In the end, we can make sense of the example by assuming that all Jarvie wants to say is that given our conventions of base 10 and the use of arithmetic, then  $2 \times 2 = 3$  is a greater mistake than  $2 \times 2 = 3\frac{1}{2}$ . But why does he need the notion of an approximate truth to be able to say that?

The case of the earth's perihelion differs from the arithet<sup>h</sup>ical one in several crucial ways. The choice here is a straight one between a proposition which we know to be true and one which we know to be false. Here, there is a fact of the matter to which we can refer. Unless we adopt a species of Platonism with regard to numbers, this is not so of the first case. It seems, then, that we are not choosing between one approximate truth and another here either; the choice is between a truth and a falsehood. Suppose, instead, that modern planetary astronomy was based on a serious error, that both the sun and the earth were circling some other stellar object and it was only because their orbits intersected in the peculiar way ~~that they do~~

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that ~~the one~~ appears to circle the other. Which of the initial propositions would be more approximately true now? How could we tell? The whole idea of approximate truths is given currency simply because Jarvie associates truth statements with measurement statements and wishes to provide ourselves and science with the opportunity to correct our measurements. But neither we nor scientists talk of the truthfulness of measurements, merely of their accuracy, the determination of which is relative to different measurement systems we use and the purposes we have. What the second case speaks of, therefore, is not the coherence of ~~the~~ notion of an approximate truth, but of the fact that many scientific ~~and~~ other propositions could be made more <sup>precisely</sup> predictive, more general in scope and more definitive by means of an improvement in the measurement procedures on which they are based. But this cannot apply to all true statements, be they scientific or otherwise. Neither does it require a critical epistemology motivated by the desire to eradicate untrue approximations to truth.

The illustrations are clear examples of an attempt to cook an argument in advance in order to fend off some uncomfortable conclusions. <sup>Jarvie</sup> ~~he~~ chooses examples which, on the surface, appear so obvious as to be unarguable. He then talks as if these cases could stand for all possible instances. But, as Jarvie well knows, saying that is tantamount to importing a naive realism with regard to truth, and hence landing <sup>him</sup> ~~Jarvie~~ almost if not entirely with traditional authoritarianism. We might not have all of the absolute truths, but in principle we could have them. Only purely contingent facts about the history of the world and science have preventing our knowing, like <sup>the clever men at Oxford</sup> ~~Mr. Toad~~, everything there is to be known. Most philosophers would rather

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not be seen mounting a defence of naive realism. Indeed, Popper exerted a great deal of effort trying to avoid so doing. This is not the place to discuss the shortcomings of naive realism, the underdetermination of theory by fact and so on. All we can say is that Jarvie's case would be clearer if it included a consideration of the serious discussions of naive realism that have been mounted of late. Instead, he appears to be slightly embarrassed by the whole issue and, by means of misleading examples, forces us to think that the only real choices are between science and mathematics or superstition and chaos, when in fact what he is offering is a naive realism with regard to scientific truths and a platonism with regard to mathematics. As other more subtle thinkers than either ~~we~~<sup>ourselves</sup> or Jarvie have noted, in rejecting naive realism and Platonism we are not committed to rejecting the achievements of science and the superiority of rational thought.

What Jarvie has done is to trick us into thinking that the suggestion that science is socially constrained or that there might be some problems which are not amenable of scientific investigation is to say that science has no value, is irrational and does not contribute to our society. None of these follows at all. Such an exaggerated reaction looks as if it is calculated to make us withdraw. And yet we need not. We can agree with Jarvie that we find our society mostly congenial; we like to live in the environs of a large conurbation and do enjoy the advantages that modern technology bestows. But we agree with him simply because we share the same sort of society as ~~Jarvie~~<sup>he does</sup>. It is our society and it provides most of what we want out of life. But simply because we and Jarvie agree, that does not mean that everyone else in

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the world should agree with us. Naturally, we are going to evaluate other societies from our own point of view ( who else's would we use?) but in doing this we should not loose sight of the fact that they are our standards enshrined in our point of view. We are not licensed to make the further assumption that everyone should want what we want. To do that is to fall precisely into that entrenched dogmatism that Jarvie claims is characteristic of tradionalist, irrational and authoritarian societies. "either does ssaying science is a social product commit us to the rejection of scientific advance and technological innovation. All it does allow us to do is to demur from some of the overblown and pretenti~~q~~s ambitions which some epistemologists (Popper and his disciples among them) have had for science in our society. We can watch t.v., take food out of the freezer and drive down the motorway to see our relatives without samctifying either science or scientists. We can point to the social and political constraints and circumstances in which science has to operate without committing ourselves to the preferability of life in a Navaho hogan, feudal France or among the Yanomamo.

The suggestion that the classification of things reflects the classification of men has the look of a relativist epistemology. In his use of the dictum in his investigations, David Bloor<sup>13</sup> makes no bones about his endorsement of such a relativism. If it is true, he argues, it ought to make a considerable difference to our attitudes towards science and scientific developments.

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.

( 1982 p 267)

Exactly what kind of difference this is to be Bloor tries to show by means of an extended illustration, that of Robert Boyle and the development of corpuscular philosophy. His task in this illustration is to show that it is possible to offer bona fide causal explanation of

the preference that developed in certain quarters, rather suddenly, for an inert and passive, rather than an active and self moving, matter..

( ibid p 285)

which refer to social circumstances and which do not rely on approximation to truth and the other trappings of the rationalist account. These causal determinants can be discerned when one looks to see just who was promoting the corpuscular philosophy and what it was in competition with. The conception of nature ~~as expressing~~ animation and intelligence has its origins in Greek metaphysics, or at least it was codified there. This philosophical view was given a wide currency during the Civil War in England where it

and the predicate calculus are conventional. Work carried out in the foundations of logic and mathematics seems to point, or so Bloor and Barnes suggest, to the inability of logic or mathematics to justify their methods of inference from within the frameworks which they adopt. The inferential procedures are kept in place by social agreements which match our socially ~~x~~ determined intuitions. It is only because we have developed philosophical logic in the society we have, that we use it to define rationality for us.

By making their relativism primarily a methodological matter, Barnes and Bloor do lay the possibility for an effective sociology of science. In so doing, they also mark themselves off from the rationalist line and the rationalist's concerns. Nonetheless, their case is not fully convincing nor fully consistent. In fact, it is a jumble of fair points, good intentions ~~and~~ indifferent arguments *and unsupported generalizations.*

#### IV

Let us go right back to the starting place, the dictum that the classification of things represents the classification of men. What does it mean? How should it be used? At first sight it does appear ~~as if it is~~ <sup>to be</sup> akin to that more famous dictum of Durkheim's "Treat social facts as things", but this very similarity should alert us to what is going on. The injunction to treat social facts as things is not the recommendation of an ontology, ~~that is~~ the suggestion that the world consists of material things such as rocks and chairs and bicycles, and social things such as rates of inflation, beliefs about the hereafter and support for political parties, all of which are to be treated in exactly the same way. The



injunction is a methodological maxim not an ontological proposition. This might seem obvious enough but its implication is often misunderstood. By means of the maxim, Durkheim is able to formulate a series of distinctive sociological problems and hence can claim that sociology has a discrete, non-reductive character. The same holds for the suggestion concerning classification. In saying that the classification of things recapitulates the classification of men, Durkheim is not making a sociological discovery. This is not a piece of news, so to speak. Rather, he is setting out a methodological position from which investigations can be made. Since this is the case, there is no point in asking whether the maxim is true or not. It is not a proposition about states of affairs in the world, and hence cannot be either true or false. In one respect, though, Bloor is right here. Adopting Durkheim's maxim does make a difference to the sociological interest that we can take in science or other bodies of knowledge. A distinctive form of investigation becomes possible. But that is all. This sociological interest does not impinge in any way upon "the history of science" or "philosophical speculations on the nature of knowledge", except and insofar as they are the subject of sociological investigation themselves. We say this not in order to make an argument for hard and fast academic boundaries, nor simply to re-affirm the distinctiveness of sociological and other competencies, but to draw attention to the very peculiar nature of the interest which sociology takes in its phenomena, an interest which can easily be discerned in Durkheim's work. This interest has its root in the determination to treat all of its topics as displaying a social organisation, a determination which is sociology's

central methodological tenet and not its most famous discovery. As a consequence, it makes little sense to try to use this tenet as a way of correcting philosophy or the history of idea. What ought to be at issue is whether a sound and fruitful sociological programme of work can be based on Durkheim's version of this methodological principle not whether he or Marx or Mannheim has got the nature of knowledge right. It is for this reason that Shapin's <sup>now famous</sup> challenging dismissal of critics

One can either debate the possibility of a sociology  
of scientific knowledge or one can do it

( 1982. p 157)

is fatuous. What is in question is what the sociologists <sup>of science</sup> can claim to be doing when they do it.

To what use does Bloor wish to put this maxim of Durkheim's? At the outset, he makes it quite clear that he wishes to use it to explain "the incredible diversity of interpretations which men have used to understand the world" (1982. p 320). What is odd here is that Bloor should find this fact of diversity puzzling at all, and should want to explain it. We should have thought that it is hardly surprising at all that, left to their own devices, different societies develop their own explanations of things. As John Cook once pointed out<sup>5</sup>, the idea of cultural relativism is ethnocentric because it rests upon the supposition that there ought to be just one explanatory scheme in use, but since there is not, all of the competing version have to be tolerated. It is only by contrast with the single schema view, that the fact of diversity becomes a problem to be explained. We do not find it puzzling that the children of different parents differ from one another in personality, looks and so on. They

left back to  
Mannheim

are after<sup>f</sup>all, we might say, the children of different parents. But, if on going to the hardware store to have a key cut, we find that the duplicate not only looks different from the original but will not turn the required lock, then we have something which we want to explain. We can find the explanation either in the skill of the cutter, or the setting of the machine, or something similar. Things have not turned out as we expected and so there is a puzzle to be explained. This point is worth emphasising because one of Bloor and Barnes' key arguments in defence of relativism is the denial of the universality of ratiocinative procedures defined in formal logic. And yet, the position they adopt only derives its investigative topic, the thing to be explained, by supposing a universality of logic generating just one explanatory scheme, departures from which are the objects for explanation. The relativist line begins in the possibility of a unified scheme and then denies such a possibility. We would like to ask why such a possibility should be given serious consideration in the first place.

