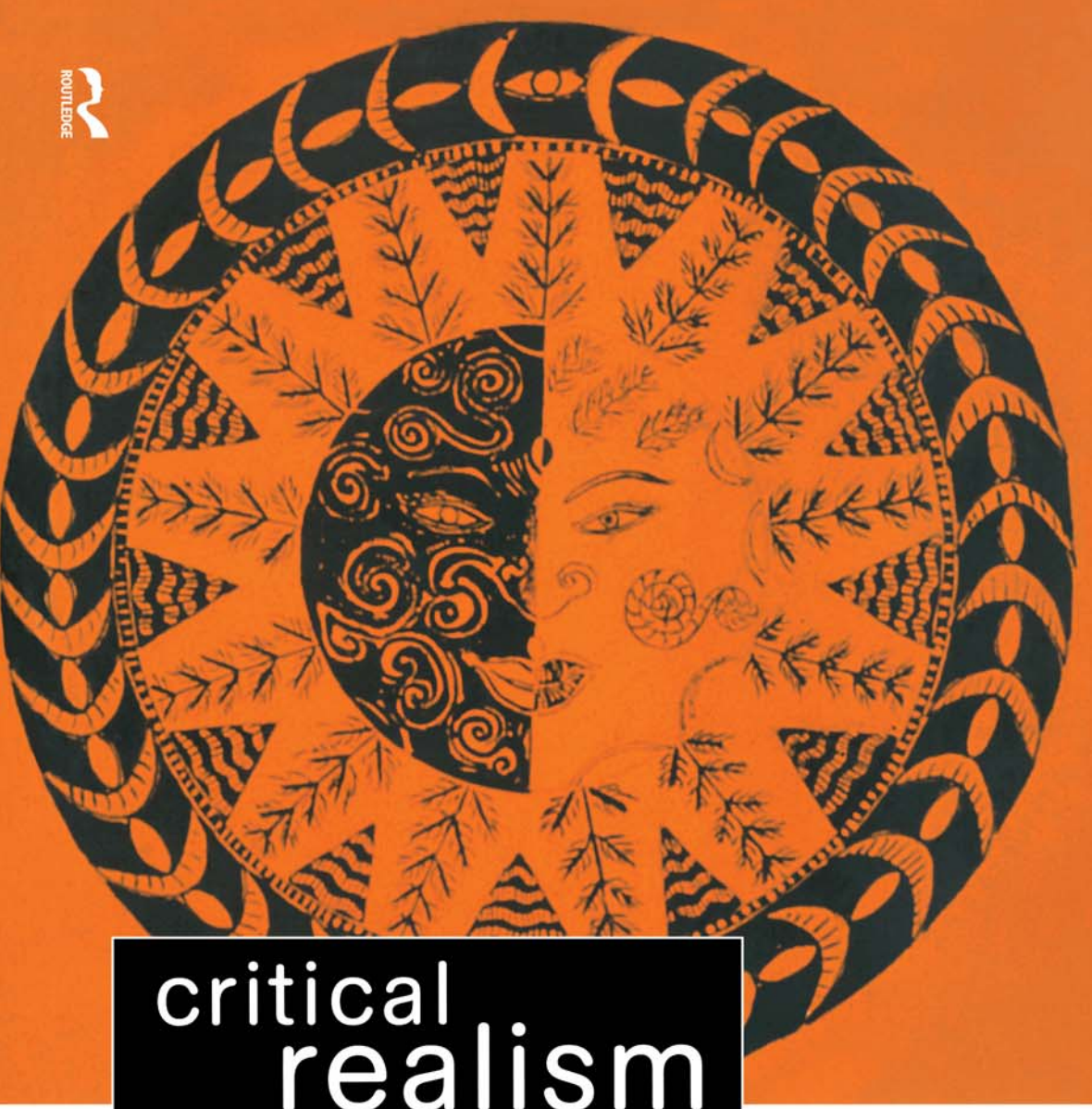

critical realism

Essential Readings

Edited by
Margaret Archer, Roy Bhaskar,
Andrew Collier, Tony Lawson
and Alan Norrie

Centre for Critical Realism

Critical Realism: Interventions

CRITICAL REALISM

Since the publication of Roy Bhaskar's *A Realist Theory of Science* in 1975, critical realism has emerged as one of the most powerful new directions in the philosophy of science and social science, offering a real alternative to both positivism and post modernism. This reader is designed to make accessible in one volume, to lay person and academic, student and teacher alike, key readings to stimulate debate about and within critical realism.

The four parts of the reader correspond to four parts of the writings of Roy Bhaskar:

- part one explores the transcendental realist philosophy of science elaborated in *A Realist Theory of Science*
- the second section examines Bhaskar's critical naturalist philosophy of social science
- part three is devoted to the theory of explanatory critique, which is central to critical realism
- the final part is devoted to the theme of dialectic, which is central to Bhaskar's most recent writings

The volume includes extracts from Bhaskar's most important books, as well as selections from all of the other most important contributors to the critical realist programme. The volume also includes both a general introduction and original introductions to each section.

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GENERAL INTRODUCTION

Critical realism is a movement in philosophy and the human sciences and cognate practices most closely associated with – in the sense of identified with or emanating from – though by no means restricted to – the work of Roy Bhaskar. This movement is now fully international and multidisciplinary and arguably in the quarter century since the initial publication of *A Realist Theory of Science* (1975) has transformed the intellectual scene. At least, at the turn of the millennium it presents an intellectual challenge to other philosophies that they can scarcely refuse. This reader is designed to make accessible, in one volume, to layperson and academic, student and teacher alike, key readings to stimulate debate about and within critical realism.[1]

The four parts of the reader correspond to four parts of the writings of Roy Bhaskar: section one to his *transcendental realist* philosophy of science, elaborated in RTS (1975, 1978) and subsequently augmented and refined; section two to his *critical naturalist* philosophy of social science, first systematically presented in PON (1979, 1989, 1998) and likewise developed; section three to the theory of *explanatory critique* implicit in PON, elaborated in a number of articles published in the early 1980s (see e.g. RR Chapter 6) and most fully in SRHE (1986); and section four to the theme of *dialectic* on which Bhaskar had published since the early 1980s but only fully developed in DPF (1993) (and PE (1994)). Extracts from all four canonical books are included below. These theories did not appear in an intellectual vacuum and this introduction will say something about the context in which they arose as well as their principal features.

The term 'critical realism' arose by elision of the phrases 'transcendental realism' and 'critical naturalism', but Bhaskar and others in this movement have accepted it since 'critical', like 'transcendental', suggested affinities with Kant's philosophy, while 'realism' indicated the differences from it. It should be noted that the principal themes of each section both presuppose and develop the themes of the sections preceding it, so that there is a definite ordination to 'critical realism'. Thus Bhaskar refers to the philosophy espoused in DPF and PE as 'dialectical critical realism' and this does radically refine and

rework the theories of science, social science and ethics presented earlier; as he has indicated they will be further recast in his turn to the third (totalizing) and fourth (reflexive or transformatively practical) moments of his dialectical philosophy. Something will now be said about the context and content of the theories of transcendental realism, critical naturalism, explanatory critique and dialectic sequentially by way of general introduction to the readings excerpted below.

Transcendental realism

Transcendental realism was born in the context of vigorous critical activity oriented against the positivist conception of science that had dominated the first two-thirds of the twentieth century. This was based squarely on Humean empiricism, epitomized in the claim of Mach[2] that 'natural laws were nothing but the mimetic reproduction of facts in thought, the object of which is to replace and save the trouble of new experience'. It is perhaps most familiar to us retrospectively in the guise of the logical positivism of the Vienna circle of the 1920s and 1930s which married the epistemological empiricism and reductionism of Mach, Pearson and Duhem with the logical innovations of Frege, Russell and Wittgenstein. The positivist vision of science pivoted on a *monistic* theory of scientific development and a *deductivist* theory of scientific structure. The attack on the former came from three main sources. First, from Popper and (ex-) Popperians like Lakatos and Feyerabend who argued that it was falsifiability, not verifiability, that was the hallmark of science and that it was precisely in revolutionary breakthroughs such as those associated with Galileo or Einstein that its epistemological significance lay. Second, from Kuhn and other historians and sociologists of science who drew scrupulous attention to the real social processes involved in the reproduction and transformation of scientific knowledge in what critical realism called the transitive (epistemological and geo-historical-social) dimension of science. Finally, from Wittgensteinians such as Hanson, Toulmin and Sellars who latched on to the non-atomistic or theory-dependent and mutable character of facts in science.

A problem for all these trends was to sustain a clear concept of the continued independent *reality* of *being* – of the intransitive or ontological dimension – in the face of the *relativity* of our *knowledge* – in the transitive or epistemological dimension. This arose particularly acutely in the debate about the possibility and, according to Kuhn and Feyerabend, the actuality of meaning variance as well as inconsistency in scientific change. Kuhn and Feyerabend suggested that it may come to pass that no meaning was shared in common between a theory and its successor. This seemed to render problematic the idea of a rational choice between such 'incommensurable' theories and even encouraged (superidealist) scepticism about the existence of a theory-independent world. However, if the relation between the theories is

one of conflict rather than merely difference, this presupposes that they are alternative accounts of the *same* world, and if one theory can explain more significant phenomena in terms of its descriptions than the other can in terms of *its*, then there is a rational criterion for theory choice, and *a fortiori* a positive sense to the idea of scientific development over time (cf. RTS, p. 248). In this sort of way critical realism claims to be able to combine and reconcile *ontological realism*, *epistemological relativism* and *judgmental rationality*.

The deductivist theory of structure initially came under fire from, among others, Michael Scriven, Mary Hesse and Rom Harré for the lack of *sufficiency* of Humean criteria for causality and law, Hempelian criteria for explanation and Nagelian criteria for the reduction of one science to another more basic one. This critique was then generalized by Roy Bhaskar to incorporate the lack of *necessity* for them also. Bhaskar argued that positivism could sustain neither the necessity nor the universality – and in particular the transfactuality (in open and closed systems alike) – of laws; and for an ontology (1) that was irreducible to epistemology; (2) that did not identify the domains of the real, the actual and the empirical; and (3) that was both stratified, allowing emergence, and differentiated. That is, in effect for three kinds of *ontological depth* which may be summarized by the concepts of *intransitivity*, *transfactuality* and *stratification*.

The lynchpin of deductivism was the Popper-Hempel theory of explanation, according to which explanation proceeded by deductive subsumption under universal laws (interpreted as empirical regularities). Its critics pointed out, however, that deductive subsumption typically does not explain but merely generalizes the problem (for instance, from 'why does x θ?' to 'why do all x's θ?'). Instead what is required for a genuine explanation is, as Whewell had inveighed against Mill in the 1850s and Campbell against Mill's latter-day successors in the 1920s, the introduction of *new* concepts not already contained in the explanandum, models, picturing plausible generative mechanisms, and the like. But the new realism broke with Campbell's Kantianism by allowing that, under some conditions, these concepts or models could describe newly identified and deeper or subtler or otherwise more recondite levels of reality. Theoretical entities and processes, initially imaginatively posited as plausible explanations of observed phenomena, could come to be established as real, through the construction either of sense-extending equipment or of instruments capable of detecting the effects of the phenomena. (In the latter case we invoke a *causal* criterion for attributing reality: *esse* no longer *est percipi*.) All this strongly suggests a *vertical* or theoretical realism. Science could now be seen as a continuing and reiterated process of movement from manifest phenomena, through creative modelling and experimentation or other empirical controls, to the identification of their generative causes, which now become the new phenomena to be explained. The stratification of nature imposes a certain dynamic logic to scientific

discovery, in which progressively deeper knowledge of natural *necessity a posteriori* is uncovered.

However critical or transcendental realism argued that a *horizontal* or transfactual realism was additionally necessary to sustain the *universality* (within their range) of the workings of generative mechanisms or laws. Thus it is a condition of the intelligibility of experimentation that the laws which science identifies under experimental or analogously closed conditions continue to hold (but transfactually, not as empirical regularities) extra-experimentally. And this provides the rationale or ground for practical and applied explanatory, diagnostic, exploratory, scientific work too. Indeed the whole point of an experiment is to identify a universal (within its range) law, which, by virtue of the necessity for the experiment, is not actually, or even less empirically, so. Laws, then, and the workings of nature have to be analysed dispositionally as the powers, or more precisely tendencies, of underlying generative mechanisms which may on the one hand – the horizontal aspect – be possessed unexercised, exercised unactualized, and actualized undetected or unperceived; and on the other – the vertical aspect – be discovered in an ongoing irreducibly empirical open-ended process of scientific development.

A transcendental argument from the conditions of the possibility of experimentation in science thus establishes at once the irreducibility of ontology, of the theory of being, to epistemology and a novel non-empiricist but non-rationalist, non-actualist, stratified and differentiated ontology, that is characterized by the prevalence of structures as well as events (stratification) and open systems as well as closed (differentiation).

Thus let us revert to the three kinds of depth in transcendental realism:

(1) *Intransitivity*. The Western philosophical tradition has mistakenly and anthropocentrically reduced the question of what is to the question of what we can know. This is the '*epistemic fallacy*' (cf. RTS, p. 36), epitomized by concepts like the 'empirical world'. Science is a social product, but the mechanisms it identifies operate prior to and independently of their discovery (existential intransitivity). Transitive and intransitive dimensions must be distinguished. Failure to do so results in the reification of the fallible social products of science. Of course being contains, but it is irreducible to, knowledge, experience or any other human attribute or product. The domain of the real is distinct from and greater than the domain of the empirical.

(2) *Transfactuality*. The laws of nature operate independently of the closure or otherwise of the systems in which they occur, and the domain of the real is distinct from and greater than the domain of the actual (and hence the empirical too). Failure to appreciate this results in the fallacy of *actualism*, collapsing and homogenizing reality. Once the ubiquity of open systems and the necessity for experimentation or analogous procedures are appreciated, then laws must be analysed as transfactual, as universal (within their range) but neither actual nor empirical. Constant conjunctions are produced not

found. Laws operate independently of both the conditions for and their identification. *Theoretical explanations* for their part explain laws in terms of the structures which account for or perhaps merely ground them, while they are applied transfactually in the *practical* explanation of the phenomena they co-produce in open systems.

