Realism and Social Science

Some Comments on Roy Bhaskar's 'The Possibility of Naturalism'

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1 Introduction

An increasing body of philosophical work is now available which (a) presents a 'realist' alternative to the hitherto predominant 'positivist' and 'conventionalist' currents in the philosophy of science and (b) attempts to use this realist account of science in the analysis of social scientific practice. In general the objective of this analysis has been to transcend the polar opposition, which has always characterised debate in the philosophy of the social sciences, between positivism and 'humanist', 'hermeneutic', or 'neo-Kantian' dualisms. Commonly the outcome of this work has been to sustain the explanatory procedures of historical materialism, in one reading or another, as compatible with realist philosophy. Further, elements of a realist epistemology have also been attributed to Marx, Engels and other Marxists in their philosophical writings. What is remarkable, though, has been the great diversity of readings of Marxism - ranging from Critical Theory to Althusserian structuralism - which seem to be indifferently assimilable to the realist defence. Since, though, the new 'transcendental' realism is concerned solely with the general conditions of possibility of a number of characteristic forms of scientific activity (experiment, scientific education, etc.), it is neither surprising nor worrying to discover that it is equally compatible with several different, even mutually incommensurate attempts at explanation within a particular science. What might be more worrying, though, is that it appears to be compatible with more than one of a number of conflicting philosophical reflections on those scientific traditions. In part, I shall argue, this difficulty derives from the reliance of the most influential realist account of the natural sciences on consideration of a narrow and inappropriate range of these sciences. The application of the resulting model of natural scientific activity to the social sciences has been problematic in such a way as to reproduce some of the familiar characteristics of the positivist/dualist opposition.

The influential work in question is that by Roy Bhaskar. His first book, A Realist Theory of Science (RTS from now on), made an immense contribution in establishing and systematising transcendental realism as a coherent and well-articulated alternative to the established traditions in the philosophy of science. These rival accounts of science, characterised as 'empirical realism' and 'transcendental idealism', were subjected to formidable criticisms, but almost wholly in relation to their accounts of the natural sciences. In RTS the question of the possibility of naturalistic social and psychological sciences is posed, but not systematically dealt with. Roy Bhaskar's second book, The Possibility of Naturalism (hereafter PN), takes up this challenge, arguing for:

'a qualified anti-positivist naturalism, based on an essentially realist view of science. Such a naturalism holds that it is possible to give an account of science under which the proper and more-or-less specific methods of both the natural and social sciences can fall. But it does not deny that there are significant differences in these methods grounded in real differences in their subject matters and in the relationships in which their sciences stand to them.'

Stated in these general terms, I am in broad sympathy with Roy Bhaskar's project, but on the nature of the differences which he identifies, and their significance, I shall take issue. In particular, I propose to argue that the extent and significance of the natural science/social science asymmetries which Roy Bhaskar claims to identify would justify description of his position as a form of anti-naturalism, rather than as a 'qualified naturalism'. It would follow from this that his intended transcendence of the positivism/hermeneutics polarity is not entirely successful. The failure in this respect derives from the reproduction in Roy Bhaskar's work of the very dualist ontology of a natural/human opposition which
is the basis of hermeneutic and neo-Kantian forms of anti-positivism. This ontology is, in turn, sustained by an unnecessarily restricted conception of the natural sciences. This excludes, or under-represents, the philosophical and methodological characteristics of a range of historical and life-sciences whose bearing on the social sciences, both philosophically and substantively, is direct and most pertinent to Roy Bhaskar's philosophical project.

2 The Argument of RTS

It will be remembered that RTS poses in relation to a number of characteristic natural scientific practices the transcendental question, 'What are the conditions of possibility (presuppositions) of these activities (or their rationality, or intelligibility):" The practices investigated in this way include experimentation, the application of scientific knowledge in 'open' systems, scientific perception, scientific education, and change and development in science. There are, unfortunately, some ambiguities in Roy Bhaskar's posing of these questions, however, which have implications for the status of the answers he gives. Some of these ambiguities, and possible sources of misunderstanding, are cleared up in Chapter 1 of PM, but some are persistent. Most significant are ambiguities surrounding the premises of the transcendental deductions. Are we to take as a premise the existence of a scientific practice, such as experiment, or, rather, its intelligibility, or, yet again, its rationality (in the sense of 'rational justifiability')? It could well be argued, of course, that, since experiment is a symbolically meaningful cognitive practice, it could hardly be said to exist unless it were intelligible. But there remains an important difference between accepting as a premise the intelligibility of scientific experiment and accepting it as rationally justifiable. It seems to me that the strong ontological conclusions of the transcendental deduction follow only from the latter version of the premise, and not the former. In the former, it is legitimate to argue from the intelligibility of scientific experiment to the presupposition that the world has such-and-such characteristics (i.e. that scientists who conduct experiments are thereby committed to the existence of a world with these characteristics) but that the world really does possess these characteristics only from the premise that experimentation is rationally justified. It is, however, my view that these difficulties of articulation can be resolved, and, in any case, they are not centrally involved in this paper's concern with the application of the transcendental realist model of science to the social sciences.