The answer, of course, is the fascination with the diversity, the ease with which bizarre collections of items can be built up, and the compulsion to explain everything. Following Durkheim, Bloor's explanation will be a modified relativism:

.....of course the world enters into the structure and development of our knowledge, but always and necessarily in conjunction with our conventions, decisions and purposes. If we then ask how these two factors are conjoined.....Hesse's network model provides a simple but thorough account of all the central points.

*The combination of*

6 The way the world is and our conventions, purposes and decisions are the causal factors which produce our bodies of knowledge. Differences in conventions et al must be enough to explain differences in schemes since, presumably, the way the world is will not vary from society to society. What we are trying to point to here is the 'causalist mentality' which is on view. " Causal (ie scientific) explanations can and ought to be given for why everything is the way it is. There is no hint of an appreciation that somethings, such as the diversity of interpretations do not need to be explained scientifically, <sup>simply</sup> because we have explanation enough to hand in our ordinary awareness that diversity is what we would ~~ordinarily~~ <sup>naturally</sup> expect. To try to offer further, 'deeper' explanations of a causalist or reason-giving kind is to misjudge the phenomenon under inspection. We will now try to get the magnitude of this misjudgement into focus.

*are not  
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The object of explanation for the sociologist of knowledge is the form which knowledge takes. Bloor offers this explanation by making a connection between knowledge and its social environment; the latter is one of the causal factors determining the nature of the former. Two things have to be achieved in order to convince anyone of the soundness of this explanatory route. First, there has to be a demonstration of the plausibility of talking of a body of knowledge in terms of its surrounding social circumstances. Second, having demonstrated the plausibility of the connection, it would be necessary to go on and fix just how strong and determining the connection might be. Rather than <sup>leaving</sup> these questions open as matters for empirical investigation, Bloor and others following the

'Durkheimian' tendency in the sociology of knowledge, proceed in exactly the opposite way. They stipulate how a body of knowledge must be related to the social circumstances in which it is found; it will function as a collective representation(or an ideology) expressing the needs and interests of particular groups. As we saw in the case of Robert Boyle and corpuscular philosophy set out earlier, this stipulation is the explanatory mechanism and is underwritten by a clear-cut image of the nature of bodies of knowledge such as science. They are collections of practices which embody one or many conceptual schemes. The task in the examination of cases such as that of Robert Boyle is to show how the historical facts as far as they are known are amenable to interpretation by means of this stipulation.

What then, is the strength of the connection which Bloor offers between Boyles' philosophy and his social circumstances? What kind of a connection might there be? We will take up the latter question first because it will quickly become apparent that the evidence which is available to Bloor is insufficient to enable him to make the case out that he would wish.

The model of knowledge whci Bloor adopts is, as was noted earlier, the 'network model' associated with Mary Hesse. Under this rubric, knowledge is conceived as a weave of propositions, laws and classifications which coheres because of the unity of social ends to which it is put. The knowledge expressed in this network provides an organisation to the things which make up the world. As Shapin has put it

Knowledge is not regarded in this literature as contemptively produced by isolated invididuals; it is

produced and judged to further particular, collectively sustained goals. Knowledge, in this perspective, is always tailored to doing things. It is in the course of doing things with knowledge that its meaning is produced: thus the notions of use and meaning are intertwined.

( 1982. p 197)

As we noticed above, the coherence conditions of the network are not wholly one-sided. The scheme is in part determined by the nature of reality itself. In Bloor's sociological use of the network model, then, we encounter a kind of conventionalist empiricism. The content of the scheme or the network is to some extent socially determined and to some extent empirically determined, that is determined by the social and natural 'facts of the matter'. In utilising the scheme-content dichotomy, Bloor endorses what Donald Davidson has labelled the 'third dogma' of empiricism.<sup>17</sup> This states that one can separate from the conceptual scheme that which the scheme organises. In typically witty manner, Davidson explains why this is a dogma.

.....even those thinkers who are certain that there is only one conceptual scheme are in the sway of the scheme concept; even monotheists have religion. And when someone sets out to describe "our conceptual scheme", his homey task assumes, if we take him literally, that there might be rival systems.

(1973-4. p 5)

Davidson goes on to point out, it does not make sense to talk as if on the one hand there was the scheme and on the other there was reality, <sup>in general,</sup> that which had to be organised. And this much being so, it makes even less sense to talk of such schemes as if it were possible to estimate just how well they organised reality, just how far they corresponded with the way things are. And <sup>following</sup> ~~if~~ ~~this is so,~~ then, is there any point at all to talking of the <sup>in</sup> ~~constraining~~ influence which the world brings to bear upon our schemes <sup>or</sup> ~~and hence~~

of a limited correspondence between any scheme and the facts, so that the level of this correspondence will enable us to choose between rival systems or schemes? Let us take these arguments one by one.

What does it mean to say that a body of ideas or a conceptual scheme is a picture or image of reality, that is, the ways that things in general are? What this clearly does not mean is the obvious fact that some of the things that, say, Roman Catholics say can be construed as making reality claims. To say that a (small) number of people have been transmuted from the earthly to the spiritual realms is to make some, not altogether clear claims about the nature of real things. To say that some naturally occurring events such as conception can have non-natural causes is of similar ilk. And so is the suggestion that the wine and bread of the communion are really and not metaphorically transformed into the body and blood of Christ. However, taken either individually or collectively, none of these suggestions adds up, to a claim about what reality-in-general is like. They make some specific claims about some particular real things. For a conceptual scheme or body of ideas to have a ~~view~~ view about reality-in-general, it would be necessary for there to be an order of reality which transcended particularities; an ~~order~~ order of reality about which it would be possible to make propositions expressing reality claims. The point, of course, is that we cannot segregate a general order of reality from particular things we might want to say about particular <sup>real</sup> objects. We cannot tell what an instance of the general would be that was not an instance of the particular. This is as true ~~for~~ religion as it is for science. There are numerous propositions about matter, light, motion, the chemistry of cells and the propagation of species. But we can only make these propositions add

up to a general thesis about reality-in-general by adding to them a further non-scientific proposition about the nature of reality. Such a proposition would be a metaphysical and not scientific one. This is not to say that scientists have not gone in for metaphysical speculations, nor that they have not drawn a great deal of comfort from the parallels which they have seen between their scientific theories and <sup>a few</sup> preferred metaphysical ones. But, and here is the important point, the scientist has no more special competence as a metaphysician than anyone else. We do not have to treat scientists' metaphysical talk in any way different to any other metaphysics. Certainly, we would not expect to be able to measure the fit between their beliefs and reality ~~simply~~ because such metaphysical theories tell us just what will and what will not count as reality. How could one hope to evaluate the correspondence of a causalist, inanimate, materialist conception of the world with the world except in causalist, inanimate and materialist terms? And hence we cannot hope to use the metaphysics we might find scientists adopting as a way of decrying, or deflating the claims that are made on behalf of science. As <sup>18</sup> Rush Rhees once asked, if some reason scientists were to cease speaking in causal and mensurational terms and to speak in functional and classificational ones, would that make them any less scientific? The sociology of science which Bloor proposes, treats the development of science as a history of several conceptual schemes, each of which has some limited correspondence to the ways that things are. Our suggestion is that he can only do so by engaging in a sociologically inspired metaphysics which only gains credence because it admits, at the very beginning, the possibility of talking of an order of things glossed as 'reality-in-general'. But there is no sense in talking of reality in general when all that one means is some



observation about real things.

There is, then, no specific content to be organised by the scheme. The scheme is not a scheme of reality. What about the other side of the dogma, the scheme itself. This, it will be remembered, consists of a set of propositions, laws and classifications, which if treated in general enough ways will display a unity. The general way we are invited to view them is to treat all scientific talk as articulating a network of propositions <sup>sentences espousing</sup> or propositional attitudes. The difficulty here is in sustaining the view that every utterance that one might make about a scientific or religious topic was an expression of a proposition of some kind. As John Austin made us all aware,<sup>19</sup> even utterances containing clear-cut statements of propositional attitudes can do a great deal more besides and other than stating propositions. It is not all that surprising that Hesse's network should be based upon this logicist view of utterances and propositions, because her concern was with the nature of scientific inference, and hence it was reasonable for her to restrict her topic to utterances cast in predicate terms. But why should sociology take over this limitation without question? In his use of it, Bloor gives no justification beyond that cited earlier. Neither does he say why this logical view should underpin a sociological one.

According to the network model, the scheme of knowledge consists of classificatory propositions held together by laws. These laws can be of many different types from 'fire is hot', 'wood floats' to 'diamonds are hard' and 'copper sulphate is water soluble'.

In one respect the laws ~~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 upon it.

(Bloor 1982. p 271)

Alongside this strange inductive psychology, is the clear suggestion that these laws are both commonsense and scientific ones. By talking of conventional classification and selective attention, Bloor is suggesting that the degree of constraint placed upon attention by the facts of our neurophysiology, our psychology or the material world is vague. What the anthropological evidence would seem to indicate is that there are very many different ways of satisfying the constraints, not that there is complete constraint (naive realism) nor that there are some constraints (relativism). Social determination is not the only way that the constraints can be satisfied; it is not the only story that could be told.

Embedded in Bloor's sociology of science is a discovery procedure which could be formulated as the maxim "Look for the underlying propositions". It is this that we wish to discuss now. Clearly we do not do so in order to illustrate an alternative conception of the nature of bodies of knowledge, one that was not based on propositions. We do not want to offer an alternative theory of knowledge of any kind. Our aim is much more limited, namely the indication of the difficulties of using a logician's conception of knowledge as the basis for the description of the social organisation of knowledge, or of the day to day activities in which knowledge can be seen and is used.

Consider the following three stories:

1. Jane is learning to play the banjo. To help in tuning her instrument, she sings 'My dog has fleas'.
2. Daniel joins a faction of the anarcho-syndicalist movement. At a lecture on the history of the movement he hears about Proudhon's famous slogan "Property is theft". Much taken by this oxymoron, he pins it to his bedroom wall and repeats it every morning on awakening.

3. George is an arms dealer. Times are hard. He is approached by a small but fanatical group. When taxed with his connection with this group, George replies 'Business is business'. (This example is John Cook's.<sup>21</sup> The story is different though.)