(3) There is *stratification* both in nature, and reflecting it in science, and both (a) within a single science or subject matter and (b) between a series of them.

(a) Recognition of the stratification of nature and the isolation of a concept of natural necessity discernible *a posteriori* allows the resolution of a whole host of philosophical problems, most notoriously the problem of induction, the untheorized or tacit condition of possibility of which is actualism. Thus if there is a real reason, located in its molecular or atomic constitution, why water boils rather than freezes when it is heated, then it *must* do so (cf. RTS, chapter 3.5–3.6).

(b) The real multiplicity of natural mechanisms grounds a real plurality of sciences which study them. Even though one kind of mechanism may be explained or grounded in terms of another, it cannot necessarily be reduced to or explained away in terms of it. Such grounding is consistent with its *emergence* so that the course of nature is different than it would have been if the more basic stratum alone operated; so that, to invoke our causal criterion for reality, the higher-order structure is real and worthy of scientific investigation in its own right.

This takes us neatly to the domain of the social sciences, where what Outhwaite has called the 'law-explanation' orthodoxy[3] was never even remotely plausible.[4]

Critical naturalism

For most of its recognized history, the philosophy of the human sciences has been dominated by dichotomies and dualisms. It was the aim of *The Possibility of Naturalism* to transcend them. (1) The overriding dichotomy or split was between a *hyper-naturalistic positivism* and an *anti-naturalistic hermeneutics*, resolved in the generation of a *qualified critical naturalism*. I discuss this in detail immediately below. (2) Then there was the split between individualism and collectivism (or holism), which critical naturalism would resolve by seeing society *relationally* and as *emergent*. (3) A connected split, upon which the debate about structure and agency was joined, was between the *voluntarism* associated with the Weberian tradition and the *reification* associated with the Durkheimian one. This critical naturalism would transcend in its *transformational model of social activity*. (4) Then there was the dichotomy between facts and values, most sharply expressed in Hume's law (discussed in the next section), which critical naturalism would refute in its theory of *explanatory critiques*. (5) Then, fuelling the positivism/hermeneutics debate, was the

dichotomy between *reasons* and *causes*, which critical naturalism would resolve by showing how, once one rejected Humean causality, reasons could be causes *sui generis* on a critical realist conception of causality. (6) Finally underpinning many of these dichotomies was the dualism between *mind* and *body* (or, more macroscopically, between society and nature), which critical naturalism would overcome, by seeing mind as an emergent power of matter in its *synchronic emergent powers materialism*.

The Possibility of Naturalism, first published in 1979, was oriented primarily to the first of these questions, which was whether society, and human phenomena generally, could be studied in the same way as nature, i.e., 'scientifically'. There were two leading positions. (1) A more or less unqualified naturalism, which asserted that they could, which normally took the form of *positivism*, dominant in the philosophy and practice of the social sciences. Its immediate philosophical antecedents lay in the work of Hume, Mill, Mach and the Vienna Circle, providing the spine of the orthodox conception of science which it transplanted to the social world. (2) An anti-naturalism, based on a distinctive conception of the uniqueness of the social realm, that is as pre-interpreted, conceptualized or linguistic in character – *hermeneutics*, the official opposition to positivism. Its philosophical ancestry came from Dilthey, Simmel, Rickert and Weber who fused Hegelian and Kantian dichotomies to produce a contrast between the phenomenal world of nature and the intelligible world of freedom so as to ground dichotomies between causal explanation and interpretive understanding, the nomothetic and ideographic, the repeatable and the unique, the realms of physics and of history. If positivism found expression in the Durkheimian sociological tradition and in behaviourism, structuralism and functionalism, hermeneutics did so in aspects of the Weberian tradition and in phenomenological, ethnomethodological and interpretive studies. A discrimination must be made within the second camp between those who sought to synthesize or combine positivist and hermeneutical principles such as Weber and Habermas, and those dualists, such as Gadamer or Winch, who denied positivism any purchase in the human sphere. (It should be noted in passing that it is less easy to characterize the work of post-structuralist or, more generally, post-modernist thinkers. For the most part they adopt a Nietzschean epistemological perspectivism on a Humean or positivist ontological base.)

Now both positivist and hermeneuticist views, that is the standard naturalist and anti-naturalist positions, shared an essentially positivist account of natural science. If this is, as critical realists argue, *false*, then the possibility arises of a third position: (3) a qualified, *critical* and non-reductionist, *naturalism*, based upon a transcendental realist account of science and, as such, necessarily respecting (indeed grounded in) the specificity and emergent properties of the social realm. Moreover if the positivist account of natural science is false, then positivists have to make out a special case as to why positivism should be uniquely (and most implausibly) applicable to the

human realm; and hermeneuticists, for their part, have to reassess their contrasts. Thus both of Winch's two main arguments in his very influential *The Idea of a Social Science* (1959) are parasitic on a positivist ontology. Constant conjunctions of events are neither necessary nor sufficient either for natural or for social scientific understanding: both alike are concerned with the discovery of intelligible connections in their subject matter. Nor do the conceptual and the empirical jointly exhaust the real. Critical realism can allow that conceptuality is distinctive, without supposing that it is exhaustive, of social life.

Let me elaborate on this. The social world is characterized by the complete absence of laws and explanations conforming to the positivist canon. In response to this positivists plead that the social world is much more complex than the natural world or that the laws that govern it can only be identified at some more basic, e.g. neurophysiological, level. But positivists are wrong to expect the social sciences to find constant conjunctions in the human world, for they are scarce enough in the natural; while hermeneuticists are wrong to conclude from the absence of such conjunctions that the human sciences are radically unlike the natural sciences. Closed systems cannot be artificially established in the human sciences. But, as Tony Lawson has shown in his contributions to Part I, this does not mean that the identification of epistemically significant non-random patterns or results cannot provide the empirical controls and contrasts that experimentation plays in physics and chemistry. Moreover the fact that social life is pre-interpreted provides a ready-made starting point for the social sciences. But there are no grounds for treating these data as exhaustive of the subject matter of social science, as incorrigible or their operation as non-causal. Thus rejecting Humean causality and acknowledging emergence allows us to see reasons as causes, but causes which may, for instance, be rationalizations.

Thus the hermeneutical position is often buttressed by the argument that the human sciences are concerned with the reasons for agents' behaviour and that such reasons cannot be analysed as causes. For, first, reasons are not logically independent of the behaviour they explain. Moreover, second, they operate at a different language level (Waismann) or belong to a different language-game (Wittgenstein) from causes. But natural events can likewise be redescribed in terms of their causes (for instance, toast as burnt). Furthermore, unless reasons were causally efficacious in producing one rather than another sequence of bodily movements, sounds or marks, it is difficult to see how there can be grounds for preferring one reason explanation to another, and indeed eventually the whole practice of giving reason explanations must come to appear as without rationale.

The positive case for critical naturalism turns on the extent to which an independent analysis of the objects of social and psychological knowledge is consistent with the transcendental realist theory of science. Thus whereas on the Weberian tradition social objects are seen as a result of, or constituted by

intentional or meaningful human behaviour, tending to voluntarism, and on the Durkheimian tradition social objects are seen as possessing a life of their own, external to and coercing the individual, tending to reification, the critical realist conception stresses that society is both (a) a pre-existing and (transcendentally and causally) necessary condition for intentional agency (Durkheim's insight) but equally (b) as existing and persisting only in virtue of it. On this conception, then, society is both the condition and outcome of human agency and human agency both reproduces and transforms society. However there is an important asymmetry here: at any moment of time society is pre-given for the individuals who never create it, but merely reproduce or transform it. The social world is always pre-structured. This is a major difference between Bhaskar's transformational model of social activity and Giddens's theory of structuration which Margaret Archer highlights in Part 2. It means that agents are always acting in a world of structural constraints and possibilities that they did not produce. Social structure, then, is both the ever-present condition and the continually reproduced outcome of intentional human agency. Thus people do not marry to reproduce the nuclear family or work to sustain the capitalist economy. Yet it is the unintended consequence (and inexorable result) of, as it is the necessary condition for, their activity.

On this conception, in contrast to the hermeneutical perspective, then, actors' accounts are both corrigible and limited by the existence of unacknowledged conditions, unintended consequences, tacit skills and unconscious motivations; but in opposition to the positivist view, actors' accounts form the indispensable starting point of social enquiry. The transformational model of social activity entails that social life possesses a recursive and non-teleological character, as agents reproduce and transform the very structures which they utilize (and are constrained by) in their substantive activities. It also indicates a relational conception of the subject matter of social science, in contrast to the methodological individualist and collectivist conceptions characteristic of the utilitarian (and Weberian) and Durkheimian traditions of social thought. Related to this is the controversy about ideal types. For critical realists the grounds for abstraction lie in the real stratification (and ontological depth) of nature and society. They are not subjective classifications of an undifferentiated empirical reality, but attempts to grasp (for example, in real definitions of forms of social life already understood in a pre-scientific way) precisely the generative mechanisms and causal structures which account in all their complex and multiple determinations for the concrete phenomena of human history. Closely connected with this is a reassessment of Marx as, at least in *Capital*, a scientific realist – contrary to pre-existing marxist and non-marxist interpretations. In its wake too is a reassessment of other founding figures in the social sciences (such as Durkheim and Weber) as combining aspects of a realist and some or other non-realist method and ontology.

Certain emergent features of social systems which, on the invocation of a causal criterion for ascribing reality, can be regarded as *ontological* limits on naturalism, are immediately derivable from the transformational model of social activity. These may be summarized as the concept-dependence, activity-dependence and greater space–time specificity of social structures. The causal interdependency between social science and its subject matter specifies a *relational* limit; while the condition that social systems are intrinsically open – the most important *epistemological* limit – accounts for the absence of crucial or decisive test situations in principle, necessitating reliance on exclusively explanatory (not predictive) criteria for the rational assessment of theories. (A fourth *critical* limit will be discussed in the next section.) However subject to (and, arguably, just in virtue of) these qualifications both the characteristic modalities of theoretical and applied explanation which critical realists specify appear possible in the social, just as in the natural sphere. Thus theoretical explanation proceeds by *d*escription of significant features, *r*etroduction to possible causes, *e*limination of alternatives and *i*dentification of the generative mechanism or causal structure at work (which now becomes a new phenomenon to explain) (DREI); applied explanation by *r*esolution of a complex event (etc.) into its components, theoretical *r*edescription of these components, *r*etrodiction to possible antecedents of the components and *e*limination of alternative causes (RRRE).

On critical naturalism, then, the social sciences can be 'sciences' in exactly the same sense as natural ones, but in ways which are as different (and specific) as their objects. If the hermeneutical starting point of social science, in some pre-conceptualized social practice, lends to them a closer affinity with the transcendental and dialectical methods characteristic of philosophy, any slight on a *critical* naturalism is dissolved by reflection on the fact that these forms of argument are merely a species of the wider genus of retroductive ones familiar to all the sciences.

Explanatory critiques

The Possibility of Naturalism had identified a fourth *critical* difference between the social and natural sciences, necessitated by the consideration that the subject matter of social science includes not just social objects but beliefs about those social objects (or put another way that social objects include beliefs about themselves), making possible an *explanatory critique* of consciousness (and being), entailing judgements of value and action without parallel in the domain of the natural sciences, so vindicating a modified form of a substantive *ethical naturalism*, i.e., the absence of an unbridgeable logical gap between statements of facts and values of the kind maintained by Hume, Weber and Moore. And the theory of explanatory critique is most economically presented as a refutation of the philosophical orthodoxy known as 'Hume's law' that the transition from factual to evaluative statements,

although frequently made (and perhaps even psychologically necessary), is logically inadmissible.

It need not be denied by the advocate of Hume's law that causal relations exist between factual and evaluative statements such that they motivate, predispose or *causally influence* each other, but it is asserted to be the case that facts do not *logically entail* values. Doubt is immediately cast upon this by the value-impregnated character of much social scientific discourse. This seems closely bound up with the value-impregnated character of the social reality that the social sciences are seeking to describe and explain, which is such that the best (most precise or accurate or complete) description of a social situation will almost inevitably be evaluative, i.e., possess value implications. However the defender of Hume's law can still argue that one is free to reject the value, so to speak, in the social reality which necessitates such a description. It is for these sort of reasons that the arguments, prevalent in the mid- and late-1960s of Searle from institutional facts, Prior, Philippa Foot and others from functional facts and Anscombe's generalization of their arguments through to the notion of flourishing are less than logically compelling. For one can always dispute that promising, good watches, knives or guns or the flourishing of some particular species are themselves good things.

The critique of Hume's law really gets off the ground when we refuse to detotalize or extrude (e.g. by hypostatization) social beliefs from the societies in which they are found, i.e., which include or contain them and in which they are in some manner formed. Such beliefs may patently be logically contradictory, as Edgley and Archer note, or in some other way, be false to the subject matter they are *about*. And it is clearly within the remit of factual social science, which includes in its subject matter not just social objects but, as social objects, beliefs about those objects, to show this. If and when it has done so we can pass immediately to a negative evaluation of them and of action based on them, and, *ceteris paribus*, to a positive evaluation of their rejection.

The second step is taken when we reject the idea that beliefs cannot be causally explained. If we have a true account of the causes of such false beliefs then we may pass immediately to a negative evaluation of those causes, and thence to any condition, structure or state of affairs found to be necessary for them, and thence, *ceteris paribus*, to a positive evaluation of action directed at removing or transforming those causes and their conditions. In a nutshell, as Collier points out, the theory of explanatory critique opens up the exciting possibility that we may be able to *discover* values, where beliefs prove to be *incompatible* with their own true explanation.

Let us now consider some possible rejoinders. First, it might be objected that this refutation depends upon our acceptance of the value that truth is a good and falsity is an ill. But that this is so is a condition of factual discourse (an aspect, as it were, of the logical geography of the concept of a belief), and so it does not involve anything other than considerations intrinsic to facts to legitimate the deduction of values, as is denied by Hume's law.