In RTS, then, transcendental arguments are adduced to demonstrate the general characteristics which must be possessed by the world if it is to be a possible object of scientific knowledge, and by society if knowledge, as a species of social practice, is to be sustained. These 'conditions of possibility' of science can be grouped as belonging to two 'dimensions', a 'transitive' and an 'intransitive' dimension, which are characterised as follows: '... a transitive dimension, in which the object is the material cause or antecedently established knowledge which is used to generate the new knowledge, and an intransitive dimension, in which the object is the real structure or mechanism which acts quite independently of men and the conditions which allow men access to it.' In the intransitive dimension, transcendental deduction yields the conclusion that the world is both structured and differentiated. That is to say, that the world (unlike the world of empirical realist epistemology) has ontological depth. It is constituted by mechanisms whose tendencies and powers may or may not be exercised. When exercised, the powers of real mechanisms may not be 'realised', and even when realised, the resulting event-sequences may not be detected by 'man'. The world is differentiated in the sense that mechanisms may exist and operate either in closed systems, where 'constant-conjunction' event-sequences do occur, or in open systems where the outcomes of the operation of a multiplicity of mechanisms are such that constant conjunctions do occur. Characteristically mechanisms in nature operate in open systems: usually, though not always, closure is artificial, the achievement of experimental practice... Laws are 'normic' statements concerning the tendencies or powers of things, which are manifested in the form of constant conjunctions under conditions of closure, but which must be supposed also to exist and be exercised in open systems, where no constant conjunction is manifest, because of co-determination of outcomes by other mechanisms.

In the transitive dimension, RTS concludes, society must be an 'ensemble of powers irreducible to, but present only in the intentional actions of men'5 who must, in turn, be causal agents, capable of intentionally acting on the world, monitoring this activity, and second-order monitoring of this. In the transitive dimension, the 'object' is antecedently established knowledge which is transformed to produce new knowledge.

Now, it follows directly from this that, since social and psychological mechanisms and structures clearly cannot exist and act 'quite independently of men', they are not possible intransitive objects of scientific knowledge. It may be that certain of their general characteristics may be derived by transcendental deductions of the conditions of possibility of natural scientific practices, but here they figure as conditions, in the transitive dimension, of scientific knowledge of nature only, and as objects of philosophical, rather than scientific, knowledge.

Furthermore, since Roy Bhaskar's central arguments have been concerned with the implications of experimental activity, since experimental activity presupposes the possibility of closed systems, and since we are told that social and psychological mechanisms occur only in open systems, there follows a further epistemological obstacle to naturalistic social and psychological
sciences: the absence of experimental practice. Strictly speaking, then, Roy Bhaskar's position in RTS commits him to a radical dualism of the natural and human domains, which further commits him to an epistemological dualism with respect to the possibility of knowledge of these domains:

**NATURAL** | **HUMAN**
---|---
1 Person-independent mechanisms | 1 Person-dependent mechanisms
2 Predictive science possible | 2 Predictive science impossible
3 Experimental practice sustained | 3 No experimental practice
4 Intransitive objects of scientific knowledge | 4 Transitive condition of scientific knowledge only

The outcome of the position adopted in RTS, then, seems to be a dualist anti-naturalism, so far as the human sciences are concerned. But this is not a conclusion which Roy Bhaskar is readily prepared to accept. Though apparently already ruled out by definitional fiat, the possibility of a naturalistic scientific knowledge of social and psychological mechanisms does get discussed in RTS. Roy Bhaskar recognises that so far his central argument has 'turned on the possibility of experimental activity', so either some analogue of this in the human sciences must be found, or we must 'appreciate the great gulf that must separate them from the sciences of nature'.