In all three ~~of these~~ stories we have examples of propositions which might qualify as classificatory or law like propositions under the Hesse network. But is any one of the cases really an instance of the articulation of a proposition? Surely it would be perverse to argue that 'Jane is animadverting to the condition of her dog? She could perfectly well sing the ditty and not be uttering a falsehood if she had never owned a dog. The sentence is an aid to a practical task. In the case of Daniel we seem to be on surer ground. But are we? Is Daniel on repeating the slogan every morning really making the observation that property is theft? Or is it more of a ritualistic re-affirmation of his commitment to certain sorts of values? To say that "business is business" is a proposition is immediately to render it a tautology and hence to define it as saying nothing about the world. But, of course, it tells us a great deal about George simply because its use is not that of a proposition. It is, rather, a cliché about the justification for separating commercial and moral life.

The point of these stories is to indicate just how much would be gained here from the consideration of cases. What can certainly be seen is the critical role that <sup>detail of the</sup> circumstances of use have for the nature of the activity in question. This implies two things. First, we ought to be very careful about adopting a viewpoint which allocates foundational status to just one form of language use. To do this is to delimit the kind of sociological interest one can take in

examples such as these. The second implication concerns the nature and role of evidence in the discussions to hand. Before we could talk about the conceptual scheme that Robert Boyle and his friends sought to impose upon science, we would have to look very carefully for evidence of occasions when the required propositions were being made. As Arthur Child pointed out many years ago,<sup>22</sup> the sociology of knowledge suffers acute difficulties over the question of imputation of beliefs, knowledge and attitudes; problems which can only be resolved with regard to a very restricted range of cases, those wherein a self-conscious, ideologically organised <sup>group</sup> is to be discerned and where a coterie of ideologues is given the role of codifying the ideology. Such phenomena as political parties and interest groups might qualify. An institution as broad and vaguely defined as science would not. But, even in these 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 would certainly not be enough to gloss a general socio-political attitude and to claim that this was not incompatible with some particular scientific or other viewpoint. Shapin himself makes this point very forcibly.

.....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 thought and social structure. But these are not conclusions, they are the starting points for more searching analyses of scientific knowledge as a social product.

( 1982. p 176 )

Unfortunately, if we are to judge from his later discussion, Shapin has little idea what rigour or being 'more searching' could amount to here. He cites a study by MacKenzie<sup>23</sup> ( ) of the controversy between Pearson and Yule concerning

the correlation of data in contingency tables. MacKenzie traces the differences of opinion back to the social purposes which Yule and Pearson 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.....  
~~Firstly~~.....beyond any doubt that even the most technical and esoteric scientific activities may need to be referred to wider social interests.

( 1982. p 190 - 91 )

Providing what is being said here is not the banal observation that even statisticians have their own reasons for following the lines of investigations which they do, then all that can be said about the MacKenzie study is that it demonstrates a homology between social interests and scientific theories. This is not a quarrel with MacKenzie's account of the reasons why Pearson and Yule had the interest in developing correlation in the ways that they did. But MacKenzie's account <sup>is</sup> ~~remains~~ a plausible, hypothetical attribution, and that is all. The evidence that would be needed to show "beyond any doubt" that statistical innovations may "need to be referred to wider social interests" for their explanation simply could not be available. We do not have the data in the detail required; nor do we have the techniques for turning what data we do have into that which would be required.

Our central theme has been that Bloor's account of science is predicated on the scheme-content distinction and it is this which is its fundamental weakness. The real problems emerge simply because talking in scheme-content terms in a particular kind of logical reconstruction of knowledge which may

not be all that useful for sociological work. The reason for this is the underplaying that it incorporates of language activities other than the enunciation of propositions. Propositions are treated as foundational. This element is to be discerned in the studies of Durkheim and Mauss and is only reinforced by Bloor's adoption of Hesse's network model. If one questions the advisability of presupposing the ubiquity of propositional attitudes, the cornerstone of the whole enterprise is taken away. It is not surprising that Durkheim and Mauss should have adopted the views that they did because they were working at the time of the high tide of positivism. However, since then developments in logic, epistemology and metaphysics (to which Mary Hesse has been a contributor) have made such general views untenable.<sup>24</sup> Where these developments impinge upon sociology they tend to surface in an encouragement to sociologists to pursue their own interests without feeling the need to address, or adopt, philosophical ones. As 19th century philosophers *manqués*, neither Durkheim and Mauss (nor Mannheim) could see this.

This imperviousness to sensitive critical evaluation of the philosophical positions he has adopted, is also to be seen in ~~his adoption of sociological positions.~~ <sup>Bloor's Sociology.</sup> His adoption of Durkheim's methodology and his defence of ~~this adoption~~ <sup>the work of</sup> by reference to neo-Durkheimians such as Mary Douglas makes it appear that since 1917 no serious, penetrating evaluations and reservations have been forthcoming for Durkheim's methodology. Notice we say, Durkheim's methodology, we do not say the truth of his findings. There can be no question of refuting Durkheim (or validating him either) because as we saw at the outset, what is at issue here is not a sociological discovery or finding but the provision of an organisation, the drawing of a picture of (some) social activities. We would not now allow our graduate students

to carry out a sociological investigation of totemism which referred only to missionary reports, traders letters and the like. We would expect some 'real fieldwork'. We would not allow them to make generalisations about suicide based on collections of government statistics, without incorporating a whole host of virtually debilitating provisos, acknowledgement of weaknesses etc. If we were to do so, our students might quite rightly accuse us of teaching them antediluvian sociology. Standards have moved on. <sup>in exactly the same way,</sup> What <sup>we</sup> now count as an articulated theory, as a clear and worked out demonstration of a case are ~~very common~~ much more detailed and sophisticated. If ~~Bloor~~ wishes to engage in a Durkheimian sociology of knowledge there are to-day many exemplars for him to look to to determine the theoretical sophistication required - Levi Strauss and Parsons, for example, or <sup>for</sup> the depth of detail necessary - Ladurie and Braudel. ~~He shows no sign of following their lead.~~

It seems to us, therefore, that the sociology of knowledge which Bloor represents is not an innovation but a regression. It marks a return to philosophies once abandoned and <sup>to a</sup> hackneyed sociology. At the same time it deliberately pushes sociology back into debates which it ought to stay well clear of. For someone ostensibly committed to the underdetermination of theory by facts, Bloor ought to have realised that sociological facts will never provide the decisive edge for any philosophical theory. Any set of facts can be incorporated into any philosophical theory with a bit of licence and ingenuity.

I

Between philosophy, sociology and the natural sciences lie a number of free fire zones. The unwary who wander into them, are apt to have an uncomfortable time. But, despite the risks to be run, we would like to explore one of these zones, that much contended over territory known as the sociology of knowledge. In particular, our attention is focused on a small portion of that territory, namely 'the strong thesis in the sociology of science'. What follows is not intended as a thorough survey of 'the strong thesis' and the investigative programme derived from it since we limit ourselves to some initial difficulties only. But, as we will suggest, many of the problems which we discuss are also to be found in the sociology of knowledge more generally, for they have to do with the basic methodological issue of how descriptions of bodies of knowledge and belief, mental activities such as knowing, believing and understanding, and the relationships between social actions of various sorts and mental activities, are to be arrived at. The importance of this issue for us is not the product of our adoption of some arriviste philosophical sociology. It can be seen, we suggest, at the heart of all discussion and analysis in the sociology of knowledge.<sup>1</sup> The reservations we express are derived from positions in philosophy and sociology which are well known, but somewhat ignored. And yet, in our opinion these positions pose severe methodological questions for both 'the strong programme' and the sociology of knowledge, to which they would have to find answers before they could implement their own investigative policies. To provide such answers requires reflection on conceptual matters such as the nature of a body of knowledge and belief, on what



it means to know something or to be converted from one belief to another, on the reasons for holding on to some beliefs and abandoning others, on what is meant by sharing beliefs, and, of course, most importantly, what kinds of relationships can be held to obtain between concepts, actions and social life.

In order to demonstrate that this task is necessary, we will show, first, that the most crucial of the central tenets of 'the strong programme' is deeply ambiguous. We will then extend our argument to suggest that this ambiguity is shared by the rest of the sociology of knowledge. Third, we will indicate just what kinds of methodological reflections would be necessary to provide the clarifications we seek. And, finally, we will consider what the likely outcome of such reflections, and the incorporation of the sensitivities they entail into the sociology of knowledge might be.

## II

According to two of its major proponents, Bloor and Hesse,<sup>2</sup> 'the strong programme' aims at the scientific investigation of bodies of scientific knowledge. What this appears to mean is that any successful explanation of any 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 transitivity requirement;

- (3) would offer symmetrical explanations of true and false knowledge. That is, the same causal conditions will produce the same consequences no matter whether these be 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.

The strength of the connections between bodies of knowledge and social conditions is what gives 'the strong programme' its name. These connections are to be both explanatory and true as well as causal. We will see, in a moment, what this implies for explanations in this area and, further, what is required for such an explanation to be true. However, first we must look at what is undoubtedly the corner-stone of the programme, namely, that true explanations are to be causal in form.

Hesse and Bloor adopt what might be thought of as an indeterminate version of causality.<sup>3</sup> At any particular time, there will co-exist multitudes of possible and identifiable causes for the acceptance of some body of knowledge or any of its component theoretical schemas. This is well worth emphasising. 'The strong programme' deliberately does not offer a uniform determinist account of causation. It is merely saying that from within a myriad of possible causes, some social conditions can be discerned as helping to bring about belief in some particular bodies of knowledge. By such a body of knowledge is meant a theoretical scheme such as that associated with the theory of evolution as an explanation of the species differentiation to be observed in the natural world. Put in this way, the Bloor/Hesse

programme does not look all that strong but, in fact, 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 all. Presumably, although this is not made clear in the various discussions, at some point the programme would want to be able to group the causes which it was identifying into primary and secondary ones at least, otherwise it will never be possible to decide exactly what order of claim is being made for the programme's products.

This indeterminacy does not vitiate 'the strong programme' although it does make it difficult to see just how strong the programme's findings are likely to be. Closely related to this problem is a much more fundamental difficulty, namely the causal conception that the programme holds to concerning knowledge and belief.