Second it is not an objection to point out that truth is not the only social good or falsity the only social ill, so that the inference schemes of explanatory critique may be overridden by other considerations. Science is only one among other social institutions, and truth among a number of values. But this does not gainsay the fact that other things being equal truth is good and falsity is ill. Third, it is the case that the inference from the negative evaluation of a structure or state of affairs accounting for the falsity of a belief to a positive evaluation of action rationally directed at transforming it is contingent upon (i) substantive theory and (ii) concrete practical judgements. *That* something should be done *ceteris paribus* is undeniable; *what* should be done is a different matter. It is perhaps this consideration that motivates Lacey's emphasis on the importance of insider, shared, tacit, 'movement-based' knowledge as distinct from 'grand theory'.

Finally all these inference schemes only hold *ceteris paribus*, other things being equal. But this has an exact parallel in scientific discourse *simpliciter*. To invoke a causal law is not to say what will happen but what tends to happen or what would happen *ceteris paribus*. The *ceteris paribus* clause is a condition for moving from fact to fact in the open-systemic world to which the laws of nature transfactually apply as much as it is to moving from fact to value in the practical social world of belief, judgement and action. Where philosophical orthodoxy poses radical dichotomies, critical realism finds instead exact parallels. It is difficult not to feel that the theory of explanatory critiques has definitively refuted Hume's law.

Dialectic

The dialectical phase of critical realism was initiated by the publication of DPF in 1993 (the principal themes of which were resumed in PE (1994)). This had three main objectives: (1) the dialectical enrichment of critical realism; (2) the development of a general theory of dialectic, of which Hegelian dialectic could be shown to be a special, limiting, case; (3) the generation of the rudiments of a totalizing critique of Western philosophy. DPF argued that *determinate absence* was the void at the heart of the Western philosophical tradition; that it was this concept that was crucial to dialectic, a concept which in the end Hegel could not sustain. It essayed a real definition of dialectic as the *absenting of constraints* (which could be viewed as *absences*) on *absenting absences or ills*, applicable quite generally, whether in the epistemic, ethical or ontological domains; and it adumbrated a system – of dialectical critical realism (DCR), the terms of which were themselves related dialectically. This system was composed of a *first moment* (1M) – of *non-identity* – corresponding roughly to transcendental realism; a *second* dialectical *edge* (2E), pivoting on the notion of absence and other concepts of *negativity*; a *third level* (3L), revolving around notions of *totality*, holistic causality and the like and a *fourth dimension* (4D), turning on *transformative praxis*, the unity of theory and

practice in practice and so on. It should be noted that even though the triadic Hegelian dialectic – of identity, negativity and totality – shared two of these terms in common, their content in the critical realist dialectic is radically different. Thus DPF argued that Hegel ultimately could not sustain real negativity and that his totalities were all essentially closed rather than open. The upshot of DPF is that the moral good, more specifically a vision of a freely flourishing society, is implicit in every expressively veracious action or remark. Moral realism is here now combined with ethical naturalism; and the theory of explanatory critique is conjoined with a very radical emancipatory axiology turning on the theoretico-practical duality of every judgement and act. There is objective good, but it cannot necessarily or normally be identified with the actually existing morality of any particular society.

The introduction to Part IV outlines some of the main themes of DPF. Here it will be sufficient to contextualize it and say a little about its structure and its relation to pre-dialectical critical realism. I have already noticed that critical realists tended to (and were in part motivated by) a reassessment of Marx as a scientific realist, at least in *Capital*. There he maintains that explanatory structures (or, in his favoured terminology, essential relations) are (a) distinct from (b) often, and even normally, out of phase with (i.e., disjoint from) and (c) perhaps in opposition to the phenomena (or phenomenal forms) they generate. But, Marx never satisfactorily theorized his scientific, as distinct from material object, realism. This, together with four other imbalances or asymmetries in his intellectual formation – *viz.* the underdevelopment of (i) his critique of empiricism in comparison with his critique of idealism, (ii) of the theme of (α) objectivity as distinct from (β) labour (i.e., of the intransitive in contrast with the transitive dimension), (iii) and of normativity in relation to geo-historicity (i.e., of the intrinsic – judgementally rational – within the extrinsic – epistemically relative – aspect of the transitive dimension) and (iv) of the research programme of geo-historical materialism in comparison with the critique of political economy, helped to account for all of (1) Marx' mature return to Hegel, (2) the Hegelian residues in Marxist thought, (3) the ambivalences and contradictory tendencies within his writings and (4) the tendency for Marxist epistemology to fluctuate between a sophisticated idealism (roughly β without α) and a crude materialism (roughly α without β). Be that as it may, this inevitably led to the reopening of the question of the nature of the Marxian dialectic and of Marx's relation to Hegel.

There is a remarkable consistency in Marx's criticisms of Hegel from 1843 to 1873. These turn, formally, on Hegel's subject–predicate inversions (including the critique of his idealistic sociology which confounds alienation and objectification, thus implicating Hegel in a metaphysical closure and betraying the presence of what Bhaskar calls '*ontological monovalence*', i.e., the generation of a purely positive account of being, the absenting of absence which is the cardinal mistake of Western philosophy), his principle of iden-

tity (involving the reduction of being to thought, i.e., the epistemic fallacy) and his logical mysticism (including the reduction of science to philosophy, i.e., the 'speculative illusion'); and, substantively on his failure to sustain the autonomy or intransitivity of nature and the geo-historicity, i.e., the non-monovalent character, of social forms. Notoriously, Marx never realized his wish to make accessible to the ordinary human intelligence, in two or three printer's sheets, what is *rational* in the *method* which Hegel discovered and at the same time mystified. This sets the agenda for Bhaskar's project in DPF which is conceived as an essentially preservative generalization and enrichment of critical realism but a non-preservative sublation of Hegelian dialectic. Before I turn to the rational kernel and the mystical shell in the Hegelian dialectic it is worth sketching a plausible critical realist reconstruction of Marx's dialectic.

Thus: Marx understood his dialectic as *scientific*, because it set out to explain the contradictions in thought and the crises of socio-economic life in terms of the particularly contradictory essential relations generating them; as *historical*, because it was both rooted in, and (conditionally) an agent of the changes in the very relationships and circumstances it described; as *critical*, because it demonstrated the historical conditions of validity and limits of adequacy of the categories, doctrines and practices it explained; and as *systematic*, because it sought to trace the various historical tendencies and contradictions of capitalism back to certain existentially constitutive features of its mode of production. The most important of these were the contradictions between the use-value and value of the commodity, and between the concrete useful and abstract social aspects of the labour it embodies. These contradictions, together with the other structural and historical contradictions they ground, are both (a) *real inclusive oppositions* in that the terms or poles of the contradictions existentially presuppose each other, and (b) *internally related* to a mystifying form of appearance. Such dialectical contradictions do not violate the principle of non-contradiction, for they may be *consistently described*. Nor are they scientifically absurd, for the notion of a real inverted – or otherwise mystifying – misrepresentation of a real object, generated by the object concerned is readily accommodatable within a non-empiricist, *stratified* ontology in which thought is included within reality, not hypostatized.

What of the rational kernel and the mystical shell? The rational kernel of the Hegelian dialectic is essentially an epistemological learning process, in which inconsistencies are progressively remedied by resort to greater depth and/or (more generally) totality. Thus the Hegelian dialectic functions in one or other of two basic modes: (1) by bringing out what is implicit, but not explicitly articulated, in some notion; or (2) by repairing some want, lack or inadequacy in it. In either case some *absence* or *incompleteness* in the pre-existing conceptual field comes to be experienced as an *inconsistency* which is remedied by resort to a greater *totality*. This is essentially the epistemological dialectic called 'the logic of scientific discovery' presented in RTS Chapter 3

and revisited as a dialectic of truth in DPF Chapter 3.2. The mystical shell of Hegelian dialectics is ontological monovalence, manifest *inter alia* in the absence of the concept of determinate absence, and with it of uncancelled contradiction, open totality and ongoing transformative praxis.

For DCR, dialectic is essentially the positive identification and elimination of *absences*, whether then conceived as argument, change or the augmentation of (or aspiration to) freedom. For these depend upon the positive identification and elimination of mistakes, states of affairs and constraints, all of which can be seen as involving or depending upon absences. Indeed absence is ontologically prior to, and the condition for, presence or positive being. It includes processes as well as states (products) and states-in-process as well as process-in-states. Moreover it opens up, in what DCR styles the dialectic of dialectical and analytical reasoning (in which dialectical reasoning overreaches but contains analytical reasoning), the critique of the fixity of the subject, in the traditional subject–predicate form. Most characteristically in the 'identity thinking' of the 'analytical problematic'. Indeed it is the absence of the concept of absence in ontological monovalence that underpins the failures of traditional philosophy even at 1M.

The moments of the system of DCR will now be briefly rehearsed. 1M is characterized by non-identity relations such as those involved in the critique of the epistemic and anthropic fallacies, of identity theory and actualism. Unified by the concept of alterity, it emphasizes scientific intransitivity, referential detachment (the process whereby we detach the referent (and referential act) from that to which it refers), the reality principle and ontology which it necessitates. More concretely, 1M fastens on to the transcendentally necessary stratification and differentiation of the world, entailing concepts of causal powers and generative mechanisms, alethic truth and transfactuality, natural necessity and natural kinds. *Alethic truth* is the truth of, or real reason(s) for, or dialectical ground of, *things* as distinct from *propositions*. This is possible in virtue of the ontological stratification of the world and attainable in virtue of the dynamic character of science, social science, explanatory critique and emancipatory axiology. It is the concept of alethic truth that is the ground for the transcendental realist resolution of problems such as those of induction which arise from actualizing, destratifying nature (and then science) and for the explanatory critical refutation of Hume's law.

2E is unified by the category of absence, from which as I shall shortly show the whole circle of 1M–4D links and relations can be derived. Its critical cutting edge is aimed at the Parmenidean doctrine of ontological monovalence, the Platonic analysis of negation in terms of difference and the Kantian analysis of negative into positive predicates. It spans the gamut of categories of negativity, contradiction and critique. It emphasizes the tri-unity of causality, space and time in tensed 'rhythmic' spatializing process, thematizing the presence of the past and existentially constitutive process. Contradictions, which fall under 2E, include internal and external, formal logical and

dialectical ones. Dialectical contradictions are mutually exclusive internally related oppositions, conveying tendencies to change. If the dialectics of 1M are most characteristically of stratification and ground, those of 2E are typically of process, transition, frontier and node; but also generally of opposition including reversal.

3L is unified by the category of totality. It pinpoints the error of ontological extensionalism, including the hypostatization of thought. It encompasses such categories and themes as reflexivity, emergence, transcendence, constellationality, holistic causality, concrete universality and singularity, internal relationality and intra-activity, but also detotalization, alienation, split and split off, 'TINA formation', illicit fusion and fissure. Its dialectics are of centre and periphery, form and content, figure and ground, generative separation and dealienation, retotalization in a unity-in-diversity.

4D is unified by the category of transformative praxis or agency. In the human sphere it is implicit in the other three moments. There is a special affinity with 2E, since agency is (intentional) causality, which is absenting. Agency is sustained philosophically – in opposition to dualistic disembodiment and reductionist reification – by an emergent powers materialist orientation and substantively by the concept of four-planar social being. On this generalization of critical naturalism, social life *qua* totality is constituted by four dialectically interdependent planes: of material transactions with nature, interpersonal relations, social structures and the stratification of the personality. And the moral evolution of the species, like the future generally, is conceived as open. Its dialectics are the site of ideological and material struggles, but also of absolute reason (the unity of theory and practice in practice) and it incorporates DCR's dialectic of desire to freedom.

Let me give, by way of conclusion, an indication of how dialectical critical realism can be dialectically presented. We may start with the concept of absence, say as manifest in desire. This immediately gives us the concepts of referential detachment, existential intransitivity and thence ontology. Whence we proceed to classification and causality. With the first glimpse of ontological structure we have alethic truth and the transfactual efficacy it affords. But to cause is to negate and all negation is in space–time and so we have the entire range of 2E categories from constraint to dialectical contradiction to rhythmic spatio-temporal efficacy. The contradictions within and between entities yield emergence, and thence it is a short route to the 3L categories of totality, holistic causality and concrete universal = singular. Totality is inwardized as, *inter alia*, the reflexivity shown in judgement and the monitoring of practice. Now in the realm of 4D, in virtue of the transcendental necessity of social structure for practice, we can derive from the sole premiss of the activity-dependence of social structure, the transformational model of social activity, the relational social paradigm and the epistemological, ontological, relational and critical limits on naturalism, including the derivation of values from facts. In virtue of our intentional

embodied agency, to act is to absent, and in desire or the solidarity implicit in the fiduciariness of the judgement form, the object of our absenting agency is constraint. Then, by the logic of dialectical universalizability, we are driven to absent all dialectically similar constraints, and then to absent constraints as such in virtue of their being dialectically similar; and finally to engage, on the basis of the progressive generalization of the concept of freedom to incorporate flourishing and potentialities for development, and the negative generalization of constraint to include ills and remediable absences generally, in the totalizing depth praxis that would usher in the eudaemonistic or good society, which in this way can be shown to be already implicit in the most elemental desire.

R.B.

Notes

1 Its publication coincides with the second Annual Conference of the Centre for Critical Realism (CCR) which is a registered educational charity designed to promote and network for critical realism; and the establishment of the International Association for Critical Realism (IACR), a democratically constituted membership body affiliated to the CCR.
2 *Popular Scientific Lectures*, 1894, p.192
3 *New Philosophy of the Social Sciences*, London, 1987.
4 Cf. A. Donegan, 'The Popper-Hempel Theory Reconsidered', *Philosophical Analysis and History*, ed. W. Dray, New York, 1966.
All references in the text refer to the original books.

