Is Bhaskar's Realism Realistic?

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In this paper I begin by briefly outlining what I consider to be the key features of Roy Bhaskar's realist account of laws of nature.[1] I regard his account to be the best available. However, I do not think his case can be established as conclusively as is suggested in much of his writing. In my second section I consider the status of Bhaskar's argument, jettison an unnecessary component and stress the provisional character of what remains. In the third and longest section I criticise Bhaskar's realism for its limited scope. I indicate how it might be usefully augmented by drawing on some recent work by Nancy Cartwright and Ian Hacking. I end by indicating why I consider the limited focus of Bhaskar's realism has serious repercussions for the programme he has undertaken in his published books, and, in particular, for the extension of his realism into the social sciences.

1. Bhaskar's realism

Bhaskar bases his realist account of laws of nature on two very general features of physics and its practice. One of these concerns experimentation. When a theory is put to the test of experiment, the regularities that constitute a significant result, the mode of deflection of an electron beam in a magnetic field, the negative result of the Michelson-Morley experiment and so on, do not, in general, occur of their own accord. They occur only under the special conditions of a controlled experiment, conditions that have to be striven for in experimental activity. The second feature of the practice of physics referred to by Bhaskar is the fact that the laws supported in experimental activity are presumed to apply, and are applied, outside of experimental situations. Theories of electromagnetism are employed in the construction of radios and are assumed to apply both when the radio functions and fails to function. Further, the employment of, for example, the laws of mechanics and electromagnetism in cosmology or geophysics presupposes that those laws are, or were, applicable to the world prior to there being any humans, let alone any physics or experimental activity.

These general observations lead to the conclusion that a wide class of accounts of laws of nature, namely, those that construe them as regular or constant conjunctions of events along something like Humean lines, are inadequate. According to those accounts, laws are to be interpreted as regularities of the kind 'event of type A is invariably accompanied or followed by event of type B'. If this characterisation of laws is taken literally then laws of physics do not qualify. Autumn leaves rarely, if ever, fall to the ground in accordance with the law of fall. On the other hand, if the characterisation is qualified by asserting that the constant conjunction only occurs if some condition C is satisfied, then the application of a law is restricted to those, mainly experimental, situations where condition C is indeed satisfied and the constant conjunction obtains. It leaves the question 'What governs the world outside of experimental situations?' completely unanswered and leaves us, for example, with no explanation whatsoever of why an autumn leaf usually ends up on the ground.

Bhaskar accommodates experiment and the application of science outside of experimental situations by transcending the empiricist ontology implicit in the constant conjunctions account and invoking the generative mechanisms that lie behind and give rise to the flux of events. Generative mechanisms are depicted as the kinds of things that there are in nature and their mode of behaviour, the latter being best understood as tendencies. Fundamental laws of nature, according to this realist view, describe how the entities comprising generative mechanisms tend to behave. They describe, for example, the inertial and gravitational tendencies of masses and the tendency of charged bodies and electro-magnetic fields to behave in accordance with the Maxwell/Lorentz theory. Such tendencies do not in general lead to regularities at the level of events because they will typically be juxtaposed in complicated ways with other mechanisms. A falling leaf will be subject to inertial, gravitational, hydrodynamic, thermal and other mechanisms. It is the gravitational tendency that is responsible for the leaf falling to the ground when it does, but to identify that tendency and to specify the law governing it, it is necessary to practically intervene to remove the perturbing influence of other mechanisms. It is in this kind of way that Bhaskar's realist account of laws renders both experimental activity and the application of laws outside of experimental situations intelligible.