When 'the strong programme' speaks of the causes of true and false beliefs, presumably it is referring to sets of predispositions or predisposing conditions. But what is being said about these conditions? That they are necessary? That they are sufficient? That they are necessary and sufficient? We could see, for example, how it might be argued that the social experiences of the Edinburgh bourgeoisie were sufficient to explain the efflorescence of phrenology in that city in the 1830's, or that a mechanical philosophy was necessary for the development of Newtonian physics.<sup>4</sup> But how are we to tell if some set of conditions is only sufficient and not necessary when we will always be dealing with bodies of beliefs which arose in the circumstances that they did? Because sociology is notoriously unable to compare social conditions with any exactitude, we will be unable to say when conditions were merely sufficient and when they were clearly necessary. This might not appear to be much of a problem providing one is prepared not

to discriminate sufficient, necessary, and sufficient and necessary conditions, but we should have thought that it would make considerable difference to the putative scientific status of the findings and the claims which can be made on their behalf.<sup>5</sup> 'The strong programme' is designed to determine some of the causes of the rise and fall of phrenology, or of the delay in the acceptance of non-Euclidian geometries, or of the impact of eugenics on statistics, or even, perhaps, of the resuscitation of creationist theories in biology. Yet all that it can come up with are statements to the effect that in the conditions in which they grew up, these explanatory schemes grew up. As explanations, such accounts are not only circular but uninteresting as well.

But it is important to see why they are so uninteresting. We feel that a large part of the problem lies with the goal of the programme itself, namely the explanation of bodies of knowledge and belief. For 'the strong programme', an explanation of a body of knowledge or belief will, as we have seen, make connections between it and the clusters of social conditions that are supposed to have engendered it. Hesse conceives of the relationship to be one of the interlocking of two systems.<sup>6</sup> The theoretical schemas that constitute a body of knowledge are systematically related to each other; the social conditions that make up the surrounding culture are also in a systematic relationship. What 'the strong programme' is searching for, then, is a cluster of systematic connections between these two systems. These connections will explain how the one system causes the other or, perhaps better, how change in one system causes change in the other. But what are these explanations to be like? The proponents of 'the strong programme' wish to stress that it is not the internal motivation of untrammelled rationality which accounts for developments in science but changes in

social conditions.<sup>7</sup> A body of knowledge, such as science, stands in a complex lead/lag relationship to the rest of the society in which it is to be found. Put in this way, such explanations are no more than very old fashioned functional ones. This should be of very little surprise since 'the strong programme' is very explicit about its debt to Durkheim and Mauss.<sup>8</sup>

However, for a functional explanation of the relationship between bodies of knowledge and social conditions to be turned into a causal one, it would have to be reshaped. Instead of a proposition such as

"Bodies of knowledge are created in order to fit the social conditions which generate them."

we have to have something like the following:

"Only those bodies of knowledge which fit the social conditions of their time can survive and these are the ones which are adopted and so persist."

The trouble is that a reformulation of this kind changes the task to be completed before the truthfulness of the explanation can be ascertained. At the very least, what it requires is the examination in detail of why competing schemes and bodies of knowledge were unsuccessful in order to show what the causes of their failure were. Investigators would have to draw up lists of schemes which were in competition with, say, Newtonian physics, and then indicate how it is possible, from the data which are now available, to distinguish between the "real" as opposed to the "publicly avowed" or "politically acceptable" reasons why some schemes were never endorsed and others censored out of existence. It would be important to be able to do this for otherwise the investigator could never be certain that in offering the reasons he does he is not merely reproducing official propaganda. Such an investigator is, then, placed in exactly the same position as any researcher who tried to test C. Wright Mills' allegations about the existence of an effectively organised power

elite. If the allegation were true, the investigator would never be allowed to see the elite in action. If a small group of people did direct political decision making in their own interest and knew that this was so, they would have to be politically inept to allow the sociologist open access to their inner coun<sup>ci</sup>ls. And if they were so inept they would not pose the threat to political freedom that Mills supposes they do. One of the conditions they would stipulate before they would allow the sociologist in would, presumably, be that he was to be given 'the official line' and hence made an investigative dupe. The investigator could only counter this possibility by using the thesis he is testing as a practical fieldwork theory. He would have to doubt the word and sincerity of all in authority. He would have to treat as significant all oversights, omissions, differences over facts and opinions, and see in such discrepancies indications of the gap between what the power elite said they were doing and what they were actually up to. But, how could anyone who utilises a hypothesis as a practical fieldwork theory in this way, claim to have substantiated that theory?

In addition to this ambiguity over whether functional or causal explanations are actually the goal of the programme, and the difficulties with investigating causal connections in this context, there are severe problems with the mechanisms of causation themselves. It is not very clear from the programme whether the causal mechanisms operate at the level of the individual and his or her beliefs, at the social level and societal frameworks of belief, or both. Given the powerful influence of Durkheim, it is not unreasonable to suggest that the causal mechanisms should be thought of operating at both the indivudal and the societal, or collective, levels. In any event, what ever levels they operate at, there are profound difficulties with demonstrating that certain states

of knowledge are brought about by the social conditions in which collections of individuals find themselves. How do individuals come to know and believe the things that they do as a causal matter? Talking about consciousness whether in class or other terms, or ideological hegemony, or invoking processes as vague as internalisation will not provide an answer, and neither will talk about bodies of knowledge and the <sup>ir</sup>causes rather than individual states of knowledge and their causes. If 'the strong programme' really does want to attack this issue in the way that it says that it does, then what is needed is not an extended list of facts about social conditions but a few clearer ideas about the social psychology of knowing and believing. Without these and some firm connections into the psychology of belief and knowing, we will be left with the presumption that somehow the knowledge current in a society gets inside the heads of societal members in a wholly unproblematic and relatively uniform way.<sup>9</sup>

In the end, as Mary Hesse recognises,<sup>10</sup> 'the strong programme' degenerates into a version of the history of ideas. It proposes associations and resonances between different collections of ideas, and, arguing on a post hoc propter hoc basis, alleges, in an unspecified way, that some ideas caused others to arise and that these others survived because of the fit between them and the social conditions in which they are to be found. Hence, insanity is treated as a disease because of the medico-physicalist paradigm, and Darwinism was popularised because of the supposed naturalness of social orders. While the story that is told in each case may be of great fascination, surely the generalisation that social conditions produce bodies of knowledge is not very enlightening?

We said that 'the strong programme' wants to explain why scientists know what they know and interprets this 'why' in causal terms. It strikes

us that there is something a bit odd about this. How could one set about explaining why scientists, or anyone for that matter, know the things they do? For a start, what kinds of things do scientists know? They know that the stars are millions of miles from the earth and would take many years to travel to. They know that autumnal changes in the colour of leaves are the result of chemical reactions. Why do they know such things? They have studied them, read about them, been told about them. For the most part, scientists know what they do because they have told things by others, and they see no good reason to doubt their word. Such an explanation of why scientists know what they do bears no relationship to the theoretically constructed sets of causally related propositions that 'the strong programme' wishes to have as its explanations. But what would explaining what scientists know and how they know it in any other way really be claiming? 'The strong programme' says that this trust of others is the expression of a shared conceptual apparatus, and not of rationality. But isn't the willingness not to doubt some things just what we mean by rationality in circumstances such as these? If everyone we know believes that the earth is round, that the universe is infinite, that death is inevitable, and that men have certain minimal obligations towards one another, would it not be irrational of us to doubt these things and to ask for proofs? What 'the strong programme' wants as an explanation is a system of propositions which form a theoretical scheme. But how could that explain why individual scientists know what they do?

### III

There are, then, some fundamental difficulties yet to be overcome by 'the strong programme'. We would suggest that these are shared by



## VI

*An interesting*

The ~~second~~ set of responses to the Bloor, Barnes and Shapin brand of relativism does not consist in a direct rejection of the propriety of the sociological attitudes which are adopted. Laudan, for example, thinks that the case that is made out is vitiated by four fundamental weaknesses.

1. The strained conception of science that is used.
2. The inscrutability of the notion of a 'science of science'.
3. The unnecessary attempt to produce a symmetry between true and false beliefs.
4. The failure to demonstrate the empirical basis of the claims that are made about science.

Objection 4 is exactly the same as the reservation that we have already expressed concerning the levels of claims that can be made on the type and volume of data usually cited by sociologists of science such as Bloor. We have suggested that such generalisations as are made are, in fact, imputations and leading hypotheses of the form "Given the background and general inclinations that have been identified, it is not unreasonable to suggest that... ..". This, of course, is much more qualified and tentative than the more exuberant supporters of the sociology of science are prone to make. Objection 2 is a little more difficult to specify. Part of the defence that is offered of the findings of the sociology of science is that they are the product of a 'scientific' set of procedures, or mode of analysis, or theory. Laudan is unsure what 'scientific' is supposed to mean here since Bloor, (the particular case Laudan is discussing) does not specify just what he means

by the term. Laudan has an important point here but he does not make it forcefully enough. The issue is not, as Bloor seems to think, whether a scientific study of science is permissible ( that issue is just a red herring) but as we considered earlier, whether a Durkheimian sociology provides the acceptable basis of a scientific sociological approach to science. As we said above, what is necessary from Bloor <sup>are</sup> ~~is~~ not expostulations about the right of sociologists to take an interest in science, but a justification of a Durkheimian sociology pure and simple 50 years after Durkheim's death, and after the welter of critical appraisal that Durkheim's work has been subjected to in the meanwhile. Simply calling Durkheim's sociology a scientific approach convinces no-one.

In objection 1, Laudan is dealing with what he takes to be Bloor's eccentric conception of the philosophy of science. This notion, he claims, allows Bloor to construct a strawman, the weaknesses of whose arguments are then easy to reveal. ✓ Laudan is ~~(to~~ <sup>are</sup> a great extent right) about this. Bloor does tend to talk as if the philosophy of science was nothing but the edification of science and the eulogising of scientists. He does appear to think that most philosophers view science as proceeding in an intellectual and social vacuum. Whereas, of course, as philosophers of science what they are interested in is the intellectual foundation, the play of ideas, the logical structure. They are specifically uninterested in sociological questions about social circumstances and social determinations. As we argued earlier, these cannot bear upon philosophical matters.