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R.B.
May 1998

Part I

TRANSCENDENTAL REALISM AND SCIENCE

1

INTRODUCTION

Basic texts and developments

Roy Bhaskar and Tony Lawson

Roy Bhaskar's *A Realist Theory of Science* emerged into the intellectual scene at a time of vigorous critical activity in western philosophy of science. Central to the latter was a sustained challenge to the then dominant positivist conception of science. Two fundamental elements of the positivist world view undergoing particular scrutiny and criticism were the assumptions that science is monistic in its development and deductive in its structure. Even so support for the positivist conception was far from giving way entirely. And a significant reason for its continuing survival was the inability of its opponents to sustain in a sufficiently coherent manner, precisely those features – scientific change and the non-deductive aspects of theory – that had been found to be fundamental to the anti-positivist critique. A major achievement of Bhaskar's *A Realist Theory of Science* is that it explained and contributed significantly to resolving this situation. Specifically, Bhaskar demonstrated how the preservation of the rational insights of both the anti-monistic and anti-deductivist tendencies in the philosophy of science necessitated the construction of a new ontology – and of a corresponding account of (natural) science. It necessitated, in fact, a reorientation of philosophy towards a non-anthropomorphic conception of the place of humanity in nature. This was a shift in philosophy, referred to by some as a Copernican Revolution, that culminated in a new *realist* philosophy of science.

It is conceivable that most scientists would subscribe to being scientific realists in the sense that they accept that the theoretical terms they employ possess real referents independently of their theorising. It is important to recognise, however, that Bhaskar's support for a realist conception of science does not depend upon any empirical assessment that scientists (implicitly or explicitly) so subscribe. Rather Bhaskar sustains a metaphysical realism by way of elaborating an account of what the world 'must' be like for those scientific practices accepted *ex posteriori* as successful, to have been possible. In this manner a realist perspective is obtained which neither presupposes

nor justifies a realistic interpretation of any substantive scientific theory, and which preserves the possibility of criticising specific practices of scientists.

In establishing such a metaphysical realism Bhaskar confirms the feasibility of a (revelatory) philosophy of science, as well as, within philosophy, of an ontology. Philosophy is distinguished from science *not* according to its subject field, nor even in virtue of the questions asked, and certainly not because of any supposed investigation of some autonomous order of being. Rather philosophy is distinguished by its method and more generally by the sorts of arguments it deploys, which are transcendental in the sense of Kant.

Specifically, the general form of a philosophical investigation accepted by Bhaskar, the transcendental argument, turns upon elaborating necessary conditions of certain human (in the case of *A Realist Theory of Science*, scientific) activities. Now Kant employed the transcendental procedure (in elaborating his transcendental idealism) in an individualist and idealist mode. However, Bhaskar demonstrates that there is little need to be so restrictive. In particular the social activities described in the premises which initiate the argument may be both historically transient and also dependent upon the powers of human beings as material objects or causal agents rather than merely thinkers or perceivers. Similarly, philosophical conceptions of scientific activities may also be historically transient, just as the results of philosophical analysis may constitute transcendental realist, not idealist, and epistemically relativist, rather than absolutist, conclusions. Philosophical argument so interpreted can be seen to be dependent upon the form of scientific practices but irreducible to the content of scientific beliefs. In applying the transcendental procedure in this less-restrictive manner Bhaskar develops and sustains his account of *transcendental realism.*

But how is it possible for premises of transcendental arguments to be selected without implying an invalid commitment to the epistemic significance of the activities described? Why, in particular, should opponents of any transcendental realist conception be convinced by Bhaskar's choice of premises for his argument? Avoidance of arbitrariness can be achieved only by focusing upon accounts of activities that are acceptable to both (or all) sides to a dispute. If possible, indeed, it is best to find premises that opponents have regarded as fundamental. Where this is achieved the aim is to demonstrate not only that the transcendental realist account can accommodate the activities in question, but also that opponent positions sponsoring the activities cannot so accommodate them consistently, i.e., without generating metaphysical absurdity or some such.

Such a demonstration is precisely what Bhaskar achieves in his classic analysis of experimental activity, an analysis which forms the centrepiece of chapter 1 of *A Realist Theory of Science* reprinted below. By so considering experimentation, sponsored by both empiricists and Kantians (as well as conceptual transformations sponsored by super idealists) Bhaskar demonstrates how, in the end, it is only a realist analysis that can sustain the

intelligibility of such practices. Moreover the resulting realist theory, Bhaskar's transcendental realism, provides an alternative to positivism which allows us both to recognise the cumulative character of scientific knowledge without collapsing this into a monism, and also to acknowledge a surplus component in scientific theory without sliding into subjectivism.

In the course of his analysis, Bhaskar grounds the insight that causal laws are ontologically distinct from the pattern of events. Specifically Bhaskar shows how the intelligibility of experiments presupposes that reality is constituted not only by experiences and the course of actual events, but also by structures, powers, mechanisms and tendencies – by aspects of reality that underpin, generate or facilitate the actual phenomena that we may (or may not) experience, but are typically out of phase with them. Bhaskar also establishes that reality in general is both multi-dimensional and stratified and also open and differentiated (in the sense that closed systemic situations in which event regularities occur are highly restricted).

From this transcendental realist ontology of structures and differences an account of rational scientific development is quickly determined. This Bhaskar sets out in chapter 3 of *A Realist Theory of Science*, the relevant parts of which are also reproduced below. Briefly put, explanatory science, according to the perspective supported, seeks to account for some phenomenon of interest – typically an experimentally produced event pattern – in terms of a (set) of mechanism(s) most directly responsible. Producing this explanation will involve drawing upon existing cognitive material, and operating under the control of something like a logic of analogy and metaphor, to construct a theory of a mechanism that, if it were to work in the postulated way, could account for the phenomenon in question. The reality of the mechanism so retroduced is subsequently subjected to empirical scrutiny, and the empirical adequacy of the hypothesis maintained compared to that of competing explanations. Following this any explanation that is (tentatively) accepted must itself be explained, and so forth, a move which, in itself, presupposes a certain stratification of reality. On the transcendental realist view of science, then, its essence lies in the *movement* at any one level from knowledge of manifest phenomena to knowledge, produced by means of antecedent knowledge, of the structures that generate them.

So among the distinctive features of Bhaskar's original account of transcendental realism are:

(i) A revindication of ontology, of the theory of being, as distinct from (ultimately containing) epistemology, the theory of knowledge, and a critique of the 'epistemic fallacy' which denies this;
(ii) A distinction between the domain of the real, the actual and the empirical and a critique of the reduction of the real to the actual in 'actualism' and then to the empirical in 'empirical realism', together with a conception of the transfactual, non-empirical universality of laws as the causal

powers, or more specifically tendencies, of *generative mechanisms* which may be possessed, unexercised, exercised, unactualised and actualised independently of human perception or detection;

(iii) A conception of the stratification, differentiation and openness of both nature and sciences, and of the distinction between pure and applied sciences and explanations;

(iv) Isolation of a general dynamic of scientific discovery and development involving the identification of different levels of natural necessity, which in turn is understood as radically non-anthropomorphic. And thence:

(v) The associated resolution of a whole series of philosophical problems to which orthodox accounts of science had given rise, most notoriously the problem of induction (cf. *Realist Theory of Science*, 3.5/3.6, reprinted below).

It is easy enough to see how philosophy of science has the potential to provide a directional input into the practices of science. For although Bhaskar's analysis suggests that when scientists are practising science they are implicitly acting upon something like transcendental realism, it does not follow that transcendental realism, or any other philosophy, is always or consistently acted upon, or dominant, or even acknowledged. It is for this reason that in his subsequent *Possibility of Naturalism*, Bhaskar is able to conclude that 'one is . . . *qua* philosopher of science, at perfect liberty to criticise the practice of any science' (p. 16). Nothing in the foregoing should be taken to imply that philosophy can do the actual work of science for it. If the elaboration of a transcendental realist perspective provides grounds for supposing that science can successfully uncover structures and mechanisms that govern some identified phenomenon of interest, philosophy cannot do the work of uncovering. This is the task of science. Philosophy, however, is able to make a difference to science in the manner noted: by, amongst other things, affecting the questions put to reality, and the manner in which this is done.

If *A Realist Theory of Science* demonstrates that an adequate account of scientific development requires the concepts of a stratified and differentiated reality, it is clearly a further requirement that knowledge cannot be equated with direct experience. Nor is it intelligible that knowledge is created out of nothing. Rather knowledge can only be a produced means of production, as revised understandings are achieved via the transformation of existing insights, hypotheses, guesses and anomalies, etc. Bhaskar's own contribution, of course, is itself a transformation of prior claims and understandings, and the work of Rom Harré figures prominently amongst those whose contributions significantly influenced *A Realist Theory of Science.* One such influential contribution by Harré – chapter 1 of *Causal Powers*, written jointly with E.H. Madden – is reprinted below, albeit a contribution that only appeared in this published form at the same time as *A Realist Theory of Science* was also appearing in print.

In line with much philosophy of science of the period, the starting point

for Harré and Madden is a conviction that positivism, specifically the Humean conception of causality and its linear descendent, the 'regularity theory', is not sustainable. Indeed, for Harré and Madden it is essential to explain why the Humean point of view continued over many centuries to attract so many adherents. In providing their explanation Harré and Madden identify two widely held, but questionable, assumptions, which lead inexorably to the Humean position. The first assumption presupposes, amongst other things, an exclusive dichotomy between the formal and the psychological. Specifically, it is a belief maintained by empiricist philosophers, that the philosophical analysis of non-empirical concepts must be wholly in terms of formal logic, and that any residual features not so susceptible to philosophical analysis, must be capable of analysis in terms of its psychological origins. Against this Harré and Madden argue that adequate accounts of the most important metaphysical concepts with which philosophy deals, like cause, theory, explanation, natural necessity, can be neither purely formal nor psychological but require attention to what they term 'the content of knowledge', content which usually goes beyond reports of immediate experience. These authors argue that such concepts can be successfully differentiated, the rationality of science defended, and the possibility of an independent reality sustained only by way of considering certain general features of the 'content' of relevant propositions by which they can ultimately be distinguished as possessing a conceptual necessity, irreducible to either logical necessity or psychological illusion.

The second Humean assumption questioned by Harré and Madden is that the ontology of science is restricted to the world of events. This conception, of course, is encouraged by Hume's opening comments in both the *Treatise* and the *Enquiry*, in which he quickly moves from a theory that experience comes in atomistic impressions, to a conception of the experienced world whereby this too is atomistic, comprising atomistic events. The supposed independence of successive events, and of coexisting properties, is a related and also fundamental aspect of this Humean view. Against this standpoint Harré and Madden draw upon the psychology of perception to demonstrate the untenability of Hume's doctrine of atomist impressions. And against the conception that the experienced world can be adequately conceived as a sequence of atomistic and independent events, the authors defend an ontology of ultimate and derived things whose interactions produce the flux of events. Specifically, through developing concepts of powers, natures and generative mechanisms, Harré and Madden, like Bhaskar, are able to demonstrate that a variety of rational constraints upon logical possibility can be determined so as to limit expectations as to the patterns of events likely to be identified and what ensembles of properties the things and materials of the world are likely to manifest. From these constraints Harré and Madden develop a theory of natural necessity. The upshot is a conception of the natural world as a interacting system of powerful particulars, giving rise to a

patterning of events and a manifestation of properties, bearing upon the multitudinous phenomena of the world we experience.

In the chapter of *Causal Powers* reproduced below, Harré and Madden indicate how natural necessity in the world is reflected in discourse about the world. In particular, they argue that causal hypotheses invariably involve conceptual necessity, and that this necessity is not merely stipulative or conventional in character but expresses something about the nature of physical systems. Fundamental here are the categories of 'power' and 'ability' possessed by something in virtue of its 'nature'. Specifically they defend the position that it is the 'ineliminable but non-mysterious powers and abilities of particular things . . . [that] are the ontological "ties that bind" causes and effects together and are what the conceptual necessity of causal statements reflects' (p. 11).

A further insight defended in this chapter is that conceptual and natural necessity are also reflected in descriptions of substances. The transformations, etc., that particulars or substances are liable to undergo as well as what they are able to do are explained by reference to the thing in itself. As Harré and Madden summarise 'the relation between what a thing is and what it is capable of doing and undergoing is naturally necessary. It is this natural necessity that the conceptual necessity of the ensemble of powers and liabilities ascribed by the use of a term like "copper" reflects' (p. 14).

These authors further argue that it is essential that an account of natural sciences sustain a distinction between two ranges of essential properties. First, there are the nominal essences, those properties whose manifestations, according to Harré and Madden, are essential to a thing or sample or substance being of a certain kind. Although acknowledging that meanings have histories, Harré and Madden hold that nominal essences are fixed, and can be known *a priori.* And second, science is also concerned with real essences, with the natures of things or substances. These are *ex posteriori* discoveries, and serve to explain manifest properties. Harré and Madden argue that it is only through considering the empirical status of the predicates involved in any investigation, as opposed merely to looking at the logical structure of definitions, that it is possible to distinguish adequately between the kinds of definitions that appear in the natural sciences.

Now when discoveries of real essences justify our holding that certain properties are its nominal essence, then a diachronic process of meaning development creates a genuine conceptual necessity. In particular, where the co-presence of an *ensemble* of manifest properties is explained in terms of the real definition of a substance, the more the corresponding predicates are used as part of the meaning of the term for the thing or substance. And when discoveries about the means of causal production make clear the role of the appropriate powerful particular in that production, and the nature of that particular allows us to claim the necessity of just such an outcome of the productive process, then the concept of that particular can legitimately

be allowed to come to include the power to produce those effects. This is the theoretical account, formulated by Harré and Madden, of the origin of necessary connections between empirical concepts, an account which, in the chapter excerpted below, is shown to make sense of the conceptual development of the substance concept 'copper'.

The realist theory of science supported by the contributions by Bhaskar and by Harré and Madden is taken up by Andrew Sayer in the next essay included below. Sayer's purpose is to use the realist theory to clarify the relations between the *theoretical* and the *empirical*, and between the *abstract* and the *concrete.* Sayer's starting point is the problematic history of these terms within Marxist writings – although, of course, the history of their usage is no less problematic in social theory more widely conceived (see, for example, Lawson's discussion of how abstraction is conceived in modern mainstream economics in the final essay reproduced below). According to Sayer, the possibility of sustaining a basis for distinguishing theoretical research (or critique or reflection) and empirical research, necessitates a prior consideration of the related, but distinct, contrast between the abstract and the concrete. And in order to proceed, it is also necessary to explicate Marx's insights that abstractions may or may not be useful or adequate ones. In Marx's terminology the result may be 'rational abstractions' or 'chaotic conceptions'. It is in making sense of these categories that Sayer first draws upon realist insights and argument.