Bhaskar's realism certainly constitutes a decisive refutation of much orthodox philosophy of science. As we have seen, it shows the constant conjunction view of laws to be untenable. It poses problems for the hypothetico-deductive model of explanation. Events can rarely be explained by deducing them from laws together with initial conditions. At best, this can be done only for ideal experimental situations. Bhaskar's account serves to identify the shortcomings of those anti-realist positions that construe the 'surplus element' in fundamental laws, over and above mere constant conjunctions, as some model or idea supplied by us and serving to render phenomena intelligible to us. This is incompatible with the application of laws, in cosmology and geophysics for example, to the world as it existed prior to science and even before there were humans for science to be intelligible to. Bhaskar's position is realist in the strong sense that the laws of nature that we attempt to identify and formulate in science are attempted descriptions of real mechanisms assumed to be in action in the world irrespective of our knowledge of them.

2. The status of Bhaskar's argument for his realism

With reservations that will be made clear later, I regard Bhaskar's realist account of laws of nature to be the best account available. Nevertheless, I do not think his position has been established as securely as some of his own presentations suggest. In this section I spell out what I consider to be the limitations of Bhaskar's argument and the corresponding vulnerability of the realist theory that he, confidently, and I, tentatively and reservedly, embrace.

Let me first dispose of an argument which Bhaskar uses to bolster his case which is fallacious and which he does not need. I will refer to it as Bhaskar's ontological argument. It runs as follows. In an experiment an experimenter is the causal agent of a sequence of events but not of the law that that sequence enables her to identify. Consequently there is an ontological distinction between laws and sequences of events. This argument has plausiblity only if we interpret some of Bhaskar's remarks in an undesirably anti-realist way. In what sense does an experimenter 'cause' the sequence of events generated in a controlled experiment? She causes them in the sense that she assembles the appropriate experimental arrangement. But what happens when she has done so is dependent on the way the world is. The generative mechanisms at work cause the sequence of events, not the experimenter. The ontological argument works only if the experimenter is taken to be the cause of the sequence of events, as opposed to the experimental set-up, an idealist assumption quite out of keeping with Bhaskar's realism.

Let us return to the sounder aspects of Bhaskar's case and assess its status. Bhaskar asks the question 'What must the world be like for science to be possible?' and presents his realist theory as the answer. More specifically, he notes that scientific theories are tested in closed experimental situations and applied outside of those situations, shows that this is incompatible with a wide range of traditional conceptions of science, and offers a realist account of science that renders those aspects of science perfectly intelligible. It does not follow from this that the world *must* be as described by Bhaskar's theory.[2] The status of his argument is weaker than that. The possibility of rival accounts of science equally able to render experimental activity etc. intelligible cannot be ruled out. This by no means trivialises Bhaskar's position. As a matter of fact I know of no rival theory of science that can accommodate certain features of experiment as it occurs in science as well as Bhaskar's, which is why I currently accept it. Nevertheless, from this logical point of view, Bhaskar's position has not been established with the rigour that some of his remarks suggest it has.

A further dimension to the inconclusiveness of Bhaskar's position can be introduced in the following reflection. Suppose a medieval scientist, working within some version of Aristotelian theory, had asked Bhaskar's question 'what must the world be like for science to be possible?' and had attempted to answer it in a Bhaskarian way. I suggest a most plausible part of his answer would have been 'the world must be a finite, harmonious whole with a centre'. The whole of Aristotelian science, with its distinctions between forced and natural motions, terrestrial and celestial regions and so on, presupposed the absolute space provided by such a cosmos. This is the conclusion that a medieval version of Roy Bhaskar would have arrived at. We now know, of course, that that conclusion was fallacious. In retrospect we can see that Aristotelian physics did presuppose a cosmos. We can also understand why that approach would have limited success in spite of the fact that it involved false assertions and has been replaced by a more successful theory and practice. It is not difficult to imagine our current science suffering a similar fate. The practices that Bhaskar correctly identifies as important components of contemporary science may be transcended by more successful practices. In that case a future historian might acknowledge that Bhaskar had correctly identified a world view presupposed by the science of his day but one that proved to be of limited applicability and inadequately geared to coming to grips with the world as it really is.