Throughout the discussion there appears to be an assumed correspondence of experimental sciences with natural sciences, on the one hand, and non-experimental with human, on the other, though this is neither explicitly stated nor defended.

Fortunately, there is an analogue of experimentation in the social sciences. It is that the theories which become embodied in social practices may constitute by participant social actors themselves as incapable of non-ad hoc explanation of significant phenomena (e.g. Neo-Classical economics and the 1930s depression). However, the characterisation of this experiment-analogue in RTS is very brief and sketchy. It also seems to be rather unpromising for any proponent of a naturalistic approach in the social sciences. Society itself is to be understood as a colossal self-constructed and self-interpreted experiment. There seems to be no room for social science as a distinct cognitive practice, with distinctive methods, and autonomous theory, as is the case with the natural sciences. The conception also is comparable in several respects to the Popperian notion of 'social engineering' as the social science analogue of experiment, and is susceptible to broadly similar objections.

However, leaving aside the question of the adequacy of this proposed experiment-analogue, it is important to recognise that the very speculation which gives rise to it - that naturalistic social science might be possible - entails a revision in the definition of the transitive/intransitive boundary. If it is possible even to consider that there may be scientific knowledge of social and psychological mechanisms, then it follows that it must be possible to consider person-dependent mechanisms as potential intransitive objects of knowledge. Since this is this is some revision of the intransitive dimension, then it follows that a revision of this definition is required if consistency is to be restored and the possibility of naturalism explored.

3 The Argument of PN and Some Criticisms

A necessary condition of Roy Bhaskar's project in PN, then, is some revision of the transitive/intransitive distinction, and consequent dispersal of the natural/human opposition. Without this, the impossibility of naturalism follows directly. The first revision of the distinction comes in Chapter 1, where the mark of intransitive objects of knowledge becomes that they 'exist and act independently of the knowledge of which they are the objects'. This revision does allow for the possibility that social and psychological mechanisms, processes etc., at least under some characterisations of them, might be possible intransitive objects of knowledge. It does, however, seem to rule out the possibility in the case of one class of such mechanisms and processes, namely those which constitute knowledge. This problem of the partial identity of subject and object of knowledge is, indeed, a general difficulty for the maintenance of the transitive/intransitive distinction in the human sciences, and Roy Bhaskar later produces a further revision in the distinction to take account of it. We can distinguish between existential and causal independence: such social relationships are existentially independent of knowledge of, but causally interdependent with it. For the social and human sciences, their intransitive objects are existentially but not causally independent of the processes by which they are known.
But of course, to remove one obstacle to the consideration of the possibility of naturalism is not the same thing as to establish its possibility. It is to Roy Bhaskar’s attempt to argue this that I shall now turn, focussing on his argument as it affects specifically social, as distinct from psychological sciences. The main burden of the argument with which I shall be concerned is given in Chapter 2 of PN. Here, the argument is that there are fundamental differences between natural and social objects of knowledge, which constitute ‘limits’ to naturalism in the social sciences, but that these differences are themselves conditions of possibility of natural scientific knowledge in the same sense, but not achieved in the same way as natural scientific knowledge.

It might seem that, in investigating the conditions of possibility, and the question of their satisfaction, of social scientific knowledge, the most obvious method would be for a transcendental realist to apply the procedures of RTS to this new domain. Social scientific practices would be identified, and a transcendental deduction of their conditions of possibility attempted. But, as Roy Bhaskar rightly points out, what is at issue here is precisely the question whether there are any social scientific practices, and, if there are, which they are.

The extension of the method of RTS would simply beg the question in favour, not just of the possibility but the actuality, of naturalism. But the method adopted in PN as an alternative isn’t entirely clear. There are conflicting accounts of it, and the practice of it doesn’t appear to be entirely consistent with any of these accounts. My reconstruction of the argument is, therefore, rather tentative. The argument appears to have three main phases. First, the a priori deduction of certain general properties of societies (and persons). Second, a comparison of these with those general properties of natural objects in virtue of which they are possible objects of natural scientific knowledge. This comparison yields a series of epistemologically significant ontological differences. Third, the attempt to demonstrate that scientific knowledge of social objects is possible, notwithstanding, or rather, because of, these differences.