As Bhaskar and Harré and Madden indicate, realist analysis undermines the Humean predilection for atomism. And in the realist theory causation is bound up with natural necessity: things have powers and dispositions to act in certain ways in virtue of their intrinsic structures or natures or real essences. Things possess powers in virtue of their intrinsic structures, powers that may or may not be exercised. If they are triggered they can be in play as mechanisms, whose effects may or may not be actualised, depending upon the play of countervailing mechanisms. In transcendental and critical realism it is, as we can see, the notion of a tendency which denotes characteristic ways of acting or effects of mechanisms which may or may not be actualised.

Accepting this perspective Sayer underlines the insight that scientific 'laws', or its fundamental results, are not about universal empirical regularities but expressive of structures, powers, mechanisms and tendencies. The essential characteristic of law-likeness is not (empirical) universality but (natural) necessity. Given this insight Sayer feels able to present a clarification of the relationship between both the abstract and the concrete and also between good and bad abstraction. Good or 'rational' abstractions are interpreted as those which isolate necessary relationships. The concrete, being a unity of diverse determinations, is a combination of several necessary relationships. However, because the form of the combination is contingent, it is only determinable though empirical research. As such, insists Sayer, 'its form cannot be assumed to have already been "taken up" into the theoretical

framework in the same way that the nature of the abstraction can' (p. 9). A bad abstraction or 'chaotic conception' is one which is based upon a non-necessary relationship, or which divides the indivisible by failing to recognise a necessary relationship. The same distinctions are drawn when considering external and internal relations. Specifically, a rational abstraction – unlike a chaotic conception – takes due account of structures of internal relations.

With these distinctions established it is possible to clarify the relationship between the 'theoretical' and 'empirical'. The theoretical, according to Sayer, makes its strongest claims about necessary relations in the world, and does so by 'anchoring itself' upon abstract concepts. The latter may be sufficient to indicate something about the tendencies in play in a given context. But in an open world such claims are inevitably non-committal about contingent relations occurring in concrete configurations. The latter, concludes Sayer, requires empirical analysis. Finally Sayer points to both the positive implications of recognising these distinctions as well as the analytical complications, limitations or regressions that follow from a failure so to do.

It is clear that the switch of emphasis in the philosophy of science engendered by the contributions of Bhaskar, Harré and others is away from epistemology towards ontology. Even so any philosophical position, even an ontologically oriented one, ultimately bears epistemological implications. Tony Lawson's *Economics and Reality* considers the epistemological consequences of critical realism at length. It is true, as has already been indicated, that Bhaskar gives a broad outline of theory development in his 'The Logic of Scientific Discovery', included as chapter 3 of *A Realist Theory of Science*, reproduced below. But it is arguable that his epistemological elaborations do not go very far. They are informative about primary objectives, e.g. to uncover natural necessity in an irreducibly *a posteriori* process of discovery, but less specific about *how* these are to be achieved. Mostly, in section 1 of chapter 3, Bhaskar distinguishes transcendental realism from 1) empiricism in seeing the initial patterning of events as signalling an invariance of a *result* rather than of a regularity; and from 2) idealism in interpreting constructed hypotheses of generative mechanisms as something that may be real rather than *merely* imaginary, stimulating a project on continuous empirical assessment. However little is said about how explanatory projects might proceed in conditions where the experimental production of event regularities is not feasible, and where, as in the social realm, few of any interest seem to occur spontaneously.

One of the features of *A Realist Theory of Science* is that it constituted an immanent critique of orthodox – mainly empirical realist – philosophies of science. As such it focused on the experimental sciences of nature, such as physics and chemistry, of which these philosophies derived their *prima facie* plausibility and ideological power. However Lawson cautions against viewing the fact of openness of the social realm as an 'epistemological limit on naturalism', a move that risks encouraging the inference that the natural

sciences can be reduced to the experimental natural sciences or astronomy. Amongst other things this may limit the possibility of inferring insights from the successful non-experimental natural sciences, such as geology, seismology and so forth. Thus, although Lawson himself is expressly addressing the possibilities for social science, in the relevant chapters reproduced below he is discussing the wider issue of the possibilities for explanatory conduct in non-experimental contexts. As such his considerations are just as relevant to many natural sciences, with the well-controlled experiment being seen to be a special case. Hence his chapters are included here in Part 1.

Despite emphasising the open, dynamic and highly internally related nature of much of reality including the social world, Lawson is confident – contrary to the views of some other critical realists such as Andrew Collier – that it *is* possible to identify causal mechanisms of interest, and even possible to say something about general strategies for doing so. Central to Lawson's assessment are the concepts of *contrastives, demi-regs* and *relative explanatory power.* Now if reality, including the social realm, is open and complexly structured, with a shifting mix of mechanisms lying behind the surface phenomena of direct experience, how can we begin even to detect the separate effects of (relatively distinct) mechanisms? In motivating his answer, Lawson emphasises that controlled experiments do not *all* take the form of insulating single stable mechanisms in 'repeated trials' with the intention of generating event regularities. An alternative scenario, illustrated for example by plant-breeding experiments, involves the use of control groups to help identify the effects of specific mechanisms of interest. Where, for example, crops are grown in the open there can be no expectation that all the causal factors affecting the yields are stable, reproducible or even identifiable. Yet progress in understanding can be achieved: through ensuring that two sets of crops receive broadly similar conditions except for one factor that is systematically applied to one set but not to the other. In this case any systematic differences in average yields of the two sets of crops can with reason be attributed to the factor in question.

In other words, experimental control frequently takes the form of comparing two different groups or populations with common or similar (if complex, irreversible and unpredictable) histories and shared (if non-constant) conditions, excepting that one group is 'treated' in some definite way that the second, control, group is not. Or, more typically, when various (similar but non-uniform) background factors such as soil composition and light are not directly controllable, it may be possible to divide the relevant land into a set of plots and then attempt to assign certain quantities of fertiliser to the various plots in a random way, with some plots receiving no fertiliser at all. Under such conditions the difference between the mean yield of the unfertilised plots is contrasted with that of fertilised plots to see if there is a systematic and significant difference – which can be attributed to the fertiliser.

In the plant breeding scenario just described, of course, the aim is to

experiment with some compound that is already suspected of possessing yield-increasing causal powers. Lawson's primary concern, however, is with detecting the effects of hitherto unknown or unrecognised mechanisms. But it is easy enough to appreciate that the logic of the argument carries over to the latter conditions. Consider, for example, a situation wherein, say, it was expected *a priori* that the yield would be roughly the same for a given crop in all parts of the field but is discovered *ex posteriori* to be systematically higher at one end. In this case an experimentalist has *not* actively treated the relevant end of the field. But it seems *prima facie* that there is an additional causal factor in operation here, even if we are as yet unaware of its identity.

The general situation Lawson is identifying as being relevant for social scientific explanation in open systems, then, is one in which there are two or more comparable populations involved, wherein our background knowledge leads us to expect a specific relation between outcomes of these populations (frequently a relationship of similarity but not always), but wherein we are *ex posteriori* surprised by the relation we actually discover. Under such conditions it is *prima facie* plausible that there is a previously unknown and identifiable causal mechanism at work.

An important methodological category here is that of *contrastives.* Contrastives are descriptive statements taking the form 'this rather than that'. And contrastive explanation is concerned with addressing such questions as 'why this rather than that in these conditions?', or 'why P rather than Q in S?'. Contrastive explanation, clearly, is concerned not so much with such questions as 'why is the average crop yield x?' but 'why is the average crop yield in that end of this field significantly higher than that achieved elsewhere?' Explaining this is much less demanding than explaining the total yield. While the latter requires an exhaustive list of all the causal factors bearing upon the yield, the contrastive question requires that we identify only the causes responsible for the difference. But the import of contrastives here lies not so much (or just) in the fact that the task delineated is less demanding, but more in the fact that contrastives alert us to the situation that there is something to be explained at all.

Lawson is suggesting, then, that the effects of causal mechanisms can be identified through formulating interesting contrastives. This, to repeat, means identifying differences (or surprising relations) between outcomes of two groups whose causal histories are such that the outcomes in question might reasonably have been expected to be broadly the same, or at least to stand in some definite anticipated or plausible relationship which is systematically at odds with what we observe. We do not and could not explain the complete causal conditions of any social or other phenomenon. To do so would presumably mean accounting for everything back to the 'big bang' and beyond. Rather we aim to identify single sets of causal mechanisms and structures. And these are indicated where outcomes or features of different groups are such that, given the respective causal histories and conditions of

these groups, their observed relation is other than might have been expected or at least imagined as a real possibility.

But if contrastives are vital for explanatory purposes in non-experimental situations, they are not sufficient for it. The possibility of a useful science depends upon being able to identify relatively enduring mechanisms or processes of the world. Now this requires that at some stage it is possible to detect any such mechanisms and to get some indication of their endurability. According to Lawson, it is frequently the case that in order to detect their effects all that is required is for partial, or rough and ready, regularities to appear. *Ex posteriori* these are frequently found to be the result of underlying mechanisms shining through. In other words, it is not the case thar the surface manifestations of our world need divide into just two scenarios: either i) closed systems supporting strict regularities (whether strictly deterministic or those covered by well-behaved probability laws); or ii) a totally unsystematic random flux. A range of real possibilities lies between these polar extremes. Lawson recognises that it could have turned out that the possibility of reasonably stable mechanisms putting in an appearance in the form of rough but detectable patterns was never actualised; that it remained only a possibility. But *ex posteriori* this has not been the case: rough and ready regularities are everywhere in evidence. Women usually (but not always) get worse jobs than men; a car journey from Cambridge to London is usually (but not always) quicker late at night than during the day; football teams from the UK premier division normally do better than teams from lower divisions in cup competitions; over the 100 years until 1980, measured productivity growth in the UK was frequently less than most otherwise comparable continental industrial countries, and so forth.

Lawson refers to such partial regularities as *demi-regularities* or *demi-regs* for short, and suggests they be categorised as (the objects of) *demi-laws.* This characterisation turns upon both of the common interpretations of the term 'demi' – as either half-way or as false. Certainly any regularity observed can be expected to be partial or incomplete. But equally, although such partial regularities may be about real phenomena and capturing associations, they are not real laws at all, but epiphenomena. Even so, these are nevertheless epiphenomena of potential significance. A demi-reg is precisely a partial event regularity which *prima facie* indicates the occasional, but less than universal, actualisation of a mechanism or tendency, over a definite region of time–space. The patterning observed will not be strict if countervailing factors sometimes dominate or frequently co-determine the outcomes in a variable manner. But where demi-regs are observed there is *prima facie* evidence of relatively enduring and identifiable tendencies in play. Of course, as with the examples detailed above, the demi-regs in evidence will usually capture *relations* between actual phenomena – such as the productivity of UK firms compared with the productivity performances of otherwise comparable firms elsewhere. In short, a basic feature to be expected of explanatory work in

science is an initial focus upon *contrastive demi-regs* considered to be of interest.

If one hypothesis of a mechanism capable of explaining a given contrastive demi-reg of interest is produced, experience suggests that there will usually be many such hypotheses in contention. These can be selected amongst on the basis of relative explanatory power, that is we can (provisionally) accept that theory which can accommodate the largest range of phenomena (typically expressed as contrastive demi-regs) upon which it bears. This remains a context-dependent affair, but entirely feasible. And Lawson suggests that skills of ordinary people in successfully negotiating their daily affairs indicates that this feasibility is regularly actualised. The task is to bring this causalist approach back into the academy. It may not satisfy the mathematical drives, preferences or ideals of the deductivist project of mainstream economists against which Lawson is mainly orienting himself, but it can facilitate an explanatory successful non-experimental science all the same.

How does Lawson's account fit into the schema outlined by Roy Bhaskar in *A Realist Theory of Science*? Contrastives, including contrastive demi-regs, along with the protolaws focussed upon by Bhaskar in *A Realist Theory of Science* (chapter 3.3) are all members of the class of potentially epistemically significant non-random patterns or results in nature (including in the laboratory). The crucial scientific transition is from a member of this class into a generative mechanism or structure which explains it and would ground a law, i.e., a transfactually efficacious tendency, understood as universal (within its range) but non-empirical, necessary but discovered *a posteriori*. Now we are only justified in inferring from the existence of a contrastive demi-reg to the causal efficacy of the mechanism which explains it (rather to the existence of a single mechanism, or set of mechanisms, which would explain it), if this is the *only relevant difference within contrast*, i.e., the intrinsic and extrinsic conditions and principles of organisation are constant (cf. RTS, p. 76) *or* their differences and changes and geo-histories, etc., are otherwise causally irrelevant; that is to say, that for epistemic purposes other things are equal, i.e., a *de facto* ontic or epistemically significant closure has been obtained. In this case the tendency will be actualised and the demi-reg, when explained, will be a law. The experimental situation is contrasted but this broader-contrasted case presupposes a *de facto* epistemically significant closure, i.e., quality (constancy or causal irrelevance or insignificance or accountability) or other things: that *ceteris* are *paribus*.

Notice, incidentally, that Lawson's marrying of realist theory and contrastive explanation facilitates a conception of science that preserves many of the recent insights of feminist philosophy without thereby going into the characteristic postmodernist judgemental-relativist overdrive. That is, Lawson's account naturally accommodates the insight that the sort of issues that are addressed in science will reflect the situations, perspectives and personal-social histories, and so forth, of the scientist without supposing thereby that

all knowledge is *merely* a social construct, immune to rational critical assessment. It is clear, for example, that in the process of choosing a primary phenomenon for explanatory analysis, scientific (and other) interests necessarily come to bear. But once we accept the contrastive nature of social scientific explanation it is equally apparent that the interests of the researcher determine which causal mechanism is pursued as well. For when phenomena in an open system are determined by a multiplicity of causes, the particular one singled out for attention depends upon the contrastive identified as puzzling, surprising, unusual, undesirable or otherwise of interest. It may be that it is only the interested farmer that can recognise that his or her animals are behaving strangely, only the parent that perceives that all is not well with the child, and only the marginalised group that appreciates the nature or extent/effects of certain inequalities, and so forth. Clearly, the inescapably interested and practically conditioned nature of all scientific explanatory endeavour is a fundamental feature of the perspective Lawson defends.