The problem for Bhaskar that this example brings out has been indicated by Ted Benton in a review of Bhaskar's *The Possibility of Naturalism*.[3] Bhaskar asks what the world must be like for science to be possible, but it is clear that mere possibility is much too weak a notion for his purpose. After all, there is a sense in which astrology is possible. It is widely practised. But I do not think that Bhaskar would be impressed by support for a realist theory based on the compatibility of that theory with the practices of astrology. The most obvious way to strengthen Bhaskar's position is to replace 'possible' by something like 'highly successful'. There is surely some sense in which modern science is highly successful however difficult it might be to characterise that notion of success precisely. Consequently, a realist view that can make sense of basic features of modern science certainly has something strongly in its

favour. But once we raise the question 'successful compared to what?' the inconclusiveness of Bhaskar's argument becomes evident. The possibility that modern science might be replaced by something more successful cannot be ruled out, and were this to come about then his form of argument would require that his realist theory be revised. It may be the case that some features of modern quantum mechanics are already incompatible with Bhaskar's realism, although I remain to be convinced. This discussion brings out the extent to which Bhaskar's theory lacks normative force. If science should productively develop in a way that clashes with Bhaskar's realism, then so much the worse for Bhaskar's realism.

I think a useful comparison can be made between the status of Bhaskar's position, as I have construed it, and Popper's position on methodological rules.[4] Popper is, I think correctly, antipathetic to metaphysical principles such as the principle of the uniformity of nature, which some see as being presupposed by science, on the grounds that there is no adequate way of establishing or arguing for such principles. His strategy is to translate these metaphysical principles into methodological rules. In place of the principle of the uniformity of nature Popper offers a methodological rule that enjoins scientists to seek exceptionless, universal laws of nature. He recognises that following such rules is not guaranteed to lead to success, although we might well be encouraged by their past success. Popper himself does not adequately discuss the conditions for the acceptance of rejection of methodological rules, but he might well have argued for them by showing them to be implicit in the practice of modern science. Of course, the substantive account of science, offered by Popper and his methodological rules, and by Bhaskar and his transcendental realism differ markedly, and I hope I have left no doubt which I prefer. However, they are similar to the extent that they are both predicated on the practice of successful physical science and are both subject to revision should that practice change in fundamental ways. Consequently, while they both may have some normative force for normal science, they do not for extra-ordinary science (to use Kuhn's distinction).

3. Limitations of Bhaskar's realism

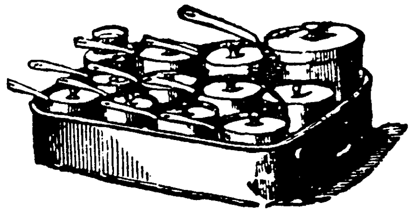
Two interesting and challenging books on realism in science that have appeared recently are Nancy Cartwright's *How the Laws of Physics Lie* and Ian Hacking's *Representing and Intervening*. In this section I exploit that material to develop what I see as some limitations of Bhaskar's analysis of physical science.

Cartwright's anti-realism has nothing in common with empiricist anti-realism which stems from a reluctance to attribute reality to unobservable 'theoretical entities'. Cartwright is thoroughly realist with respect to causes and is content to include entities such as electrons and electromagnetic fields amongst the causes of physical phenomena. The anti-realist aspects of her position are based on certain aspects of the practice of physics which, on her construal, are incompatible with a realist interpretation of fundamental laws of nature.

Cartwright distinguishes between fundamental laws of nature and what she calls phenomenological laws. Cartwright's usage of the term 'phenomenological' corresponds to that of physicists rather than philosophers. Phenomenological laws lend themselves to fairly direct rest and application, are usually mathematically formulated but offer or assume no explanation of or mechanism underlying the mathematical formalism. Examples offered by Cartwright are Airy's mathematical formulation of Faraday's magneto-optical effect, which specifies but does not explain it, the performance of lasers as specified by their manufacturers and the phenomenology of fundamental particle interactions, including such things as scattering cross-sections. Fundamental laws are general formulations which, by contrast, do offer or figure in, fundamental explanations of the phenomena of the world in general and of phenomenological laws in particular. Newton's laws, Maxwell's equations and the Schrodinger equation are amongst Cartwright's examples of fundamental laws.