I shall deal with each of these three phases of the argument in turn. The first phase, the a priori demonstration of the relevant emergent properties of societies, is problematic in several respects. Sometimes the claim is that this demonstration consists in an analysis of the necessary conditions for any form of social life, whereas elsewhere it is presented as a derivation from the analysis of a number of characteristic types of human activity (‘saying’, ‘doing’, ‘making’). The principal argument, however, seems to be one which takes the existence of intentional activity as such as its premise. On all three of these characterisations the argument is a transcendental one - what must be the case if ‘a’ (activity etc) is possible. If we take Roy Bhaskar’s argument that the pre-existence of social forms is necessary for intentional action, for example, this is clearly a transcendental argument. But there seems to be nothing, except, perhaps, its greater generality, to distinguish it from other uses of transcendental argument-forms in substantive social scientific research. Its status as a specifically philosophical argument is in doubt. Its content and plausibility rely on the acceptability of the ‘transformational’ model of human practice which is introduced along with it, and on a specific characterisation of intentional action which is subject to controversy among the different sociological research traditions.

Now, the significance of this criticism is not simply that Roy Bhaskar fails to sustain a distinction between philosophical and substantive enquiry in the social sciences. I am not sure that I would wish to place too much weight on this distinction, in any case, though ad hominem the argument must have some force, since Roy himself devotes considerable space and ingenuity in the attempt to preserve the distinction. Rather, the significance of this criticism is that the procedure adopted in this first phase of the argument involves Roy Bhaskar, after all, in siding with certain substantive research traditions within the social sciences (specifically, Durkheimian, and Marxian, or, rather, some versions of these) against others, and not just in his conclusions, but in his very premise: the characterisation of intentional action. In short this procedure is question-begging just as much as would have been a direct application of the method of RTS. There are, indeed, systematic links between disputes over the proper characterisation of intentional action and disputes over what is and what is not a properly ‘scientific’ approach to social scientific investigation. Similar remarks could be made about Roy Bhaskar’s use of Durkheim’s conception of the ‘coercive power’ of society to demonstrate its sui generis reality.

The second phase of the argument - the comparison of the general properties of societies with those of the objects of the natural sciences with a view to their epistemological significance - is no less problematic. Of course, strictly speaking, if the first phase of the argument fails, then so does the second, but I propose to treat the comparison of natural and social objects in abstraction from the methodological difficulties involved in independently establishing the epistemologically significant properties of social objects. This is partly
because Roy Bhaskar's argument has a great deal of intrinsic interest, and partly, because I am in broad sympathy with some of the most important features of his characterisation of social objects, despite my reservations both about his methods of demonstrating them and about his ways of representing those methods.

Having introduced a limited dispersal of the human/natural opposition (for the revision of the transitive/intransitive boundary) as a condition of even posing the question of the 'possibility of naturalism', Roy Bhaskar now proceeds to re-consolidate that opposition in the forms of a series of ontological, epistemological and relational 'limits to naturalism'. The first ontological difference between natural and social structures is an ontological limit to the possibility of naturalism, is that social structures do not, whereas natural ones do, exist independently of 'the activities they govern'.16 Now, this supposed dis-analogy is imprecisely expressed, and, moreover, does not appear to have been established in phase one of the argument. It is introduced, rather, as if self-evidently true. The most obvious interpretation of 'activities they govern', it simply is not true that the existence of social structures depends on these activities. For example, the concept of a power-structure required in empirical sociological research must enable the investigator to identify power-relations where powers are exercised, even though they continue to be possessed.17 In such cases, the activities constituting the exercise of powers (governed by the power-structure?) are not necessary to the existence of the power-structure (though other activities may well be). The full coercive power of the state, for example, may continue to be possessed without being exercised, though such activities as exercising taxes, the recruiting, training, and equipping of armed personnel may well be necessary to the maintenance of that structure of power-relations. This is entirely comparable with many natural mechanisms. An organism may, for example, never engage in reproductive activity, but yet retain its reproductive system and powers. However, some activities of the organism (such as nutrition) would be necessary to the retention of these powers, but not the ones directly governed by the reproductive system itself.