Notice, finally that contrastive explanation along the lines defended by Lawson does indeed generalise the modes of inference already seen to be employed in specific contexts. The significance of the well-controlled experimental situation is precisely that *under such conditions but not others* certain triggering conditions are frequently found to be systematically associated with definite predictable effects, that an even regularity is produced. It is this contrast that renders the experimental setup so significant in science. And, of course, Bhaskar's transcendental argument in support of transcendental realism itself turns upon this more general contrastive assessment, that outside astronomy, event regularities of interest to science are mostly confined to experimental setups. As Lawson summarises the directionality involved: 'Particular differentiations of the world to hypotheses about specific mechanisms; generalised differentiations to philosophical ontologies' (p. 212). Given the *ex posteriori* pervasiveness of contrastive demi-regs, the fact of open systems is seen to be debilitating neither for science nor for philosophy.

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2

PHILOSOPHY AND SCIENTIFIC REALISM

Roy Bhaskar

1. Two sides of 'knowledge'

Any adequate philosophy of science must find a way of grappling with this central paradox of science: that men in their social activity produce knowledge which is a social product much like any other, which is no more independent of its production and the men who produce it than motor cars, armchairs or books, which has its own craftsmen, technicians, publicists, standards and skills and which is no less subject to change than any other commodity. This is one side of 'knowledge'. The other is that knowledge is '*of*' things which are not produced by men at all: the specific gravity of mercury, the process of electrolysis, the mechanism of light propagation. None of these 'objects of knowledge' depend upon human activity. If men ceased to exist sound would continue to travel and heavy bodies fall to the earth in exactly the same way, though ex hypothesi there would be no-one to know it. Let us call these, in an unavoidable technical neologism, the *intransitive objects of knowledge.* The *transitive* objects of knowledge are Aristotelian material causes.[1] They are the raw materials of science – the artificial objects fashioned into items of knowledge by the science of the day.[2] They include the antecedently established facts and theories, paradigms and models, methods and techniques of inquiry available to a particular scientific school or worker. The material cause, in this sense, of Darwin's theory of natural selection consisted of the ingredients out of which he fashioned his theory. Among these were the facts of natural variation, the theory of domestic selection and Malthus' theory of population.[3] Darwin worked these into a knowledge of a process, too slow and complex to be perceived, which had been going on for millions of years before him. But he could not, at least if his theory is correct, have produced the process he described, the

Source: A Realist Theory of Science, London: Verso, 1997, chap. 1, pp. 21–62.

intransitive object of the knowledge he had produced: the mechanism of natural selection.

We can easily imagine a world similar to ours, containing the same intransitive objects of scientific knowledge, but without any science to produce knowledge of them. In such a world, which has occurred and may come again, reality would be unspoken for and yet things would not cease to act and interact in all kinds of ways. In such a world the causal laws that science has now, as a matter of fact, discovered would presumably still prevail, and the kinds of things that science has identified endure. The tides would still turn and metals conduct electricity in the way that they do, without a Newton or a Drude to produce our knowledge of them. The Wiedemann-Franz law would continue to hold although there would be no-one to formulate, experimentally establish or deduce it. Two atoms of hydrogen would continue to combine with one atom of oxygen and in favourable circumstances osmosis would continue to occur. In short, the intransitive objects of knowledge are in general invariant to our knowledge of them: they are the real things and structures, mechanisms and processes, events and possibilities of the world; and for the most part they are quite independent of us. They are not unknowable, because as a matter of fact quite a bit is known about them. (Remember they were introduced as objects of scientific knowledge.) But neither are they in any way dependent upon our knowledge, let alone perception, of them. They are the intransitive, science-independent, objects of scientific discovery and investigation.

If we can imagine a world of intransitive objects without science, we cannot imagine a science without transitive objects, i.e. without scientific or pre-scientific antecedents. That is, we cannot imagine the production of knowledge save from, and by means of, knowledge-like materials. Knowledge depends upon knowledge-like antecedents. Harvey thought of blood circulation in terms of a hydraulic model. Spencer, less successfully perhaps, used an organic metaphor to express his idea of society. W. Thomson (Lord Kelvin) declared in 1884 that it seemed to him that 'the test of "do we understand a particular topic in physics [e.g. heat, magnetism]?" is "can we make a mechanical model of it?".'[4] And as is well known this was the guiding maxim of physical research until the gradual disintegration of the Newtonian world-view in the first decades of this century. Similarly economists sought explanations of phenomena which would conform to the paradigm of a decision-making unit maximizing an objective function with given resources until marginalism became discredited in the 1930's. No doubt at the back of economists' minds during the period of the paradigm's hegemony was the cosy picture of a housewife doing her weekly shopping subject to a budget constraint; just as Rutherford disarmingly confessed in 1934, long after the paradigm was hopelessly out of date, to a predilection for corpuscularian models of atoms and fundamental particles as 'little hard billiard balls, preferably red or black'.[5] Von Helmont's concept of an arche

was the intellectual ancestor of the concept of a bacterium, which furnished the model for the concept of a virus. The biochemical structure of genes, which were initially introduced as the unknown bearers of acquired characteristics, has been explored under the metaphor of a linguistic code. In this way social products, antecedently established knowledges capable of functioning as the transitive objects of new knowledges, are used to explore the unknown (but knowable) intransitive structure of the world. Knowledge of B is produced by means of knowledge of A, but both items of knowledge exist only in thought.

If we cannot imagine a science without transitive objects, can we imagine a science without intransitive ones? If the answer to this question is 'no', then a philosophical study of the intransitive objects of science becomes possible. The answer to the transcendental question 'what must the world be like for science to be possible?' deserves the name of ontology. And in showing that the objects of science are intransitive (in this sense) and of a certain kind, viz. structures not events, it is my intention to furnish the new philosophy of science with an ontology. The parallel question 'what must science be like to give us knowledge of intransitive objects (of this kind)?' is not a petitio principii of the ontological question, because the intelligibility of the scientific activities of perception and experimentation already entails the intransitivity of the objects to which, in the course of these activities, access is obtained. That is to say, the philosophical position developed in this study does not depend upon an arbitrary definition of science, but rather upon the intelligibility of certain universally recognized, if inadequately analysed, scientific activities. In this respect I am taking it to be the function of philosophy to analyse concepts which are 'already given' but 'as confused'.[6]

Any adequate philosophy of science must be capable of sustaining and reconciling both aspects of science; that is, of showing how science which is a transitive process, dependent upon antecedent knowledge and the efficient activity of men, has intransitive objects which depend upon neither. That is, it must be capable of sustaining both (1) the social character of science and (2) the independence from science of the objects of scientific thought. More specifically, it must satisfy both:

(1)′ a criterion of the non-spontaneous production of knowledge, viz. the production of knowledge from and by means of knowledge (in the transitive dimension), and

(2)′ a criterion of structural and essential realism, viz. the independent existence and activity of causal structures and things (in the intransitive dimension).

For science, I will argue, is a social activity whose aim is the production of the knowledge of the kinds and ways of acting of independently existing and active things.

2. Three traditions in the philosophy of science

Viewed historically, three broad positions in the philosophy of science may be distinguished. According to the first, that of *classical empiricism*, represented by Hume and his heirs, the ultimate objects of knowledge are atomistic events. Such events constitute given facts and their conjunctions exhaust the objective content of our idea of natural necessity. Knowledge and the world may be viewed as surfaces whose points are in isomorphic correspondence or, in the case of phenomenalism, actually fused. On this conception, science is conceived as a kind of automatic or behavioural response to the stimulus of given facts and their conjunctions. Even if, as in logical empiricism, such a behaviourism is rejected as an account of the genesis of scientific knowledge, its valid content can still in principle be reduced to such facts and their conjunctions. Thus science becomes a kind of epiphenomenon of nature.

The second position received its classical though static formulation in Kant's *transcendental idealism*, but it is susceptible of updated and dynamized variations. According to it, the objects of scientific knowledge are models, ideals of natural order etc. Such objects are artificial constructs and though they may be independent of particular men, they are not independent of men or human activity in general. On this conception, a constant conjunction of events is insufficient, though it is still necessary, for the attribution of natural necessity. Knowledge is seen as a structure rather than a surface. But the natural world becomes a construction of the human mind or, in its modern versions, of the scientific community.

The third position, which is advanced here, may be characterized as *transcendental realism.* It regards the objects of knowledge as the structures and mechanisms that generate phenomena; and the knowledge as produced in the social activity of science. These objects are neither phenomena (empiricism) nor human constructs imposed upon the phenomena (idealism), but real structures which endure and operate independently of our knowledge, our experience and the conditions which allow us access to them. Against empiricism, the objects of knowledge are structures, not events; against idealism, they are intransitive (in the sense defined). On this conception, a constant conjunction of events is no more a necessary than it is a sufficient condition for the assumption of the operation of a causal law. According to this view, both knowledge and the world are structured, both are differentiated and changing; the latter exists independently of the former (though not our knowledge of this fact); and experiences and the things and causal laws to which it affords us access are normally out of phase with one another. On this view, science is not an epiphenomenon of nature, nor is nature a product of man.

A word of caution is necessary here. In outlining these positions, I am not offering them as a complete typology, but only as one which will be of some

significance in illuminating current issues in the philosophy of science. Thus I am not concerned with rationalism as such, or absolute idealism. Moreover, few, if any, modern philosophers of science could be unambiguously located under one of these banners. Nagel for example stands somewhere along the continuum between Humean empiricism and neo-Kantianism; Sellars nearer the position characterized here as transcendental realist; and so on. One could say of such philosophers that they combine, and when successful in an original way synthesize, aspects of those philosophical limits whose study we are undertaking. It is my intention here, in working out the implications of a full and consistent realism, to describe such a limit; in rather the way Hume did. As an intellectual exercise alone this would be rewarding, but I believe, and hope to show, that it is also the only position that can do justice to science.

Transcendental realism must be distinguished from, and is in direct opposition to, *empirical realism.* This is a doctrine to which both classical empiricism and transcendental idealism subscribe. My reasons for rejecting it will be elaborated in a moment. 'Realism' is normally associated by philosophers with positions in the theory of perception or the theory of universals. In the former case the real entity concerned is some particular object of perception; in the latter case some general feature or property of the world. The 'real entities' the transcendental realist is concerned with are the objects of scientific discovery and investigation, such as causal laws. Realism about such entities will be seen to entail particular realist positions in the theory of perception and universals, but not to be reducible to them.

Only transcendental realism, I will argue, can sustain the idea of a law-governed world independent of man; and it is this concept, I will argue, that is necessary to understand science.

Classical empiricism can sustain neither transitive nor intransitive dimensions; so that it fails both the criteria of adequacy (1)′ and (2)′ advanced on page 18 above. Moreover in its most consistent forms it involves both solipsism and phenomenalism; so that neither (1) nor (2) can be upheld. In particular not even the idea of the independence of the event from the experience that grounds it, i.e. the intransitivity of events, can be sustained; and, in the last instance, events must be analysed as sensations or in terms of what is epistemologically equivalent, viz. human operations.

Transcendental idealism attempts to uphold the objectivity (intersubjectivity) of facts, i.e. (1). And, if given a dynamic gloss, it can allow a transitive dimension and satisfy criterion (1)′; so that, in this respect, it is an improvement on empiricism. According to such a dynamized transcendental idealism knowledge is given structure by a sequence of models, rather than a fixed set of a priori rules. However in neither its static nor its dynamic form can it sustain the intransitive dimension. For in both cases the objects of which knowledge is obtained do not exist independently of human activity in general. And if there are things which do (things-in-themselves), no scientific knowledge of them can be obtained.

Both transcendental realism and transcendental idealism reject the empiricist account of science, according to which its valid content is exhausted by atomistic facts and their conjunctions. Both agree that there could be no knowledge without the social activity of science. They disagree over whether in this case there would be no nature also. Transcendental realism argues that it is necessary to assume for the intelligibility of science that the order discovered in nature exists independently of men, i.e. of human activity in general. Transcendental idealism maintains that this order is actually imposed by men in their cognitive activity. Their differences should thus be clear. According to transcendental realism, if there were no science there would still be a nature, and it is this nature which is investigated by science. Whatever is discovered in nature must be expressed in thought, but the structures and constitutions and causal laws discovered in nature do not depend upon thought. Moreover, the transcendental realist argues, this is not just a dogmatic metaphysical belief; but rather a philosophical position presupposed by key aspects of the social activity of science, whose intelligibility the transcendental idealist cannot thus, anymore than the empiricist, sustain.

Neither classical empiricism nor transcendental idealism can sustain the idea of the independent existence and action of the causal structures and things investigated and discovered by science. It is in their shared ontology that the source of this common incapacity lies. For although transcendental idealism rejects the empiricist account of science, it tacitly takes over the empiricist account of being. This ontological legacy is expressed most succintly in its commitment to empirical realism, and thus to the concept of the '*empirical world*'. For the transcendental realist this concept embodies a sequence of related philosophical mistakes. The first consists in the use of the category of experience to define the world. This involves giving what is in effect a particular epistemological concept a general ontological function. The second consists in the view that its being experienced or experienciable is an essential property of the world; whereas it is more correctly conceived as an accidental property of some things, albeit one which can, in special circumstances, be of great significance for science. The third thus consists in the neglect of the (socially produced) circumstances under which experience is in fact epistemically significant in science.

If the bounds of the real and the empirical are co-extensive then of course any 'surplus-element' which the transcendental idealist finds in the analysis of law-like statements cannot reflect a real difference between necessary and accidental sequences of events. It merely reflects a difference in men's attitude to them. Saying that light travels in straight lines ceases then to express a proposition about the world; it expresses instead a proposition about the way men understand it. Structure becomes a function of human needs; it is denied a place in the world of things. But just because of this, I shall argue, the transcendental idealist cannot adequately describe the principles

according to which our theories are constructed and empirically tested; so that the *rationality* of the transitive process of science, in which our knowledge of the world is continually extended and corrected, cannot be sustained.