Some of the reasons why Cartwright regards it as inappropriate to interpret fundamental laws realistically are as follows. She highlights the extensive use of models in physics. Metals are represented quantum mechanically as a sea of electrons in a periodic potential, laser materials are represented as a collection of two-level atoms and the solar system, in Newtonian theory, is represented as a small number of regularly shaped bodies. None of these models portray real world situations accurately. Fundamental laws are applied to models of reality rather than to reality. A related point made by Cartwright is that the formulations resulting from the application of fundamental laws to models have to be corrected before they are practically applicable to real experimental or technological situations. Thus the parameters in the theoretically derived specification of the performance of a small-signal amplifier have to be adjusted in the light of empirical measurements before a realistic description is arrived at. Using a somewhat technical example from the theory of lasers Cartwright claims that physicists, whilst they will not tolerate conflicting causal accounts of one and the same situation, are content to exploit a number of incompatible theoretical treatments involving different models. She draws on such examples to argue for a somewhat instrumentalist, Duhemian construal of fundamental laws, as opposed to her realistic treatment of causes.

Cartwright's observations about the practice of physics pose no real problems for Bhaskar's realist interpretation of fundamental laws of nature. Using Bhaskar's terminology, we can say that Cartwright is led to her anti-realist conclusions because she looks to science for a realistic description at the level of events. Once our ontology is extended in the way that Bhaskar argues to include powers, tendencies and generative mechanisms, Cartwright's case loses its force. I have shown elsewhere how Bhaskar can be defended in the face of Cartwright's arguments.[6] I will not repeat the details here. Rather, I will deal more sympathetically with Cartwright's book and try to develop her observations in a way that exposes limitations in Bhaskar's position.



Cartwright's observations about the practice of physics are not inconsistent with Bhaskar's realism with respect to fundamental laws of nature. I do not retract that claim of my earlier article. However, to argue that a position is consistent with the practice of physics is not in itself a strong commendation for that position as a useful account of physics, a point brought home to me by Wal Suchting.[7] After all, for all I know, liberation theology might be consistent with the practice of physics but it does not tell us anything useful about it. Of course, Bhaskar's realism fares better than liberation theology in this regard. As we have seen, his realism is compatible with some general features of modern science in a way that many rival accounts of science are not, and this constitutes a strong argument for it. However, there are other important aspects of science about which Bhaskar's realism has not much to say. I wish to draw on Cartwright's discussion, and later on Hacking's, to illustrate limitations of Bhaskar's position in this respect.

Let us explore further the extensive use of models in physics highlighted by Cartwright. The need for idealising models is not difficult to understand. Whilst from the point of view of Bhaskar's realism it can be maintained that fundamental laws of nature apply to the real world in an unqualified way, it can also be recognised that the behaviour of real world situations will not in general be fully explicable by or derivable from those fundamental laws. There are two reasons for this. Firstly, real situations are typically too complex for a direct application of fundamental laws to be possible. The motions of a real liquid, the excitation and decay of a molecule, even the real motions of the planets in the solar system, are too complex to be precisely characterised by fundamental laws. Secondly, the behaviour of real world situations is usually determined by a range of generative mechanisms interacting in complex and often unknown ways.

It is, of course, precisely because of the above difficulties that experimentation, as opposed to mere observation, is necessary in physical science. One can attempt to minimise the problems I have mentioned by constructing simple arrangements and shielding them from unwanted perturbations, as, for example, Cavendish did when, in his famous experiment, he shielded his attracting spheres from draughts, vibrations and magnetic effects. But I think it is a mistake to believe that all the problems referred to above can be removed in this kind of way, even in principle. Experiments involve real metals, real electrodes, real gasses and so on. The precise structure of such items is typically unknown, and in any case is not amendable to treatment by direct application of fundamental laws.