Elsewhere, Roy Bhaskar offers, possibly as a general proposition including the above, the characterisation of social structures as not existing independently of the exercise of these powers are present only in and through the activities of human agents.18 It follows from this, then, that in the social domain, all activities are activities of human agents. But, to sustain the sui generis character of social structures, it is necessary to distinguish between those activities of agents which are exercises of their own intrinsic powers, and those activities which are really exercises of powers which reside in social structures, but operate through the activities of human agents. Surely, though, if any person 'A' is the agent of an activity, 'a', then 'A' must be the possessor of the power of which 'a' is the exercise. If this is accepted then it follows that, at best, we can distinguish only between powers of agents possessed in virtue of their intrinsic natures, and powers of agents possessed in virtue of their relational properties. Roy's conception of social structures does not, after all, sustain them as autonomous possessors of causal powers, or, therefore, as sui generis realities. Roy Bhaskar, it seems, committed to a variant form of individualism in social science.19

A second ontological limit to naturalism is that social structures do not, whereas natural structures do, exist independently of agents' conceptions of what they are doing. This thesis of the concept-dependence of social structures plays a large part in the argument, but PN, as well as other anti-naturalist works,20 but is subject to varying interpretations which radically affect its epistemological significance. Is the thesis that, in general, social structures exist only if agents have some conception of what they are doing? Now, it seems to me that it has little or no epistemological significance. Certainly, it suggests that, once established, scientific conceptions may be in conflict with facts. But agents' conceptions of agents' conceptions of the same activities. A series of political consequences and problems flow from this, but no special epistemological ones, vis-à-vis the natural sciences, where similar disparities between science and 'common-sense' persist.

At the opposite extreme, the thesis of concept-dependence may be to the effect that the existence of social structures depends upon agents' having the particular conception they do have of what they are doing. Some relationships are, indeed, like this (e.g. friendship). If each party to the relationship changes his or her conception of what the relationship is, then the relationship ipso facto ceases to exist. But many, perhaps most, and certainly the most sociologically significant, for example, common relationships are not like this at all. Where society surrounds and sustains a relationship with sanctions, including coercive powers, social relationships can be, and are, sustained across great diversity of and through immense changes in participating actors' conceptions of what they are doing (lover/emotional relationship, political domination, and marriage are three clear common examples of such social structures).

Alternatively, the thesis of concept-dependence may be taken as specifying a causal relationship between actors' conceptions and the character of social structures, such that changes in actors' conceptions of what they are doing are among the causes of structural change. Such changes may or may not be in line with the intentions of the actors whose conceptions change. Again, it seems to me that this thesis is not obviously wrong. On the other hand, it hardly counts as an a priori demonstrable truth about society as such. Questions as to the
causal relationships between social structures of various types, and actors' conceptions of them are open questions, whose answers require empirical and theoretical research. There is no reason to suppose that any answer universalisable across all types of social structure will be forthcoming. Furthermore, on this version, too, there seem to be no serious epistemological difficulties for the possibility of a social science arising from the thesis of concept-dependence.

The third supposed ontological limitation on the possibility of naturalism is that 'social structures, unlike natural structures, may be only relatively enduring (so that the tendencies they ground may not be universal in the sense of space-time invariant)'. It is, of course, true that social structures may be in fact instantiated for historically limited periods of time, and within geographically restricted areas, but this is quite consistent with their tendencies and powers being universal wherever the appropriate structures are instantiated. To substitute for space-time invariance in the required sense - i.e. spatial-temporal locations are not in themselves causal factors. Oddly, Roy Bhaskar himself seems to recognise this when, later on, he says that social laws may be universal within their range, though restricted in their scope. But precisely the same is true of the laws and structures of the natural world. As Engels argued, the discovery of historicity in nature was a distinctively nineteenth-century achievement, culminating in Lyell's geology and Darwin's evolutionary biology. Natural mechanisms, like social ones, are not eternal, but have definite conditions of existence which may or may not be present at any point in space or time.