To say that the weaknesses of both the empiricist and idealist traditions lie in their commitment to empirical realism is of course to commit oneself to the impossibility of ontological neutrality in an account of science; and thus to the impossibility of avoiding ontological questions in the philosophy of science. The sense in which every account of science presupposes an ontology is the sense in which it presupposes a schematic answer to the question of what the world must be like for science to be possible. Thus suppose a philosopher holds, as both empiricists and transcendental idealists do, that a constant conjunction of events apprehended in sense-experience is at least a necessary condition for the ascription of a causal law and that it is an essential part of the job of science to discover them. Such a philosopher is then committed to the belief that, given that science occurs, there are such conjunctions. As Mill put it, that 'there are such things in nature as parallel cases; that what happens once will, under a sufficient degree of similarity of circumstance, happen again'.[7]

There are two important points to register about such ontological beliefs and commitments. The first is that they should only be interpreted hypothetically, viz. as entailing what must be the case for science to be possible; on which interpretation it is a contingent fact that the world is such that science can occur. It is only in this relative or conditional sense that an account of science presupposes an ontology. The status of propositions in ontology may thus be described by the following formula: It is not necessary that science occurs. But given that it does, it is necessary that the world is a certain way. It is contingent that the world is such that science is possible. And, given that it is possible, it is contingent upon the satisfaction of certain social conditions that science in fact occurs. But given that science does or could occur, the world *must* be a certain way. Thus, the transcendental realist asserts, that the world is structured and differentiated can be established by philosophical argument; though the particular structures it contains and the ways in which it is differentiated are matters for substantive scientific investigation. The necessity for categorical distinctions between structures and events and between open systems and closed are indices of the stratification and differentiation of the world, i.e. of the transcendental realist philosophical ontology. These distinctions are presupposed, it will be shown, by the intelligibility of experimental activity. Whenever there is any danger of confusion between an 'ontology' in the sense of the kind of world presupposed by a philosophical account of science and in the sense of the particular entities and processes postulated by some substantive scientific theory I shall explicitly distinguish between a philosophical and a scientific ontology.

The second point to stress is that propositions in ontology cannot be

established independently of an account of science. On the contrary, they can only be established by reference to such an account, or at least to an account of certain scientific activities. However, it will be contended that this essential order of analysis, viz. science → being, *reverses* the real nature of dependency (or, we could say, the real burden of contingency). For it is not the fact that science occurs that gives the world a structure such that it can be known by men. Rather, it is the fact that the world has such a structure that makes science, whether or not it actually occurs, possible. That is to say, it is not the character of science that imposes a determinate pattern or order on the world; but the order of the world that, under certain determinate conditions, makes possible the cluster of activities we call 'science'. It does not follow from the fact that the nature of the world can only be *known* from (a study of) science, that its nature is *determined* by (the structure of) science. Propositions in ontology, i.e. about being, can only be established by reference to science. But this does not mean that they are disguised, veiled or otherwise elliptical propositions about science. What I shall characterize in a moment as the '*epistemic fallacy*' consists in assuming that, or arguing as if, they are.

3. The transcendental analysis of experience

The empiricist ontology is constituted by the category of experience. What transcendental arguments can be produced to show its inadequacy to science; and, on the other hand, to demonstrate the intransitivity and structured character of the objects of scientific knowledge? Now the occurrence of experience in science would be agreed upon by all three combatants. Moreover, it is generally assumed that, whatever its other inadequacies, empiricism can at least do justice to the role of experience in science. Now I want to argue that the intelligibility of experience in science itself presupposes the intransitive and structured character of the objects to which, in scientific experience, 'access' is obtained. This establishes the inadequacy, in its most favoured case, of the empiricist ontology. Further I want to argue that, in virtue of their shared ontological commitment, neither empiricism nor transcendental idealism can reveal the true significance of experience in science.

Scientifically significant experience normally depends upon experimental activity as well as sense-perception; that is, upon the role of men as causal agents as well as perceivers. I will consider the two independently.

A. The analysis of perception

The intelligibility of sense-perception presupposes the intransitivity of the object perceived. For it is in the independent occurrence or existence of such objects that the meaning of 'perception', and the epistemic significance of perception, lies. Among such objects are events, which must thus be categorically independent of experiences. Many arguments have been and could be

deployed to demonstrate this, which there is no space here to rehearse. For our purposes, it is sufficient merely to note that both the possibility of scientific change (or criticism) and the necessity for a scientific training presuppose the intransitivity of some real objects; which, for the empirical realist at least, can only be objects of perception. If changing experience of objects is to be possible, objects must have a distinct being in space and time from the experiences of which they are the objects. For Kepler to see the rim of the earth drop away, while Tycho Brahe watches the sun rise, we must suppose that there is something that they both see (in different ways).[8] Similarly when modern sailors refer to what ancient mariners called a sea-serpent as a school of porpoises, we must suppose that there is something which they are describing in different ways.[9] The intelligibility of scientific change (and criticism) and scientific education thus presupposes the ontological independence of the objects of experience from the objects of which they are the experiences. Events and momentary states do not of course exhaust the objects of perception. Indeed, I do not think they are even the primary objects of perception, which are probably processes and things, from which events and states are then 'reconstructed'.[10] However I do not wish to argue the point here – as it depends upon a prior resolution of the problems of causality and induction, upon which their status as objects of experience must, at least for the empiricist, depend.[11]

Events then are categorically independent of experiences. There could be a world of events without experiences. Such events would constitute *actualities* unperceived and, in the absence of men, unperceivable. There is no reason why, given the possibility of a world without perceptions, which is presupposed by the intelligibility of actual scientific perceptions, there should not be events in a world containing perceptions which are unperceived and, given our current or permanent capacities, unperceivable. And of such events theoretical knowledge may or may not be possessed, and may or may not be achievable. Clearly if at some particular time I have no knowledge of an unperceived or unperceivable event, I cannot say that such an event occurred (as a putative piece of substantive knowledge). But that in itself is no reason for saying that such an occurrence is impossible or that its supposition is meaningless (as a piece of philosophy). To do so would be to argue quite illicitly from the current state of knowledge to a philosophical conception of the world. Indeed, we know from the history of science that at any moment of time there are types of events never imagined, of which theoretical, and sometimes empirical, knowledge is eventually achieved. For in the transitive process of science the possibilities of perception, and of theoretical knowledge, are continually being extended. Thus unless it is dogmatically postulated that our present knowledge is complete or these possibilities exhausted, there are good grounds for holding that the class of unknowable events is non-empty, and unperceivable ones non-emptier; and no grounds for supposing that this will ever not be so.

Later, I will show how the domain of actualities, whose categorical independence from experiences is presupposed by the intelligibility of sense-perception, may be extended to include things as well as events.

B. The analysis of experimental activity

The intelligibility of experimental activity presupposes not just the intransitivity but the structured character of the objects investigated under experimental conditions. Let me once again focus on the empiricist's favourite case, viz. causal laws, leaving aside for the moment such other objects of investigation as structures and atomic constitutions. A causal law is analysed in empiricist ontology as a constant conjunction of events perceived (or perceptions). Now an experiment is necessary precisely to the extent that the pattern of events forthcoming under experimental conditions would not be forthcoming without it. Thus in an experiment we are a causal agent of the sequence of events, but not of the causal law which the sequence of events, because it has been produced under experimental conditions, enables us to identify.

Two consequences flow from this. First, the real basis of causal laws cannot be sequences of events; there must be an ontological distinction between them. Secondly, experimental activity can only be given a satisfactory rationale if the causal law it enables us to identify is held to prevail outside the contexts under which the sequence of events is generated. In short, the intelligibility of experimental activity presupposes that a constant conjunction is no more a necessary than a sufficient condition for a causal law. And it implies that causal laws endure and continue to operate in their normal way under conditions, which may be characterized as '*open*', where no constant conjunction or regular sequence of events is forthcoming. It is worth noting that in general, outside astronomy, *closed systems*, viz. systems in which constant conjunctions occur, must be experimentally established.

Both Anscombe and von Wright have recently made the point that our active *interference* in nature is normally a condition of empirical regularities.[12] But neither have seen that it follows from this that there must be an *ontological* distinction between the empirical regularity we produce and the causal law it enables us to identify. Although it has yet to be given an adequate philosophical rationale, the distinction between causal laws and patterns of events is consistent with our intuitions. Thus supposing a nuclear explosion were to destroy our planet no-one would hold that it violated, rather than exemplified, Newton's laws of motion;[13] just as if something were to affect Mercury's perihelion it would not be regarded as falsifying Einstein's theory of relativity. Similarly it lies within the power of every reasonably intelligent schoolboy or moderately clumsy research worker to upset the results of even the best designed experiment,[14] but we do not thereby suppose they have the power to overturn the laws of nature. I can

quite easily affect any sequence of events designed to test say Coulomb's or Guy-Lussac's law; but I have no more power over the relationships the laws describe than the men who discovered them had. In short, laws *cannot* be the regularities that constitute their empirical grounds.

Thus the intelligibility of experimental activity presupposes the categorical independence of the causal laws discovered from the patterns of events produced. For, to repeat, in an experiment we produce a pattern of events to identify a causal law, but we do not produce the causal law identified. Once the categorical independence of causal laws and patterns of events is established, then we may readily allow that laws continue to operate in open systems, where no constant conjunctions of events prevail. And the rational explanation of phenomena occurring in such systems becomes possible.

In a world without men there would be no experiences and few, if any, constant conjunctions of events, i.e. had they been experienced Humean 'causal laws'. For both experiences and invariances (constant conjunctions of events) depend, in general, upon human activity. But causal laws do not. Thus in a world without men the causal laws that science has now as a matter of fact discovered would continue to prevail, though there would be few sequences of events and no experiences with which they were in correspondence. Thus, we can begin to see how the empiricist ontology in fact depends upon a concealed *anthropocentricity.*

The concept of causal laws being or depending upon empirical regularities involves thus a double identification: of events and experiences; and of constant conjunctions (or regular sequences) of events and causal laws. This double identification involves two category mistakes, expressed most succinctly in the concepts of the empirical world and the actuality of causal laws. The latter presupposes the ubiquity of closed systems. Both concepts, I shall argue, are profoundly mistaken and have no place in any philosophy of science. This double identification prevents the empirical realist from examining the important question of the conditions under which experience is in fact significant in science. In general this requires both that the perceiver be theoretically informed[15] and that the system in which the events occur be closed.[16] Only under such conditions can the experimental scientist come to have access to those underlying causal structures which are the objects of his theory. And not until the categorical independence of causal laws, patterns of events and experiences has been philosophically established and the possibility of their disjuncture thereby posed can we appreciate the enormous effort – in experimental design and scientific training – that is required to make experience epistemically significant in science.

The intelligibility of experimental activity presupposes then the intransitive and structured character of the objects of scientific knowledge, at least in so far as these are causal laws. And this presupposes in turn the possibility of a non-human world, i.e. causal laws *without* invariances and experiences, and in particular of a non-empirical world, i.e. causal laws and events without

experiences; and the possibility of *open systems*, i.e. causal laws *out of phase* with patterns of events and experiences, and more generally of epistemically insignificant experiences, i.e. experiences out of phase with events and/or causal laws.

In saying that the objects of scientific discovery and investigation are 'intransitive' I mean to indicate therefore that they exist independently of all human activity; and in saying that they are 'structured' that they are distinct from the patterns of events that occur. The causal laws of nature are not empirical statements, i.e. statements about experiences; nor are they statements about events; nor are they synthetic a priori statements. For the moment I merely style them negatively as 'structured intransitive', postponing a positive analysis of them until §5.

4. The status of ontology and its dissolution in classical philosophy

This analysis of experimental episodes enables us to isolate a series of metaphysical, epistemological and methodological mistakes within the tradition of empirical realism. For if the intelligibility of experimental activity entails that the objects of scientific understanding are intransitive and structured then we can establish at one stroke: (i) that a philosophical ontology is possible; (ii) some propositions in it (causal laws are distinct from patterns of events, and events from experiences); and (iii) the possibility of a philosophy which is consistent with (and has some relevance for), i.e. which is itself 'in phase with', the realist practice of science. Ontology, it should be stressed, does not have as its subject matter a world apart from that investigated by science. Rather, its subject matter just is that world, considered from the point of view of what can be established about it by philosophical argument. The idea of ontology as treating of a mysterious underlying physical realm, which owes a lot to Locke and some of his rationalist contemporaries (particularly Leibniz), has done much to discredit it; and to prevent metaphysics from becoming what it ought to be, viz. a conceptual science. Philosophical ontology asks what the world must be like for science to be possible; and its premises are generally recognized scientific activities. Its method is transcendental; its premise science; its conclusion the object of our present investigation.

The metaphysical mistake the argument of the previous section allows us to pinpoint may be called the 'epistemic fallacy'. This consists in the view that statements about being can be reduced to or analysed in terms of statements about knowledge; i.e. that ontological questions can always be transposed into epistemological terms. The idea that being can always be analysed in terms of our knowledge of being, that it is sufficient for philosophy to 'treat only of the network, and not what the network describes',[17] results in the systematic dissolution of the idea of a world (which I shall here

metaphorically characterize as an ontological realm) independent of but investigated by science. And it is manifest in the prohibition on any transcendent entities. It might be usefully compared with the naturalistic fallacy in moral philosophy. For just as the naturalistic fallacy prevents us from saying what is good about e.g. maximizing utility in society, so the epistemic one prevents us from saying what is epistemically significant about e.g. experience in science. To show that it is a fallacy and to trace its effects are two of the principal objectives of this study. In showing that the intelligibility of experimental activity entails that the objects of scientific knowledge, in so far as they are causal laws, are intransitive I have already succeeded in the first of these aims. For this means that a statement of a causal law cannot now be reduced to or analysed in terms of a statement about anyone's knowledge of it or knowledge in general. On the contrary, its assertion now entails that a causal law would operate even if unknown, and even if there were no-one to know it. So that knowledge ceases to be, as it were, an essential predicate of things.