The history of solid state physics provides a useful example to illustrate my point. From the 1920s onwards solid state physics emerged as a major branch of physics. The main aim was to explain the properties of solids in quantum-mechanical terms. A major component of that effort involved the devising of appropriate *models* of metals, crystals and so on, which, on the one hand, were amenable to theoretical treatment and, on the other, were able to replicate some of the properties of real metals, crystals etc. The early treatment of metals, for example, involved a lattice model and a free electron model, with the latter eventually proving more productive. In 1946, N. F. Mott, one of the most distinguished solid state physicists, summed up his approach to the subject thus:

> The trouble is that for even the simplest metals the problem of a large number of electrons interacting together is so forbiddingly complicated that at present one can solve it only by making a number of simplifying assumptions, and it is seldom certain that these are valid. What we can do is to take a simple model of a phenomenon and to work out its consequences, and to see how much of the observed facts can be fitted in to the resulting framework.[8]

Roughly speaking, then, in this major branch of physics the fundamental laws of nature are taken for granted. The main effort is directed towards the construction of models of real world situations whose behaviour is assumed to be governed by the fundamental laws, and which can replicate, more or less, some of the behaviour of real solids. This picture corresponds quite well with Cartwright's simulacrum account of science. Bhaskar says little about such matters and his realism needs to be augmented in a major way to accommodate them.

Cartwright draws anti-realist conclusions from her simulacrum account. I do think there are some weak anti-realist conclusions to be drawn here but they are in direct opposition to Cartwright's. Cartwright maintains that the use of models in science is one reason why a realist interpretation of fundamental laws is inappropriate. I maintain that, once we move from what Cartwright embraces under the name of actualism, which, in Bhaskar's terminology, limits us to an ontology of events, to a richer ontology involving powers, tendencies and generative mechanisms, then realism with respect to fundamental laws can be defended in the face of Cartwright's case. However, there is room for an element of anti-realism when we come to consider the relation between models and the real world. Whilst, as Ernst McMullin has nicely illustrated,[9] models can be systematically 'de-idealised', so that they come closer to an accurate description of reality, we never, or very rarely, reach a stage where the gap between description of model and true description of reality is closed. Let us consider, for example, the Wiedemann-Franz law, which relates the thermal and electrical conductivity of metals, a candidate for what Cartwright calls a phenomenological law. Bhaskar, illustrating his realist stance with respect to scientific knowledge, says of the law that, in a world without science it 'would continue to hold although there would be no-one to formulate, experimentally establish or deduce it.'[10] By contrast, I doubt that any real metal conforms or ever did conform precisely to the structure of the model for which, given the truth of fundamental laws, the Wiedemann-Franz law holds. Contrary to Cartwright, whilst fundamental laws may be true of reality, it is the phenomenological laws that lie.

A further aspect of Cartwright's book that can be exploited to highlight the limits of Bhaskar's realism is her focus on the practical, technological aspects of science, the ways in which scientists and engineers come to grips with the behaviour of real liquids, real amplifiers, real lasers and so on. This is also the focus of the second half of Hacking's book. In what immediately follows I draw heavily on the latter source.

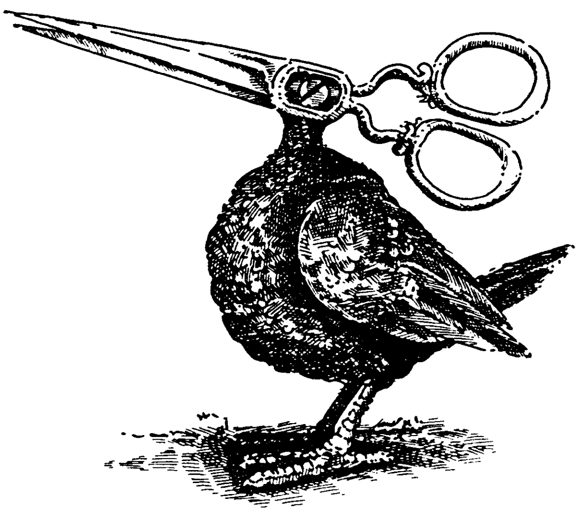
The role of experiment as it is construed by mainstream philosophy of science is to test theories. This view is clearly conveyed in the following quotation from Popper's *The Logic of Scientific Discovery*.