The symmetry between 'true' and 'false' beliefs that Bloor and Barnes wish to argue for is a symmetry with regard to causation. The same social

factors produce true and false beliefs. This line is demanded because, it is alleged by Bloor et al, philosophers of science want to say that true beliefs are self-explanatory. However, what is fact is being said is that individual beliefs are not encountered in isolation and not defended in isolation either. They form part of a web of beliefs and knowledge and the rationalising procedures associated them. It is this kind of thinking that is expressed in Hesse's network model, which shares with other philosophies the proposal that the network or web provides the sufficient and necessary conditions which explain the holding of particular beliefs.

However, the point which Laudan does not make and which ought to be made here is that Bloor is in error in supposing that what is the object of explanation is the whole concatenation of knowledge and belief. It is this which is rejected by modern philosophers. <sup>So</sup> ~~What~~ <sup>are</sup> to be explained are individual beliefs not the whole complex of knowledge and belief held by members of a society, a group or an institution. We do not need an explanation of why people hold the beliefs they do; we have an explanation, they are their beliefs. What we might want to explain, though, is why given they believe this and that, they also believe in the following. So, we might have a problem and wish to explain why Newton could develop a mechanistic physical theory of light while holding to a 'metaphysical' motion such as gravitation. Scientific-type causal explanations might have a place here, but they have no place as explanations of the whole gamut of scientific beliefs associated with the 17th century. What philosophical discussions of science usually come to is the observation that scientific causal explanations are very peculiar things provided for

distinctive sets of events and states. They do not have universal application. A great number of phenomena simply are not amenable to scientific, causal-type explanation. One such phenomenon is the generation and sustaining of complexes of knowledge and beliefs. The philosophers who advocate this line of thought would gain a fair amount of satisfaction and support from the fact that the causal explanations which Bloor and the other sociologists of science actually offer turn out to be little more than functional descriptions.

No philosopher would dispute the symmetry of explanation for particular true and false beliefs. They do seek to explain them in the same ways. Since most would say that the compendia of beliefs associated with science or magic or whatever are not to be explained causally, or, indeed, that no explanation for their being in place is necessary, then the symmetry argument of Bloor's becomes an irrelevance.

## VII

Let us summarise our discussion so far. In this first part, we have discussed recent developments in the sociology of knowledge associated with the sociology of science. We have suggested that there are two main clusters of problems to be seen in this work to which we can see no resolution. The first concerns the philosophical foundations adopted and claims that are made. We have suggested that neither the advocates of the sociology of science in its relativist form nor their opponents has an overwhelming set of arguments in their favour. This, we suggest, is because

~~47~~ 39.

both sides subscribe to the view that sociological findings can bear upon epistemological questions. This is a view which we would question. Once this is put on one side, then the key issue becomes the methodological adequacy of the kind of sociology of science that is being offered. We have suggested that what is necessary for this sociology to gain credence is first a clear and sound justification for the disinterment of Durkheimian sociology in its pristine state, and second a demonstration that the level of generalisation claimed can be supported by detailed examination of enough appropriate data. On both counts we have found, the sociology of science wanting. But, as we shall see, in its proclivity for Generalisation <sup>31</sup> Rush and Empirical Stretch, the sociology of science is not alone. These and other tendencies are to be discerned elsewhere in the sociology of knowledge.

## VIII

In the first part of this paper, we considered the recent controversy over the sociology of science. In doing so, we picked out three themes around which to organise our discussion of relativism, the incommensurability thesis and the social determination of knowledge and attitudes. The first of these was the level of theoretical aspiration to be discerned. We expressed the opinion that the relativist tendency in the sociology of science looked set to mark a regression in sociology not an innovation. We came to this judgement, not ~~simply~~ because the work of Barnes, Bloor and Shapin deliberately espoused what are passé concepts and theoretical tools, but because their deployment of them appeared to be so uncritical and insensitive. In addition, we came to the conclusion that the net result of this line of work was likely to be the repetition of a classic error in the sociology of knowledge, namely the conflation of sociological arguments, interests and results with philosophical ones. Second, we gave voice to some severe doubts about the support that the data available can give to the generalisations usually made. We suggested that, in the ~~end~~, many of these generalisations took on the character of tenuously grounded hypothetical imputations. This was not to say that they were unreasoned nor implausible, but that far too much emphasis was being placed on them for their preliminary and provisional status to be appreciated. Progress would only be made, we implied, when technical difficulties and improvements have been made. Third, and for us by far the most important, was the sociological nature of the interest

displayed in scientific knowledge. This, we argued, was distinctive from  
 and not merely complementary to a philosophical interest in knowledge and  
 truth. Now matter how much we might disagree with any of the procedural and  
 conceptual formulations of the relativist thrust in the sociology of science,  
 we are in no doubt that they are engaged in a serious, bona fide, proper  
 sociology. These themes will be present in the next part of our discussion,  
 although in a somewhat altered guise, perhaps. However, our discussion will  
 differ in some important ways. The first of these is a product of the fact  
 that our next subject does not fall squarely within the sociology of knowledge  
 as that is traditionally conceived, but is an attempt to synthesise and  
 integrate sociological findings, theories and methods with those of <sup>socio-</sup>linguistics.  
 Any confusion that arises, then, is likely to be the outcome not of an  
 error or inadvertance but as the concomittant - the risk to be run, so to speak-  
 of a deliberate policy. As a consequence, we will feel free to be a little  
 more sharp than we have been hitherto when, in our opinion, serious and obvious  
 methodological and other difficulties have been disregarded. Those who when  
 operating on the margins of disciplines, seek to move with absolute freedom  
 across disciplinary boundaries, have ~~as~~ as a first priority to see that when  
 they do so they abide by the standards obtaining locally. There is another  
 crucial difference to be noted here which will affect our approach. Of all  
 of the discussants in the sociology of science case, only two, Gellner and  
 Jarvie, sought to broaden the area of controversy beyond the praochial limits  
 of the epistemological and the methodological to the moral and the political.  
 The relativist camp steadfastly refused to countenance this move. In contrast,  
 the proponents of 'critical linguistics',<sup>32</sup> the theoretical and substantive

we suggested that the sociology of science was an example of just one style of sociological  
 analysis, the dependent/independent variable kind and not definitive of any kind of sociology.  
 We would offer an organisational approach.

body we shall discuss, have the clear and avowed intention of making the point of their analyses political. This means that the politicisation of linguistics as an end of the analyses offered will be likely to influence the way that arguments are made, inferences drawn and cases discussed.

Some appreciation of the net effect of these differences and a place to begin our account can be gained from the following quotation.

((INSERT LANGUAGE AS IDEOLOGY P15-6))



At first sight, what we have here is a fictional but easy to recognise instance of mild familial bickering. A wife asks what we are invited to see as a pointed question, and gets an exasperated, rather brusque reply. However, Kress and Hodge do not want us to leave matters there. For them, this is an example of the presentation and transformation of <sup>a</sup>version of reality. We are asked to look beneath and beyond the mundane to the deeper meaning which the incident has.

What do Kress and Hodge mean by calling this incident one concerned with the presentation and transformation of versions of reality? What is a version of reality? The term here seems to be being used as a generic label for classifications. The story has the classification of two people as 'husband' and 'wife', and then attaches some obligations or duties to one of them. The husband is expected, by the wife at least, to empty the garbage. This seems to be what is meant by a version of reality. The wife, of course, says nothing about reality and it is only because the question she asks is seen as having the implication ( what Grice would call the implicature)<sup>33</sup> it does that there is any question of ~~even~~ the classification of persons entering here. It is the husband's reaction which Hodge and Kress use to find a version of reality in the wife's utterance. It is not there on its own, so to speak. And even then, what they find is a specific expectation about a particular individual. The expectation may be real enough, and so might the person, but as we pointed out in our discussion of theories of reality in the first part, it is a long and unsecured step to ~~go~~ <sup>take</sup> from talk about particular real things to <sup>talk about</sup> reality-in-general. It is, of course,

perfectly possible to imagine that some very peculiar encounters might constitute examples of the presentation and transformation of versions of reality. But even here we would be dubious. Often when a theory or version of reality is under discussion really what is being talked about are particular kinds of religious, scientific and philosophical theories, which is to say certain very delimited theoretical objects are being talked about in ways that relate to individual bodies of thought. A debate between a creationist and a non-creationist would be a debate about the scope and grounding of a biological theory, not about a version of reality.<sup>34</sup> So too would an argument between a Newtonian and an Einsteinian.

So, whatever our homely example is about, one thing is certain it is manifestly not about theories of reality in any serious sense, and saying that it is is a case of unmitigated hyperbole. Even if it is ever possible to talk about theories of reality being in competition with each other, or being transformed into one another, (and as we pointed out in the first part this is very dubious), there are none on display here. Of course, the anecdote is always subject to what we like to think of as the Amartya Sen proviso;<sup>35</sup> but even then it fails by its own tests and does not do what Kress and Hodge claim for it.

If the suggestion that there is a transformation of versions of reality to be seen here is so absurd, how do Kress and Hodge support their claim that it is so? They do this by the following 'explanation'. The sentence 'Has the garbage been emptied?' is a transformation into the passive mood of the active sentence 'Have you emptied the garbage?'. This

transformation is effected in the following two steps:

- 1 Have you emptied the garbage  $\Rightarrow$  Has the garbage been emptied  
by you
2. Has the garbage been emptied by you  $\Rightarrow$  Has the garbage been emptied

This process of deletion and substitution is, for Kress and Hodge, a direct recapitulation of the processes by which the husband came to understand what his wife said.

If we reconstruct his preconscious of interpretation, it might go something like: this question is about the garbage being emptied, though it doesn't say by whom. Who does she mean should have emptied the garbage.- oh, me! This sentence is really about me emptying the garbage ( or actually about me not emptying the garbage). Why can't she come straight out and say so! This constant sly nagging really gets my goat: I'll tell her.

( Kress and Hodge. p 16)

And, furthermore, this transformation is a deliberate ploy used by the wife to attempt to manipulate her husband.

It is equally clear that her transformation of the linguistic form served very specific and, as she felt, very necessary purposes.

( ibid p 17)

While this example may be a trivial one, it displays most clearly what we think is the fundamental mistake that 'critical linguistics' is predicated on. As students of language use, linguists have, of late, been primarily interested in two things; (a) how well formed sentences are put together in arrays or strings: (b) how such sentences acquire meaning. The first is the concern of syntax; the second of semantics. <sup>36</sup> The descriptive and explanatory strategy that has been used in both of these areas has tended to be based on two components. First there is the setting out of a hierarchy of forms of sentence. Some are considered primitive or <sup>more</sup> basic <sup>than</sup> to others.