If we take into account Roy Bhaskar's later qualification of his position with respect to the space-time variance of social structure, then he is committed to a denial of historicity in nature. This would, indeed, constitute a limit to naturalism in the social sciences. Fortunately, we do not have to agree that natural mechanisms are not historical in character. There are in fact, in which the historicity of the social presents dis-analogies with the historicity of natural mechanisms which might be held to have epistemological consequences. It is generally the case that the historical changes which require basic conceptual distinctions in their science (i.e. 'qualitative' changes, where these uses of this historical periodicity which is very great in relation to the periodicity of conceptual change in science itself. In all cognitively relevant senses, then, it can be said that the world which is grasped through the categories of a science following a revolution in that science is the same world as was grasped, perhaps with less penetration, by the superseded categories of the science. No major new division of living organisms was, for example, emerging contemporaneously with the Darwin/Wallace production of the concept of natural selection and which itself rendered earlier theories obsolete. Now, scientific revolutions and cognitive advances generally are social processes. When they take social processes as their objects, too, their ob-
jects have a temporal periodicity of change which is of the same order as the periodicity of change in the knowledge-process itself. Now this certainly can give rise to methodological problems in the social sciences - particularly with respect to long-term historical prediction. But epistemologically speaking, the situation is quite comparable with the social sciences. Such a mechanism is, indeed, suggested in RTS and is a familiar feature of some historicist Marxisms. Such a necessary correspondence is, however, quite incompatible with a conception of science as a distinct and relatively autonomous cognitive social practice which Roy Bhaskar (most of the time) and myself, too, would wish to sustain.

Next, Roy Bhaskar presents, as an epistemological limit to naturalism, the argument, familiar from RTS, that social mechanisms exist only in open systems and that, therefore, controlled experiment, prediction and decisive tests of theory are impossible in the social sciences. In answer to this, it is first necessary to ask whether decisive tests of theory are possible in the natural sciences either. Even with an experimental closure of the classic kind, assumptions have to be made in practice about whether a closure has, in fact, been obtained (i.e. an assumption of the non-interference of undetected extrinsic influences on the instance of the mechanism under investigation). Theoretical assumptions also have to be made concerning the characterisation of the mechanism and its activities, as well as the instrumentation employed. Of course, Roy Bhaskar is, in other contexts, well aware of this, but the sharp natural/social science contrast he draws can only be understood, I think, in terms of a residuum of the positivist conception of the experiment/prediction/testing relationship in his thinking.

Connectedly, it seems to be presupposed in Roy's argument that the constant conjunction of events associated with closure is necessary for prediction. Why should this be so? What is to rule out the calculation of the resultant effects of the joint operations of several of the causal factors. Of course, prediction is always, of course, prediction of something under some description. Where very complex systems of interacting mechanisms, operating under conditions and for initial states which may be known only approximately, outcomes may only be predictable as falling within a certain range of possibilities. Of course, it might be argued that even if such a system of 'prediction', the outcomes of multi-mechanism ('open') systems are only predictable if it is possible to first isolate each constituent mechanism to examine its operations independently and its relations with others. This, of course, is a methodological problem of the social sciences but not, it seems to me, an epistemological one.
Durkheim, for example, in his classic work on suicide, uses elementary statistical comparisons in an effort to demonstrate that a definite coefficient of preservation or aggravation is associated with each of several different religious ways of life. The purpose of the statistical comparison in each case is to rule out the possibility that a given outcome (in this case, suicide rate), or given contribution to such an outcome, results from some other mechanism (minority status in a society, persecution etc). Of course, Durkheim's implementation is susceptible of criticism, but the principle is clear, and more sophisticated (though still, of course, problematic in various ways) statistical techniques have since been developed. In cases like this, the isolation of mechanisms is achieved theoretically and theory is corrected on the basis of statistical comparisons of differently constituted systems which nevertheless have one or more mechanisms in common.

Finally, but most importantly, Roy Bhaskar again argues that amongst natural sciences in which experimental closure is not an available means of empirical control on theory. Historical natural sciences such as biology and evolutionary biology explain phenomena in terms of the interaction of pluralities of mechanisms in open systems. In each of these sciences mechanisms have been developed - many of them directly comparable to the uses of statistics in sociology - for including an element of empirical control into theory, production and theory-correction. The classic experimental closure is one technique (class of techniques) among many, which is available in some, but by no means all, natural sciences. Roy Bhaskar's critique of the empirical realist, 'constant conjunction', conception of causal laws is insufficiently radical in that it retains a certain paradigm of experimental closure and its role in the testing of theories in common with the 'constant-conjunction' account. Again, the result of this is an artificial and unnecessary natural/social contrast.