> The theoretician puts certain definite questions to the experimenter and the latter, by his experiments, tries to elicit a decisive answer to their questions, and to no others. All other questions he tries hard to exclude.... But it is a mistake to suppose that the experimenter proceeds in this way 'in order to lighten the task of the theoretician', or perhaps, in order to furnish the theoretician with a basis for inductive generalisations.

On the contrary, the theoretician must long before have done his work, or at least what is the most important part of his work; he must have formulated questions as sharply as possible. Thus it is he who shows the experimenter the way.... Theory dominates the experimental work from its initial planning up to the finishing touches in the laboratory.[11]

It is undoubtedly the case that these words describe a vital role of experiment in science, and it is this role that is well accommodated and skilfully exploited by Bhaskar. But there are other dimensions to experimentation within science that Hacking stresses which, whilst not inconsistent with Bhaskar's realism, illustrate limitations of it, and perhaps suggest that it is unduly theoreticist.

Much experiment in science involves what Bacon referred to as 'twisting the lions tail', practically intervening with nature to see how it will behave under unusual circumstances, often in response to practically or technologically posed problems and not guided by an explicit theory. Into this category come, for example, many properties of light discovered and exploited prior to their explanation by the wave theory, double refraction, and the phenomenon of polarisation, together with many of the laws governing that phenomenon discovered by Brewster. Brewster would have none of the wave theory of light. As Hacking puts it, 'Brewster was not testing or comparing theories at all. He was trying to find out how light behaves.'[12] In a similar vein one could mention the conduction properties of metals, well known before their quantum mechanical explanation, or knowledge of emission spectra prior to their explanation. One of the most telling examples of practically efficacious, through not theoretical or fundamental, scientific knowledge is the series of practical inventions culminating in the steam engine and the Industrial Revolution. This practical knowledge owed little or nothing to knowledge of fundamental laws of nature. As Hacking puts it, experiment sometimes has a life of its own.



Now, of course, there is a straightforward response that mainstream philosophers, and Roy Bhaskar, can make to these observations. Phenomenological laws, practical inventions and the like can be accepted as some low level, lesser kind of, scientific knowledge, but it can be insisted that they reach the level of genuine scientific knowledge only when they have been explained by a deeper theory involving fundamental laws of nature. Such a view could be solidly backed by pointing to the increased power and range of practical, phenomenological knowledge made possible by the explanatory theory. The improved design and greater range of engines made possible once the steam engine was understood in terms of the laws of thermodynamics would provide an obvious example to support such a case.

I do not wish to dispute that there is much that is correct about that response. However, undue emphasis on the theoretical mode leads to a failure to capture the character of and the vital role played in science by practically efficacious knowledge at the experimental, technological level. Whilst it is true that fundamental theory often shows the way towards practically efficacious knowledge, as with the discovery of radio waves, for example, it is also the case that that order is frequently reversed, as the examples drawn from Hacking illustrate. What is more, knowledge at the practical, phenomenological level constitutes an important part of the preconditions for the emergence of fundamental theory. Theories do not come out of the air, or straight out of the minds of geniuses, as Bhaskar's discussion of the social production of knowledge makes clear. Whether it is more appropriate, at some particular conjuncture within a science, to pursue a theoretical quest for fundamental laws or to employ a more practically orientated strategy will depend on the contingencies of the situation. 'Seek fundamental laws characterising the generative mechanisms of nature' would have been poor advice for the pioneers of the industrial revolution. There was a period of a quarter of a century (1880-1905) in which phenomenological thermodynamics was more productive than the kinetic theory. A search for underlying mechanisms to explain the behaviour of gasses was eventually productive. Corresponding efforts to discover mechanical properties of an aether to explain electro-magnetic fields proved to be futile. The view of science emphasised in Bhaskar's realism, a search for fundamental laws characterising the generative mechanisms of nature, whilst sometimes appropriate, often misses the mark. What is more, if his portrayal of science were used as a basis for advising scientists it would often yield bad advice.