Thus the active mood is basic for the passive and the subjunctive. Second , there is the provision of mechanisms by which the transformations of the basic into the less basic forms can be traced out. For syntax this has taken the form of transformational grammars; for semantics, <sup>meaning has</sup> there have been provided <sup>by generative</sup> strategies of deletion and substitution. Both Transformational Grammar and <sup>Generative</sup> Transformational Semantics have achieved a great deal in linguistics and remain interesting areas to work in.<sup>34</sup> The attraction of both is that they provide general and systematic procedures for deriving what appear as complex patterns from simple components. The complexity is dealt with as structured simplicity. <sup>When we discussed the sociology of science, we called</sup> The explanatory strategy just identified ~~has the character of what we called with reference to the sociology of~~ science a rationalising device. And as a rationalising ~~it~~ device it has clear investigative potential. It provides a way of reconstructing in an idealised way, <sup>in this case</sup> for linguistic purposes of classification and grammatical relation, the myriad types of utterances that constitute language use. The one thing that such a reconstruction manifestly is not is a psychology of any sort, although it could be tied to a psychology of a particular kind. It does not claim to represent the thought processes of individuals. It is descriptive of their <sup>linguistic</sup> competences not their <sup>psychological</sup> performance.<sup>35</sup> Without any argument or justification, Hodge and Kress take over the linguistic rationalisation strategy and use it as a psychological performance device. This is the order in which the thoughts occurred. Now, of course, they could always claim that we are being over hard here, that the illustration is simply an illustration of what could be done. And it is precisely that we wish to take issue with. It is the supposition that linguistic analytic procedures can be imported into social psychology without any real difficulty

and without any justification which we would dispute.<sup>37</sup> In our discussion of the sociology of science, it will be remembered, we found sociologists of knowledge apparently willing to treat sociological findings as relevant to philosophical disputes. Now we find sociological conclusions being drawn from linguistic analytic procedures. *To show just how wrongheaded we think this is we will now* ~~Let us now have a look at this~~ *look at the arguments that are put up in defense* apparently bizarre methodology. *of this kind of analysis.*

## IX

The analytic framework which underpins 'critical linguistics' is set out as follows in Language and Control.

1. Forms of social organisation influence linguistic structure and linguistic usage.
2. This influence operates in a deterministic fashion: social structure x demands linguistic variety a.
3. The process may be unconscious or, if a speaker does know what is going on, he or she is under great pressure not to resist it.
4. Social structure bears on all parts of language, not merely those parts that are 'about' personal and group relationships such as personal pronouns or the labels for classes and roles.
5. Different forms of language should not be regarded as cognitively equivalent. They are not 'merely stylistic' in effect, but affect the potential expression of concepts, and thus the availability of concepts, too.
6. Prominent among the social structures which influence linguistic structures is inequality of power.
7. Language not only encodes power differences but is also instrumental in enforcing them.

( 1979: 194-5 )

The same ideas are somewhat more condensed in the Preface to Language and Ideology, but nonetheless the continuity is observable. Here the

basic propositions are:

1. Language embodies specific theories of reality;
2. Variation in discourse, and hence theories of reality, are correlated with social structural differences;
3. Language is both part and product of social process. It constitutes meanings and hence determines social structure.

Whatever else one might want to say about these two lists of propositions, there is one thing to be said for them. They are crisp and clear. What they add up to is a theory of co-determination. Social structure ( in this instance, class) determines discourse which in turn determines theories of reality which in turn determine social structure. Language, class structures and theories of reality are mutually self-reproducing. In Language and Control, Fowler et al define the connections between language, social structure and theory of reality ( ideology, in their terms) as causal links. One of the things we shall have to be on the look out for, then, is precisely what sense can be given to this notion of causality. Is it causality in the classic sense? Or is it merely a general designation which could be translated by 'has the consequence of'? It will be remembered that, on inspection, the causal analysis of the sociology of science turned out to be functional description under another name. Causal analysis in 'critical linguistics' may well turn out to be the same thing.

For brevity's sake, we will concentrate for the moment on the second set of propositions. As we say, they are summaries and condensations of the first. Proposition 1 proposes that languages embody specific theories

of reality, a position that is often referred to as the Sapir-Whorf hypothesis,  
 Whether or not 'critical linguistics' actually adopt the positions set  
 out by Whorf and Sapir is a matter to which we will return. Despite its  
 assertive character, proposition 1 really functions as a methodological  
 dictum for the sociologist. It says 'Treat languages as embodying theories  
 of reality'. If it is to be taken as a proposition ~~to be taken~~ *about how things are*, then it  
 it is outside the realm of sociology, *falls well with* and in that of metaphysics. As many  
 commentators have pointed out, it was precisely because it was a metaphysical  
 proposition that the assertion was both attractive and generative for Whorf.  
 The important upshot of this, as we already have pointed out, is that  
 methodological dicta are not to be assessed for their truth status. They  
 do not state things about the world, but provide ways of bringing the  
 world under investigable scrutiny. If you suppose that languages embody  
 theories of reality, you can generate investigations of just how those  
 theories might be displayed and supported. Most of what we want to say about  
 critical linguistics can be traced, in the end, to what we think is their  
 confusion of methodological dicta with scientifically proven assertions  
 or propositions.

<sup>+</sup>  
 Proposition 2 is an empirical generalisation. Such generalisations can be  
 inductive or axiomatic. An inductive generalisation would be the outcome of  
 accumulated studies which did not presume a priori that there is a structure  
 to language use and a structure to social life nor that if there are such  
 structures whether they are correlated or not. If the generalisation  
 is axiomatic, it is not a finding at all but a product of the premisses  
 and methodological presuppositions *which have been* ~~we have~~ adopted. A second thing to look

for, then, is some clarification of the status of the generalisation about the correlation of discourse (notice not language but types of language) and social structure. If, in the end, all it turns out to be is a working hypothesis, there is no disgrace in that. As a working hypothesis it can act as a simplifying procedure and so have a pay-off in the kinds and power of the investigations it allows.

Proposition 3 is, by any standards, a sociological truism. It is not a finding of any sort but the central tenet of <sup>one version of the</sup> ~~the~~ sociology of knowledge in another guise.

There are, therefore, <sup>leaves</sup> several problems left hanging. <sup>↓</sup> This schematic outline. As we move through the detail of the arguments and cases, we will try to resolve the problems and see how far clarification of the obscurities is forthcoming. Before we do that, though, it might be as well to try to get a general view of the overall shape of the enterprise as a going concern. What is it aiming to do and how does it try to do it?

Early on in Language and Ideology we are told "The grammar of a language is its theory of reality". (p 7). A grammar is a canon of rules descriptive of the linguistic practices of competent speakers. Using it, analysts could <sup>generate</sup> ~~generate~~ recognisably correct sentences <sup>for</sup> in a language. ~~What~~ A grammar provides the rules for ~~the~~ the combination of words and phrases into sentences, and the stringing together of sentences into utterances. A grammar, then, is <sup>what</sup> ~~an~~ idealised depiction of linguistic practices. <sup>however, is</sup> A theory of reality is <sup>what</sup> ~~thoroughly unclear~~ ~~notion~~. One sense that could be made of it, and, we think, the one that Fowler et al intend, is that such a theory represents how objects and states of affairs in the world



relate to one another as a whole. A theory of reality is a holistic theory. In the first part of this paper, we encountered this idea and called it a metaphysical ~~thesis~~<sup>s</sup>. Theories of reality are metaphysical theories. The proposition that the grammar of a language is its theory of reality, is, in fact, suggesting that the ~~fe~~<sup>is</sup> is at least a parallel if not an identity between the grammatical possibilities for combination and classification of words and the ontological possibilities for combination and classification of things. Given that we know that grammars vary ~~as~~ between languages, we can say ~~that~~<sup>that</sup>, therefore, ~~ontologies~~<sup>that</sup> will vary between languages, a thesis that is known as "linguistic relativism".

A first glance. linguistic relativism appears to be saying something like this. On the one hand there is the way that, in fact, the world is. On the other hand, there are the ways that we perceive the world. Because we cannot perceive the world in theory-neutral terms, we will always employ classifications and categories which are theoretically derived, ~~and~~ hence those theories are embodied in our language. Because of this, there can be no independent of theory ~~[means]~~<sup>for</sup> measuring the fit between one set of classifications and reality itself. Being unable to settle which ~~theory~~<sup>ory</sup>, in fact, captures the way reality really is, we have to live with metaphysical pluralism. This is extended into an incommensurability thesis by the suggestion that when we come into contact with languages which appear, from their grammars, to have metaphysics wholly at odds with our own we cannot simply dismiss them as mistaken. We have no way of determining outside of language what is correct or incorrect.<sup>40</sup>

We can raise and then put on one side, two of the more usual objections

to linguistic relativism. The first of these simply remarks on the pointlessness of talking about the world as independent of our modes of thinking and expression if we can never grasp the world except through these modes. The world is the world mediated by thought and language. This objection mounts what we might call the attack from the left, since it aims to take the relativism of linguistic relativism and radicalise it into thoroughgoing scepticism. The point about such a position is that it is both consistent and not subject to refutation. It does not allow the objector a toe-hold from whence an objection can be mounted. The second objection asks whether there are any serious grounds for supposing that grammars or languages as entities state theories at all. What could be used as evidence that they do? This objection asks, then, not about the differences in grammar between, say, Apache and English and the correlation of these differences with metaphysical differences. Rather it asks what reason we might have for supposing that English or Apache have theories of reality embedded in them. We will return to this important objection later on."

Linguistic relativism is used, then, to give us metaphysical variance between languages. The next question to ask is whether such variance is organised in any way. It is here that the sociological generalisation finds its place.

"Language...involves systematic distortion in the service of class interests.

(Language and Ideology. p 6)

The theory of reality locked up in any language is a class determined theory and functions on behalf of the rich and the powerful. Because

language use involves the continual re-affirmation of this class determined theory, language use solidifies and reproduces the social relationships which underpin the class relationships which generate it.