Finally, Roy Bhaskar thinks there is a 'relational' limit to naturalism. This derives from the familiar thesis of the partial identity of subject and object of social knowledge. Knowledge is itself a social practice, so that when it takes social practice as its object, maintenance of the distinction between the intransitive and the transitive objects of knowledge is problematic. As I have already indicated, though, Roy Bhaskar distinguishes existential and causal independence of the intransitive objects of knowledge. In the social sciences, it is possible to sustain the existential independence of social structures, etc., whilst conceiving its causal connections between subject and object of knowledge. But the same is true, surely, of the natural sciences. Experiment, for example, as RTS well argues, presupposes causal interaction between natural systems and human agents. If these points are recognised then continued commitment to a natural science/social science dualism on the basis of the 'partial identity' thesis must derive from some conception of the special or distinctive status of self-knowledge, such as would be sustained by a residual Cartesian conception of the subject. This is, for example, the metaphysical basis of Lukacs's classic formulation of this natural/social science opposition.

The result of Roy Bhaskar's comparison of social and natural objects seems, then, to be a series of concessions to anti-naturalism, such that his position would be better described as a form of anti-naturalism, rather than as a naturalism, however qualified. Nevertheless, he remains committed to the possibility of a scientific social science, if not a naturalistic one. But the greatest obstacle to even this - the absence of prediction and experiment in the social sciences - remains to be removed. As in RTS, the search is for a social-science analogue of experimentation. This time, it is the epistemological significance of social crises which seems to offer promise of a solution. If it is supposed that during periods of social crisis, the underlying generative structures of society become visible to them, then one result of crisis will be a transformation of participant actors' conceptions of their activities. These new conceptualisations may now serve as raw materials in the production of new knowledge of the social form.

There are, however, some serious difficulties in the way of such a process providing even a partial analogue of scientific experiment. First, it appears to be a condition of production of new knowledge, rather than a means of empirical control or correction. Second, it seems to entail that social scientific knowledge is possible only for those societies characterised by periodic crises of the required sort (capitalist societies?), unless there are yet other experiment-analogues appropriate to other types of society. Third, Roy Bhaskar gives us no theoretical account of the visibility/invisibility of generative structures and, surely, even if he could, this would beg the epistemological question. Finally, one universal feature of social crisis which is difficult to reconcile with Roy Bhaskar's epistemological requirements is that they polarise populations ideologically and politically. If actors make sense of the newly visible generative structures in profoundly diverse and antagonistic ways, what sense it is still possible to make of metaphor of 'visibility', and how are we to solve the problem of which actors' conceptions are adequate raw materials for scientific transformation?

It seems, then, that Roy Bhaskar, having
minimally dispersed the natural/social opposition as a condition of posing the question of the possibility of naturalism, goes on to reconstitute that opposition. The resulting philosophy of the social sciences is anti-naturalistic, and seems incapable of sustaining the possibility of evolutionary naturalism. The ontological opposition of the natural and human domains continues to affect the epistemological argument throughout The Possibility of Naturalism, determining concessions to anti-naturalism which are quite unnecessary. The ontology of the natural/human opposition is itself sustained by the unduly restricted range of sciences (mainly, though not exclusively, physics and chemistry) and, therefore, of scientific practices, which are paradigmatic for the model of science constructed in RTS and presupposed in PN. This model of the natural sciences has in common with the logical empiricism which it so effectively refutes that it under-represents historicity and development as epistemological and methodological characteristics of the objects of the natural sciences. Evolutionary biology, cosmology, geology, embryology are all natural sciences for which historicity and qualitative transformation pose epistemological and methodological problems which are in many respects directly comparable with those encountered in the sciences of human history, society and psychology.

It is also a characteristic of these historical natural sciences that the explanatory models they employ designate mechanisms which are not practically isolable in experimental closures. If the impossibility of closure is an epistemological obstacle to a scientific sociology, then it must also be so for this range of natural sciences. In fact, a great diversity of non-experimental means of empirical control and correction, as well as adaptations of experimental methods themselves, have been developed in these sciences. This is true just as much of the historical social as of the historical natural sciences. If we consider, for example, the range of empirical controls implicit in the developmental explanations of the later correction of Darwin's evolutionary biology, it is easy to see that these by no means all fit the paradigm of the classic "experiment". An important raw material for Darwin, which both establishes the possibility of organic transformation, and sets definite limits to the range of possible mechanisms which might be supposed to bring it about, are rule-of-thumb generalisations derived from stock-breeders and gardeners. These are forms of reflection on non-experimental human interventions in nature, which rule out certain theoretical explanatory possibilities, and set definite target-requirements for theoretical reasoning.