Another aspect of Hacking's analysis of science is worthy of mention before I pursue this line of criticism a little further. Hacking, by focussing his attention on what is *done* in experimental science is able to develop a robust brand of realism that differs from Bhaskar's. He argues for the reality of electrons and the entities observed through high-powered microscopes, for example, in a low-level way that focusses on the practical procedures involved. He argues, convincingly, that experiments that involve the spraying of electrons in a controlled way to produce causal effects in targets presuppose the existence of electrons whilst he argues, equally convincingly, that strong evidence for the reality of microscopic structures is provided when a number of microscopes utilising quite different physical principles reveal identical structures. Hacking wishes to confine his realism to a realism with respect to theoretical entities, defending his position with reference to experimental activity using examples like the ones I have mentioned. He is reluctant to interpret fundamental laws realistically, and signals his agreement with Cartwright in that regard. I can find nothing in Hacking's book that provides a convincing reason to abandon Bhaskar's realist interpretation of fundamental laws of nature. However, I suggest that the addition to Bhaskar's realism of some of Hacking's contributions would enrich the former and give it a healthy, down to earth, practical dimensions which it might be said to lack.

Perhaps this account of the limitations of Bhaskar's realism could be summed up thus. Whilst Bhaskar has given a satisfactory realist account of fundamental laws of nature, there is much more to science than establishing such laws.

4. Implications for Bhaskar's programme

My case for there being serious omissions in Bhaskar's realist account of science does not challenge his realist rendering of laws of nature. My considerations concerning models, drawing on Cartwright, and those concerning experimentation and theoretical entities, drawing on Hacking, all point to ways in which Bhaskar's theory can be usefully augmented, rather than to ways in which it is incorrect. This suggests a way of construing my case in a way that is charitable to Bhaskar. He has given us a realist account of fundamental laws of nature that is superior to others and free from telling objections. If his account can be usefully augmented by adding to it characterisations of other aspects of science drawn from Cartwright, Hacking or whomever, all well and good. One person cannot be expected to do everything.

Given what I understand to be the nature of Bhaskar's project, I am not sure that such a charitable reading of my critique is appropriate. Bhaskar uses his realist account of science as a starting point for proceeding to an analysis of the human sciences and thence to general considerations concerning the nature of ideology and possible routes to human emancipation. If my assessment of the partial character of his analysis of physical science is correct, and he does emphasise the theoretical as opposed to more practically orientated modes, then there is the danger that this emphasis will persist through the remainder of his programme. Indeed, I believe that it does. I believe that his passage into the social and political domain takes place at a level of theory that his arguments are unable to sustain and removes him too far from the domain of practically efficacious intervention.

My criticism of Bhaskar's realism can be put with a slightly different emphasis which will help point to a further difficulty with his progression to the social sciences. Bhaskar takes certain features of scientific practice as the starting point for his 'transcendental deduction' of a realist theory of natural science.[13] But his choice of features is selective. His view of realism does justice to those features of science that he selects, but not to other, equally important, features of contemporary science. The general problem raised here is the all-important one of the criteria to be employed in deciding what is to be included in the premises of the argument.