A first objection to this suggestion might be along <sup>the lines of, earlier, we</sup> what called ~~be called~~ the rationalist critique <sup>ie</sup> (It appears to be self-refuting). If, between languages, we have to accept metaphysical variance and hence theoretical indeterminacy, why do we have to accept that this theoretical account of the relationship between language and social structure is not similarly undetermined? Fowler, Hodge et al cannot have it both ways. They cannot say that there is no way of telling whether the Hopi or ourselves are right about physical states of affairs, but we are right about social states of affairs. For that is to undergird relativism with a sociological absolutism which makes it self-refuting.

We will proceed as if there is some point to the suggestion that the theories of reality embodied in particular grammars are class determined and that this is not simply an idle allegation. What grounds might be offered in support of it? One way, as we hinted earlier, would be through the empirical investigation of actual cases of language use which found first that they were structured, and second that they were class structured. In such a strategy, the class determination of language would be a finding, an outcome, a discovery. If, on the other hand, we are asked to accept the class determination of language use and hence of theories of reality as a departure point, then the point of this pre-supposition would need to be demonstrated in the power of the analyses it allowed [of particular instances]. These cases will not

count as proofs in any sense but as exercises in foremsics. A great deal turns, then, on just what Fowler et al claim that their analyses show and what they can be said to have achieved. For, even if we are convinced of the power, fruitfulness and insight offered by this approach, we will have no alternative to rejecting its major claims if <sup>said</sup> ~~is felt~~ by its authors to be offering some kind of test of the hypothesis that language use, theories of reality and thought are class determined and we come to the conclusion that all they have shown <sup>in such an event,</sup> is that it is possible to analyse language in that way. <sup>Even if we</sup> ~~admire~~ what they do, we <sup>could not</sup> ~~cannot~~ save them from the manifest ~~falsehood of the~~ falsehood of the claims they might make about their work. <sup>There are,</sup> <sup>u</sup> <sup>^</sup> then, two ways to arrive at an estimation of critical linguistics. <sup>One is</sup> through the consideration of the general framework adopted. The other is by looking closely at the analyses which are offered. We will do both.

## X

The framework which locates and legitimates the claims being made is a mode of analysis designated <sup>as</sup> 'Orwellian Linguistics' in Language and Control. <sup>42</sup> Ideology. This is a direct reference to the description in Orwell's 1984 of the way in which language <sup>might be</sup> ~~was~~ used as an instrument of political control.

✓ Orwell depicts (in the novel) a state in which the government has a monopoly of communication media. The mechanism by which mass communication is channelled is a Ministry of Truth which, by disseminating an endless stream

of 'agitprop', information about the state, foreign affairs, current events and so on, ensures that public opinion always matches the pragmatic requirements of the government at any time. The most effective means at the Ministry of Truth's disposal to manipulate the populace in this way is the invention of a novel language for communication known as 'Newspeak'. This form of communication shortens and deletes the subtleties and variations to be found in 'Oldspeak' (ie conventional English). *thereby creating neologisms such as Minutemen* ~~Thus~~ the Ministry of Truth ~~becomes MINUTEMEN~~ *and for the phrase 'extremely'* ~~and the phrase 'extremely unsatisfactory', becomes~~ 'doubleplusungood'. This deletion and substitution channels the ways that people think and hence restricts their possibilities for action. The point of Orwell's novel is, of course, that in a fictional setting it is possible to take elements which can be discerned in the non-fictional world of our daily lives and stretch them so that they take on terrifying proportions. The overt political message of the book, as everyone knows, is that the seeds of totalitarian dictatorships are to be found in the willingness of ordinary people to suffer ~~and put up with~~ restrictions, limitations and loss of rights. The whole point of 1984 is to be found in the scale of the difference between Oceania and our own society. What Orwell does is magnify the differences *while at the same time* ~~but~~ indicate that they are only differences in scale. In proposing that they seek an Orwellian Linguistics, Fowler et al ellide the distinction between the fictional state in 1984 and our own society. They can see no differences between them; or at least, none that matters.

What Orwellian Linguistics is saying, then, is that by shifting from Oldspeak to Newspeak Oceania's government were able to fix a different

theories of reality for the population. 1984, then, displays a species of linguistic relativism. Unfortunately, as John Cook points out in his paper on Whorf,<sup>43</sup> Orwell cannot be counted as an exponent of linguistic relativism as Whorf envisaged it. In 1984, it is the vocabulary which limits and distorts reality. Whorf associates relativism with grammatical difference. More importantly, linguistic relativism refers to what might be thought of as the shape of what we can think, our cognitive map so to speak, and not to what we actually think about. Thus it is to the possibility of discerning causal relations or states of affairs<sup>a</sup> that Whorf is pointing, not to the facts that some things might stand in causal relations to others and some might not. Orwell wants to talk about what can be seen as related to what. The third and the vital point is that Newspeak is the product of a deliberate policy, enacted by those who wish to dominate the proles. It is a policy which, in the book at least, has not been successful. Winston Smith and his friends do not all speak Newspeak. There can be no sense to the suggestion that the shaping of our forms of thought can be a deliberate policy because such a proposition would have to include under its rubric those who were doing the shaping.

✓ It might, As a matter of contingent fact, be the case that some linguistic features operated in the interests of a particular segment of the language community which used them. Although quite how this might be so is more than a little opaque. However, this could not be considered to be a candidate instance of ideological domination because the group doing the dominating would be equally dominated. This, is of course, the self refuting argument once again.

Even if Orwellian Linguistics does not owe any ancestry to Orwell, and

even if it did <sup>not</sup> appear to be massively self-refuting, nonetheless it might be worth taking seriously if it generated interesting and provocative studies which yielded hitherto unrecognised insights into social and linguistic practice. Does this look as if it might be the case? One leading example which the authors use to illustrate the nature of their work is the following. ~~Merlyn~~ Merlyn Rees, then Home Secretary, announced in Parliament that the Government had decided to replace the Official Secrets Act with an Official Information Act. Our authors are appalled.

We habitually accept such perversions of language from Government officers and agencies, and yet there is still a sense of shock at the precision and openness of the lie, the naked insanity of its logic.

(Language and Control p 6 emphasis added)

There isn't a lot of room for mistaking the sentiment here, is there? From the terms that are used and the strength of the reaction, we ~~might~~ <sup>cannot</sup> think that it is the very idea of a control on official information which is appalling. That would be a position we might feel some sympathy towards. But it is not what is in dispute here. Merlyn Rees, they say, is guilty of 'doublethink', saying one thing while meaning entirely the opposite. The one thing that is not going to be disseminated is official information: all that will happen is that more and more things will be labelled as official secrets. Well, that might be so. But we do not see what that has to do with the case in point. The Official Secrets Act is an Act controlling access to and dissemination of officially designated secrets. The Official Information Act, we presume, would refer to all sorts of information compiled by governments including Official Secrets. To propose to replace the narrow Act with a more broadly framed one is hardly ~~to think~~ <sup>an instance</sup> in doublethink. It is to say

simply that Government would like to control a great deal more than Official Secrets. Whatever one might think of that desire, it is not itself based upon a contradiction. Furthermore, as most of us are perfectly well aware that politics is a shabby and mendacious business at times, it does not surprise us in the least that politicians should want to broaden the range of material over which they can exercise political control. <sup>The</sup> ~~To suppose~~ that Rees is engaged in doublethink ~~is to miss~~ the point of Rees' announcement and ~~to arouse~~ the suspicion that Fowler and the rest are determined to find the manipulative operation of crude class politics everywhere, no matter what appearances might be like. ~~to the contrary~~

Of course, what is upsetting Fowler et al is the insincerity of people like Merlyn Rees. They must know, one can almost hear them say, that when they make such announcements they are fudging, misrepresenting and distorting how things are. Or, perhaps they don't. Perhaps it never occurs to them to see that they ~~are~~ engaged in doublethink, because doublethink is beneath the level of consciousness. It is built into the mode of discourse which is used. Fowler et al will not be satisfied with the answer that Merlyn Rees is a politician and politicians are primarily interested in pragmatic politics, in obtaining and maintaining power for as long as possible. That is not a sufficiently deep explanation. Doublethink is built into the language. Let us look at another sphere of life and see if we can get a firmer grip on what might be meant by considering <sup>a</sup> ~~that~~ <sup>point.</sup> Most of us have, at one time or another, had dealings with estate agents and estate agents' advertisements for houses. We know, or have found out by experience, that the phrase 'ideal for the first time buyer' actually means 'is in the worst category of dwelling

Para



in the neighbourhood'; 'compact and efficient kitchen' means 'very few domestic facilities crammed into a tiny space'; 'easily maintained garden' means 'has a patch of grass'. This could be a instance where a vocabulary might be said to be misleading, and even ~~that it is~~ <sup>to be</sup> deliberately so, except that no-one is really misled for long. All one has to do is to visit the house in question to discover that the 'outstanding views' include the local glue works and 'suitable for young family' means that the streets are crowded with yelling children from morning till night. The point about the adverts is to interest a potential buyer not systematically to mislead them, or distort reality. Once the potential buyer is on site the first and most difficult hurdle is over. What is going on, then, is a very practical activity of selling houses, not the philosophical or metaphysical activity of constructing versions of reality.<sup>44</sup> The same goes for the case of Merlyn Rees. And, what is more, we know this to be the case. We know that politicians have to make the best of a bad case, have to make political points, turn everything to their advantage and so on. And as a consequence, we discount what they say. We don't have any difficulty at all recognising just what Merlyn Rees is up to and why he finds it necessary to define things in the ways that he does.

The upshot of all of this is that since language is not tied to any one activity, its use cannot be subservient to the interests and demands of that activity. We use language to do many more things than buy houses, or talk politics. Consequently, ~~it~~ <sup>merely</sup> is impossible for those who practise politics of how selling to control language in their own interests, by imposing their categories and vocabularies. A language is not ~~like~~ something that is amenable to control. This is not, of course, to say that it is autonomous of social life either. Far from it. But it is to say that the relationships

between social activities and language are tricky to conceptualise and one simplistic model of unilateral determination is ~~simply~~ to naive to contemplate. All it can lead to is a persistent determination to find someone responsible, someone to blame, someone manipulating, not matter what the evidence to the contrary might be, and to the error of supposing that the politicisation of language refers to language itself and not to what can be said in a language.