Another important set of empirical controls was the range of theoretically informed observations of the geographical distribution of living forms, together with palaeontological evidence of their historical succession, and the geographical distribution of "related" forms. Again these are evidences from a non-experimental source which tell against the idea of special creation, and for the notion of common descent by gradual transformation. As to the mechanism of natural selection itself, of course, no experimental demonstration of the formation of new species by its agency is available, but the subsequent development of such adjacent sciences as genetics and ecology have both sustained and modified Darwin's conception, whilst elements of the process are relatively isolable, and have been examined by means of adaptations of experimental technique. For example, numerous investigators have exposed different colour-varieties of insect larvae against various backgrounds in the vicinity of the nests of insectivorous birds to discover differential rates of predation on them. These 'experiments' can be combined with statistical 'thought-experiments' to determine the effects on the gene-pool of a population through successive generations of such differential predations.

Finally, on the 'human' side of Roy Bhaskar's natural/human opposition, there is an unwillingness to conceive of forms of historical causality as really distinct from individual human agency. I set down the problem of the argument for the sui generis reality of social structures. It is this remnant of what has been called 'the problem of the subject' which further sustains the ontological and epistemological dualism of PN.

At the beginning of this paper, I quoted Roy Bhaskar's advocating an anti-positivist naturalism, according to which 'it is possible to give an account of science under which the proper and more-or-less specific methods of both the natural and social sciences can fail'. It seems to me that the anti-naturalist conclusions of PN are part of a demonstration that RTS failed in this respect and that the model of science produced in that work requires revision to take into account, in particular, epistemologically significant characteristics of historical, developmental, and non-experimental natural and social sciences. This would involve a systematic attempt to adequately characterise and analyse the conditions of possibility of the non-experimental empirical sciences sketched in relation to evolutionary biology and Durkheim's work on suicide. I remain convinced that the outcome of such investigations would be a confirmation of the broad outlines of Roy Bhaskar's realist model, if not of some of its more detailed articulation.

In case I should be misunderstood as advocating the kind of conception of a monolithic unified science which for so long characterised logical empiricist orthodoxy, it may be necessary to point out that my arguments against Roy Bhaskar's anti-naturalism are designed less to show that the social sciences are (or could be) more like the natural ones that he supposes, than to show that for the sciences, or at least, some of them, are more like the social than he supposes. More importantly, though, I remain committed, as he does, to the view that there are significant differences in the methods of the different sciences, which are grounded in real differences in the subject matters of those sciences and the relationships of those sciences to their subject matters. Where I differ from Roy Bhaskar...
and other anti-naturalists is that I think these differences to be almost always of a methodological rather than epistemological kind, and that I do not, whereas Roy Bhaskar does, align the whole range of methodological diversity along a single fault-plane, dividing the natural and the social. Methodologically, if not epistemologically, the sciences display a 'family resemblance', of cross-cutting and overlapping differences and similarities of method.

Footnotes


2 Roy Bhaskar, The Possibility of Naturalism, op. cit., p.3.

3 See, for example, RTS, pp.30-36, where 'presuppositions' become 'conditions of possibility' on p.36. Connectedly, we get shifts from the 'intelligibility' to the 'rationality' and 'existence' of practices such as scientific perception, as if these were equivalent.

4 RTS, p.17.

5 RTS, p.20.

6 RTS, p.244.

7 RTS, p.245.

8 See, for example, my Philosophical Foundations, op. cit., p.38ff


10 PN, p.60.

11 PN, p.18.

12 PN, p.43.

13 PN, p.46 and p.65.

14 See PN, p.64ff.

15 See PN, p.64ff.

16 PN, p.48.


18 For example PN, p.50.

19 I am now somewhat sceptical as to the power of this argument, but I retain it because it seems to me to have intrinsic interest. I suspect that to see where it goes wrong, assuming it does, would be illuminating.

20 Most well known of these is, perhaps, Peter Winch's Idea of a Social Science, London, 1959.

21 PN, p.49.

22 PN, p.165.

23 See my 'Natural Science and Cultural Struggle', op. cit. (n.l).


25 p.15 above.

26 PN, p.61ff.

27 PN, p.3.

28 PN, p.3.

29 I should like to thank the organiser of the Sociology Graduate Seminar at the University of Sussex for providing the stimulus to write this paper, and the participants in that seminar for helping me to clarify and correct my ideas. I should also like to thank Andrew Sayer, whose written comments were most helpful.