Whilst in the physical sciences, there may be a problem concerning which aspects of science to focus on, at least there is an uncontentiously successful science to turn to. When we turn to the social domain, there is no uncontentious social science whose practices we can draw on for premises for an argument for realism. This difficulty is acknowledged by Bhaskar.[14] His response is to attempt to isolate 'more or less universally recognised features of substantive social life itself, which do not beg the issue at the outset in favour of one type of social science rather than another'.[15] The problem here goes beyond the serious one of how to select amongst possible candidates for such 'universally recognised features'. The very fact that some features of social life appear uncontentious and are universally accepted is strong evidence that they are merely pre-scientific reflections of the ideology of the day. Bhaskar himself makes the point, in another context, when talking of the birth of a science.

> Probably the most significant type of event in the history of any science is that in which it comes to define – or rather redefine – its object of enquiry... Typically this process will necessitate some scientists breaking free ... of the 'tissue of tenacious truisms' currently congealed in their field.[16]

It would seem that Bhaskar recognises the danger that his account of realism in the social sciences will turn out to be based on a tissue of tenacious truisms. In actual fact, the features of humans and society that he does select as the basis for his theory are such 'as to make it possible to pull a version of historical materialism out of the philosophical hat' as an antipodean group of reviewers unkindly putit.[17] Whilst I am sympathetic to the general drift of many of the conclusions Bhaskar arrives at in this way, I believe that his attempt to establish them by philosophical argument is flawed. I do not see how philosophy can be seen as having the resources to arrive at such substantive conclusions. In my view, philosophy can play an important critical role, exposing false philosophical and ideological intrusions into science and social and political life and I regard Bhaskar's realist account of physical science as providing a useful weapon in that struggle. However, when it comes to a positive role for philosophy, I regard Bhaskar's project as misconceived, and believe we must recognise the priority of substantive science and politics over philosophy. When looking for positive guidance from philosophy we must rest content with some vague generalisations about the need to be specific.

Notes

This is the text of a paper read at a conference on 'Realism in the Human Sciences' at the University of Sussex in September 1987.

1 Roy Bhaskar, *A Realist Theory of Science*, Sussex, Harvester, 1980.

2 *Op. cit.*, p. 27.

3 Roy Bhaskar, *The Possibility of Naturalism*, Sussex, Harvester, 1979, reviewed by Ted Benton, 'Realism and social theory', *Radical Philosophy* 27, 1981, pp. 13-21.

4 K. R. Popper, *The Logic of Scientific Discovery*, London, Hutchinson, 1968, p. 253.

5 *A Realist Theory of Science*, pp. 217-18, fails to grasp this aspect of Popper's position.

6 Alan Chalmers, 'Bhaskar, Cartwright and Realism in Physics', *Methodology and Science 20*, 1987, pp. 77-96.

7 W. Suchting, 'Realism in physics: Comments on a paper by Alan Chalmers', forthcoming in *Methodology and Science*.

8 N. F. Mott, 'Atomic physics and the strength of materials', *Journal of the Institute of Metals*, 72, 1946, p. 371. I owe this source to S. T. Leith and Paul K. Hoch, 'Formation of a research school: Theoretical physics at Bristol 1930-54', *British Journal for the History of Science*, 19, 1986, pp. 19-44.

9 E. Mullin, 'Galilean idealization', *Studies in History and Philosophy of Science*, 16, 1985, pp. 247-73.

10 *A Realist Theory of Science*, p. 22.

11 *The Logic of Scientific Discovery*, p. 107.

12 I. Hacking, *Representing and Intervening*, Cambridge, Cambridge University Press, 1983, p. 157.

13 Of course, the considerations I raise in section 2 indicate that the term 'deduction' is much too strong here.

14 *The Possibility of Naturalism*, p. 17.

15 *Ibid.*, p. 18.

16 Roy Bhaskar, *Scientific Realism and Human Emancipation*, New Left Books, London, 1986, p. 104.

17 R. Albury, G. Payne and W. Suchting, 'Naturalism and the Human Sciences', *Economy and Society*, 10, 1981, pp. 367-79.