What critical or 'Orwellian' linguistics has done is to pull upon the centrepiece of generative semantics and generative grammar as an explanatory mechanism. <sup>The device that</sup> ~~What~~ generative grammar and generative semantics utilise is the transformational aspects of deep and surface structures. The surface structures - the actual sentences and their grammatical or semantic forms - are localised and contextually formed versions of unified deep structures.. They express deep structural relationships - the form of the thought; the universal grammar. The deep structure is context independent, invariant across occasion; the surface structure is context dependent and hence varies according to occasion. Beneath the apparent heterogeneity and variability of language use is to be found orderliness. In place of pure linguistic ~~analysis~~ phenomena <sup>[in order]</sup> to generate this order, critical linguistics puts social structures ~~rather than linguistic structures~~. Because of the social determination in the transformation of deep structure to surface structure, utterances can have entirely different illocutionary force to that which might be supposed from an analysis of grammar alone. <sup>45</sup> Requests can become orders; questions can be turned into demands. Because the form of the determination is socio-political (ie class in its vaguest sense)

then what we are left with is a form of political semantics. Following the methodological injunction ~~to~~ look for the reproduction of theories of reality, we are now being asked to interpret language use as exercises in the reproduction of ideology. The method of political semantics is to explicate the transformations.<sup>46</sup>

The steps in the dissection of the political aspects of language use are these.

1. Using the concept of a 'restricted coding' derived from Bernstein's work,<sup>47</sup> Fowler et al designate the condensed, elliptical vocabulary of Newspeak as a 'restricted' form of discourse. Following the general theme of Bernstein's analysis ( which he has been back-pedalling on ever since he first proposed it), a restricted coding or mode of discourse ( there is a difference between these two but we don't have time just now to stop and mark just what it should be) is associated with a particular set of social relations, one in which those who use the restricted form are passive, subservient and glad to follow firm authority. Newspeak is a degenerate form of Oldspeak; restricted coding is a degenerate form of elaborated coding. Subtlety and complexity have been deleted out. Despite the somewhat contentious nature of this claim, no evidence for it is adduced, nor is any indication given as to why restricted coding must be tied to particular types of social relations. This is because, of course, no evidence is needed at this point. This is not an argument that is being made, but a series of stipulations which are being drawn up.

2. Once the picture of language use comprised of the co-presence of two alternative codes or modes of discourse is in place, and once the relative availability of these modes of discourse has been stipulated as class determined, it is an easy task to show how the restricted code is a degenerate version of the elaborated code, and hence how authority relations are interposed through its use. The working out of the transformational relationships between restricted codes and elaborated codes follows the same pattern as that for the case of the garbage which we discussed earlier. The illocutionary force of the utterance is gained through the analysis of the transformations which the hearer makes.
3. The condensations and deletions achieve unremarked political domination. The logic of transformation is the logic <sup>of</sup> power relations. As it stands ~~th~~ this looks both puzzling and absurd. Analysis of the regulations of a swimming club and the university exam regulations is designed to show that it is not.
- The regulation that is examined enjoins parents to 'take particular care with untrained children.' This is an element of a sentence which runs 'Please respect the facilities and equipment, and take particular care with untrained children'. This is transformed in the analysis into 'I order you that you will take particular care with untrained children'. This despite the fact that the grammatical structure of the sentence makes the analysis 'I request you please to .....' is equally as plausible. On the basis of this dubious piece of reasoning we are asked to accept

NA. This replicates  
Whorf's mistake about  
word classes. A sentence form  
is taken to indicate  
what it does.  
XX

I have lost patience with this 'analysis' of this example. It might be as well to elaborate on it at some later date.

The addressee supplies the deleted elements from his knowledge of the non-linguistic context in which the speech act occurs. This linguistic dependence makes the situation itself remarkably salient or palpable. In effect, the commander forces the commanded to take direct cognizance of those aspects of the social context which make the speech act valid.- specifically the commander declines to state that he has authority over the commanded (statements breed counter-statements and thus facilitate insubordination) but forces the addressee to acknowledge the power for himself.

( Language and Control. p 30)

As with the Merlyn Rees example, what is at issue here for Fowler et al is the apparent insincerity and duplicity of those in authority and hence the percolation of all our interactions with power relations. This loss of sincerity, they say, is epitomised by the famous who are willing to lend their names and ~~faces~~ faces for the promotion of particular goods. They only do it for the money. They do not really enjoy, like or value the products. They are being paid to say that they do. This, say Fowler et al, is hypocritical speech - a classic instance of 'bad faith'. But such a view could only be held by someone who held his fellow t.v. watchers in low esteem. Does he think that we are duped by this form of advertising? If people buy goods in response to adverts it is because they want to, not because they are morons believing wholeheartedly everything that is put in front of them. But then anything is possible from a collection of people who claim to find in the framing of a set of swimming club regulations an 'embarrassed' copying of the 'dehumanised' form of speech adopted by university regulations! They must think the rest of us are morons if they expect ~~to~~ us to swallow that!

For us, this style of analysis raises three closely related sets of issues. The first is the mode of inference which is used. The second concerns the complete insensitivity displayed to the phenomena under study. The third is the unilateral form of the relationships discussed. Although these are closely related, it would be as well to separate them for the moment.

~~21~~

As we have proceeded somewhat deliberately through the discussions and arguments of Fowler et al, at several points we have been obliged to indicate that we are less than sanguine about the effectiveness of the case they make or the forcefulness of the conclusions which they draw. We have noticed a distinct tendency for them to legislate their way to a conclusion and an inference rather than argue their way there. The general impression that is given is of collections of assertions being allowed to stand although they are unsecured by evidence or by argument. These assertions are then taken over as the 'states of affairs' which have to be explained or analysed. A rather extreme case of this mode of inference is to be found in one crucial part of the argument, namely the discussion of the relationship between language and thought. on p 48 ff of Language and thought.

We are presented with the following sequence of steps.

- (i) A stipulated relationship is held to exist between language and thought.

Thought exists in forms and structures of language -  
conversely the forms and structures of language represent  
thought.

This could be taken to be one of two sorts of claims. It could be an empirical psychological claim, that is it could be an assertion made on the basis of evidence drawn from psychological studies of thought and language. If it is, no such evidence offered. On the other hand, it could be a conceptual claim, namely that the concepts of thought and language were intertwined such that they could not be realistically separated. If this is the case, what arguments are we offered?

None. Instead we are expected to accept this stipulation as the outcome of previous arguments. Unfortunately, as we have already seen, these arguments are less than compelling and hence the stipulation given above is thoroughly unsecure.

(ii) From the suggestion that there is a close association between language and thought, we are nudged towards an operationally defined identity.

Speaking aloud in a spontaneous manner is thus the closest that we can come to the core of thought and to the process of thinking.

Again no evidence is offered for the picture of thought that is being used here nor for the parallel between the process model of language and the process model of thought.<sup>48</sup>

(iii) On the basis of this identity, we are moved towards a behaviourism in which

the process of articulation of language represents the  
the process of thinking.

But this behaviourism is not underpinned by a set of operational definitions of psychological concepts, nor by a strict 'naturalistic' method. It is a methodological convenience. The authors want to write about thought and ideology, but all they have as data is a collection of materials<sup>spoken</sup> on language. As a result they legislate that ordinary speech is the best representation we can have of thought.

Of course, no one would want to deny that there are deep and complex relationships between our use of language, the activities we engage in and our social lives. It is because these issues are so deep and complex that sociology, psychology and philosophy have had such difficulty disentangling them. The depth and complexity immediately warns us to be on our guard against simplistic

versions which are designed only to eradicate all of the serious difficulties. Saying that language 'represents' language solves nothing unless it is also quite clear just what it represents it as, in what form and how far. What are limitations upon the linguistic representation of thought? What are the conventions which it follows? No discussion of what it means to say that the process of speaking represents the ~~x~~process of thinking is forthcoming other than the quite bald statement that the hesitations, reformulations, and mistakes common in ordinary speech are all indicative of the "undisguised processes of thinking". Because they are undisguised in this way, their presence can be used to measure in some unspecified way, the security and reassurance that individuals derive from their social environment.

(iv) The fourth and final step is taken when this tenuous connection and its unspecified nature are used to make the move from speech to menatl attitudes.

Thus spontaneous speech expresses meanings about speakers' perception of social context over and above other meanings carried.

Thus are we carried from the structure of language to the structure of thought, and from thence to the structure of social relations.

A first point to make is, of course, that it would be silly to deny that we do not come to conclusions about people's menatl attitudes on the basis of what they say (among other things). But when we ordinarily do find people being evasive, truculent, apologetic, embarrassed or frightened, we do so as a result of the presupposition of shared cultural experience. We come to these conclusions because we know what people are like, and why they often say the things they do. We take it that what people are like and why they say the things they do, are the objects of investigation in an enterprise



such as the scientific study of language. Consequently we cannot suppose, in advance, that we know how people think <sup>and</sup> the attitudes they take simply from what they say. <sup>That</sup> ~~we~~ we can do that exercise is something to be shown. The one thing the investigator cannot build in to his research methodology as an unquestioned resource, therefore, is the common set of cultural experiences which as an ordinary member of his society he shares with ~~others~~ <sup>the rest of the world</sup>. He has to 'bracket' that experience off, so to speak, and introduce it only after careful consideration of what is being introduced and what for, and then one step and one item at a time.

We can take it that the real object of attention is not the structure of the relationships which might be obtained between language and social activities, but what Fowler et al take to be the same thing, the relationships between activities, social relations and thought. They only pay attention to language because they have to. It is the data they have obtained and the process of thinking, they imply, is not directly observable. Language, then, is an investigative convenience. It offers, to use a figure of Ryle's, the key to unlock the closet of the mind and peer in at <sup>the</sup> ~~the~~ thought processes <sup>within</sup>. This behaviourism is an example of what Laudan calls inference by means of the clock analogy. <sup>49</sup> From the observable features on the surface, the hands, we can infer the non-observable mechanisms which move the hands, the clockwork. By means of the picture of clockwork, we can envisage the wheels, levers, springs and balances that enable the hands to move in the way that they do. However, argument by use of analogy like that of the clock can only be ~~at best~~ possible if we have already gained a familiarity with things like clockwork or closely related mechanisms and phenomena. If we have never taken a clock apart