The epistemic fallacy is most marked, perhaps, in the concept of the empirical world. But it is manifest in the criteria of significance and even the problems associated with the tradition of empirical realism. Kant committed it in arguing that the categories 'allow only of empirical employment and have no meaning whatsoever when not applied to objects of possible experience; that is to the world of sense.'[18] (For us on the other hand if the Kantian categories were adequate to the objects of scientific thought then they would continue to apply in a world without sense, and have a meaning in relation to that possibility.) Similarly, the logical positivists committed it when arguing, in the spirit of Hume, that if a proposition was not empirically verifiable (or falsifiable) or a tautology, it was meaningless.[19] Verificationism indeed may be regarded as a particular form of the epistemic fallacy, in which the meaning of a proposition about reality (which cannot be designated 'empirical') is confused with our grounds, which may or may not be empirical, for holding it. Once this doctrine is rejected there is no need to identify the necessary and the *a priori*, and the contingent and the *a posteriori*; or, to put it another way, one can distinguish between natural and logical necessity, and between natural and epistemic possibility. Further there is no need to assume that the order of dependence of being must be the same as the order of dependence of our knowledge of being. Thus we can allow that experience is in the last instance epistemically decisive, without supposing that its objects are ontologically ultimate, in the sense that their existence depends upon nothing else. Indeed if science is regarded as a continuing process of discovery of ever finer and in an explanatory sense more basic causal structures, then it is rational to assume that what is at any moment of time least certain epistemically speaking is most basic from the ontological point of view.[20] More generally, the epistemic fallacy is manifest in a persistent tendency to read the conditions of a particular concept of knowledge into an implicit concept of the world. Thus the problem of induction is a

consequence of the atomicity of the events conjoined, which is a function of the necessity for an epistemically certain base.

Although the epistemic fallacy is of most interest to us as it is manifest in the tradition of empirical realism, it is worth mentioning that a philosopher who rejected empirical realism might still commit the epistemic fallacy, i.e. analyse being in terms of knowledge, if, as in some varieties of Platonism and rationalism, he were to define the world in terms of the possibility of non-empirical knowledge of it. For the transcendental realist it is not a necessary condition for the existence of the world that science occurs. But it is a necessary condition for the occurrence of science that the world exists and is of a certain type. Thus the possibility of our knowing it is not an essential property, and so cannot be a defining characteristic, of the world. Rather on a cosmic scale, it is an historical accident; though it is only because of this accident that we can establish in science the way the world is, and in philosophy the way it must be for science to be possible.

The view that statements about being can be reduced to or analysed in terms of statements about knowledge might be defended in the following way: ontology is dependent upon epistemology since what we can know to exist is merely a part of what we can know.[21] But this defence trades upon a tacit conflation of philosophical and scientific ontologies. For if 'what we can know to exist' refers to a possible content of a scientific theory than that it is merely a part of what we can know is an uninteresting truism. But a philosophical ontology is developed by reflection upon what must be the case for science to be possible; and this is independent of any actual scientific knowledge. Moreover, it is not true, even from the point of view of the immanent logic of a science, that what we can know to exist is just a part of what we can know. For a law may exist and be known to exist without our knowing the law. Much scientific research has in fact the same logical character as detection. In a piece of criminal detection, the detective knows that a crime has been committed and some facts about it but he does not know, or at least cannot yet prove, the identity of the criminal.

To be is *not* to be the value of a variable;[22] though it is plausible (if, I would argue, incorrect) to suppose that things can only be *known* as such. For if to be were just to be the value of a variable we could never make sense of the complex processes of identification and measurement by means of which we can sometimes represent some things as such. Knowledge follows existence, in logic and in time; and any philosophical position which explicitly or implicitly denies this has got things upside down.

The metaphysical mistake the analysis of experimental episodes pinpoints, viz. the epistemic fallacy, involves the denial of the possibility of a philosophical ontology. But if transcendental realism is correct, and ontology cannot in fact be reduced to epistemology, then denying the possibility of an ontology merely results in the generation of an *implicit ontology* and an *implicit realism.* In the empirical realist tradition the epistemic fallacy thus covers or

disguises an ontology based on the category of experience, and a realism based on the presumed characteristics of the objects of experiences, viz. atomistic events, and their relations, viz. constant conjunctions. (Such presumptions can, I think, only be explained in terms of the needs of a justificationist epistemology, e.g. for incorrigible foundations of knowledge.) This in turn leads to the generation of a methodology which is either consistent with epistemology but of no relevance to science; or relevant to science but more or less radically inconsistent with epistemology. So that, in short, philosophy itself is 'out of phase' with science. Let us see how this happens.

First, the general line of Hume's critique of the possibility of any philosophical ontology or account of being, and in particular his denial that we can philosophically establish the independent existence of things or operation of natural necessities, is accepted. Now it is important to see what Hume has in fact done. He has not really succeeded in banishing ontology from his account of science. Rather he has replaced the Lockean ontology of real essences, powers and atomic constitutions with his own ontology of impressions. To say that every account of science, or every philosophy in as much as it is concerned with 'science', presupposes an ontology is to say that the philosophy of science abhors an ontological vacuum. The empiricist fills the vacuum he creates with his concept of experience. In this way an implicit ontology, crystallized in the concept of the empirical world, is generated. And it is this ontology which subsequent philosophers of science have uncritically taken over. For whether they have agreed with Hume's epistemology or not, they have accepted his critique of ontology, which contains its own implicit ontology, as valid.

Let us examine the generation of this implicit ontology in greater detail. In Hume's positive analysis of perception and causality experiences constituting atomistic events and their conjunctions are seen as exhausting our knowledge of nature. Now, adopting a realist meta-perspective this means that such events and their conjunctions must occur in nature, if science is to be possible. But from Hume onwards the sole question in the philosophy of science is whether our knowledge is exhausted by our knowledge of such events and their conjunctions; it is never questioned whether they in fact occur. That is, philosophy's concern is with whether our knowledge of the world can be reduced to sense-experience as so conceived or whether it must include an a priori or theoretical component as well; not with whether experience can adequately constitute the world.

But in Humean empiricism two things are done. First, knowledge is reduced to that of atomistic events apprehended in sense-experience. Secondly, these events are then identified as the particulars of the world. In this way our knowledge of reality is literally identified, or at best taken to be in isomorphic correspondence, with the reality known by science. From Hume onwards philosophers have thus allowed, for the sake of avoiding ontology, a particular concept of our knowledge of reality, which they may

wish to explicitly reject, to inform and implicitly define their concept of the reality known by science. The result has been a continuing *'ontological tension'* induced by the conflict between the rational intuitions of philosophers about science and the constraints imposed upon their development by their inherited ontology. This has led to a nexus of interminably insoluble problems, such as how we can reason from one experience to another, and to a displacement of these rational intuitions whereby, for example, the locus of necessity is shifted from the objective necessity of the natural world to the subjective necessity of causally-determined or the inter-subjective necessity of rule-governed minds.

Now if transcendental realism is true, and scientists act as if the objects of their investigation are intransitive and structured, then any adequate methodology must be consistent with the realist practice of science, and so inconsistent with the epistemology of empirical realism. It is instructive to look at Hume here. One finds in the *Treatise* an eminently sensible realist methodology in almost total dislocation from, and certainly lacking any foundation in, his radical epistemology. Thus one might be forgiven for wondering what has become of his phenomenalism and the doctrine of impressions when Hume allows that the 'understanding corrects the appearances of the senses'.[23] Or what has happened to the idea of the contingency of the causal connection and the problem of induction when he argues that scientists, when faced with exceptions to established generalizations, quite properly search for the 'secret operation of contrary causes' rather than postulate an upset in the uniformity of nature.[24] This is typical. There is a similar dislocation between Kant's *Critique of Pure Reason* and his *Metaphysical Foundations of Natural Science.*

It might be argued in defence of Hume that he is concerned to show that our realist intuitions cannot be justified; that his point is precisely that there is a dislocation between what can be shown and what must be believed (that 'there is a direct and total opposition twixt our reason and our senses');[25] and that he leaves the latter intact. But the matter is not so simple as this. Humean empiricism is not neutral in its consequences for scientific practice. Taken consistently, it does generate a methodology; not indeed Hume's (or Newton's), but Mach's. For in the absence of the concept of an ontological realm, the implicit realism generated implies that whatever is experienced in sense-experience is an event and whatever constant conjunctions are experienced are causal laws. In this way, our current knowledge fills the vacuum left by the dissolution of the ontological realm; and in so doing it squeezes out, metaphorically speaking, the possibility of any substantive scientific criticism. In the methodology of Humean empiricism facts, which are social products, usurp the place of the particulars of the world; and their conjunctions, which are doubly social products (once qua fact, once qua event-conjunction), the place of causal laws. The result is the generation of a conservative ideology which serves to rationalize the practice of what

Kuhn has called 'normal science'.[26] Descriptivist, instrumentalist and fictionalist interpretations of theory do not do away with e.g. scientific laws, but by reducing their ontological import to a given self-certifying experience, they serve to exempt our current claims to knowledge of them from criticism.

It is thus quite incorrect to suppose that realist as opposed to non-realist interpretations of scientific theory have consequences for science which are in practice more dogmatic;[27] or to suppose that the concept of natural necessity is a kind of survival from the bad old days of scientific certainty.[28] On the contrary, the converse is the case. For it is only if the working scientist possesses the concept of an ontological realm, distinct from his current claims to knowledge of it, that he can philosophically think out the possibility of a rational criticism of these claims. To be a fallibilist about knowledge, it is necessary to be a realist about things. Conversely, to be a sceptic about things is to be a dogmatist about knowledge.

Now it is not only the doctrine of empirical realism, and philosophers' uncritical acceptance of it, that accounts for the ontological tension within philosophy and the dislocation of epistemology from methodology, of philosophy from science. It must be accounted for in part by the conditions of science, as well as philosophy. For the period in which Humean ontology became embedded in philosophy (1750–1900) was, at least in physics, a period of scientific consolidation rather than change. The role of philosophy was seen more and more to be that of showing how our knowledge is justified as distinct from showing how it was produced, can be criticized and may come to be changed. Thus whereas transcendental realism asks explicitly what the world must be like for science to be possible, classical philosophy asked merely what science would have to be like for the knowledge it yielded to be justified. It was presumed that our knowledge was justified; science was not viewed as a process in motion; and doing away with ontology left philosophy without any critical purchase on science. The transcendental realist, on the other hand, allows a limited critical role for philosophy. For by restoring the idea of an ontological realm distinct from science, he makes it possible for us to say that in a particular field, say social psychology, science is not being done, although as a philosopher he cannot say dogmatically whether or not a science of social psychology is possible.[29] (An ontological dimension is in this way necessary not only to render intelligible scientific criticism, but to make possible philosophical criticism of the practice of a science.) Increasingly then it was the logical structure of justificatory argument that defined philosophy's concept of science; and the philosophy of science itself became a kind of battleground for internecine warfare between opposed concepts of justified belief. Moreover, when the idea of scientific certainty eventually collapsed, the absence of an ontological dimension discouraged anything other than a purely voluntaristic reaction – in which it was supposed that because our beliefs about the world were not causally determined by the

world then they must be completely 'free creations of our own minds, the result of an almost poetic intuition'.[30]

Behind this state of affairs there ran a stong *anthropocentric* current in classical and subsequent philosophy,[31] which sought to rephrase questions about the world as questions about the nature or behaviour of men. One aspect of this is the view, which I have characterized as the epistemic fallacy, that ontological questions can always be rephrased as epistemological ones. The anthropocentric and epistemic biases of classical philosophy led to the dissolution of the concept of the ontological realm, which we need to render intelligible the transitive process of science. In this way the world, which ought to be viewed as a multi-dimensional structure independent of man, came to be squashed into a flat surface whose characteristics, such as being constituted by atomistic facts, were determined by the needs of a particular concept of knowledge. This led to a barrage of problems and an impossible account of science. For from now on any structure, if it was allowed at all, had to be located in the human mind or the scientific community. Thus the world was literally turned inside out in an attempt to confine it within sentience. An inevitable 'involution' in the philosophy of science occurred. Without a concept of a reality unknown, but at least in part knowable, philosophy could not display the creative and critical activity of science, and ceased to be of any practical relevance for it. This was the price paid for the dissolution of ontology. A philosophy for science depends upon its reconstitution.

5. Ontology vindicated and the real basis of causal laws

In §3 I argued that only if causal laws are not the patterns of events that enable us to identify them can the intelligibility of experimental activity be sustained. But causal laws are, or have seemed to philosophers to be, pretty mysterious entities. What can it mean to say that they have a real basis independent of events? The answer to this question will be seen to necessitate the development of a non-anthropocentric ontology of structures, generative mechanisms and active things.

The ontological status of causal laws can best be approached by considering the divergent responses of transcendental realism and idealism to the problem of distinguishing a necessary from a purely accidental sequence of events. Both may agree, in their modern versions, that without some conception of a generative mechanism at work no attribution of necessity is justified. For the transcendental idealist, however, this necessity is imposed by men on the pattern of events; the generative mechanism is an irreducible figment of the imagination. For the transcendental realist, on the other hand, the generative mechanism may come to be established as real in the course of the ongoing activity of science. Indeed he will argue that it is only

if existential questions can be raised about the objects of scientific theory that the rationality of theory construction can be sustained. For without them science would remain, as in empiricism, a purely internal process – with the familiarity of image replacing the reinforcement of sensation, still lacking a rational dynamic of change.

Now once it is granted that mechanisms and structures may be said to be real, we can provide an interpretation of the independence of causal laws from the patterns of events, and a fortiori of the rationale of experimental activity. For the real basis of this independence lies in the independence of the generative mechanisms of nature from the events they generate. Such mechanisms endure even when not acting; and act in their normal way even when the consequents of the law-like statements they ground are, owing to the operation of intervening mechanisms or countervailing causes, unrealized. It is the role of the experimental scientist to exclude such interventions, which are usual; and to trigger the mechanism so that it is active. The activity of the mechanism may then be studied without interference. And it is this characteristic pattern of activity or mode of operation that is described in the statement of a causal law. It is only under closed conditions that there will be a one-to-one relationship between the causal law and the sequence of events. And it is normally only in the laboratory that these enduring mechanisms of nature, whose operations are described in the statements of causal laws, become actually manifest and empirically accessible to men. But because they endure and continue to act, when stimulated, in their normal way outside those conditions, their use to explain phenomena and resistance to pseudo-falsification in open systems can be rationally justified.

Only if causal laws persist through, which means they must be irreducible to, the flux of conditions can the idea of the universality of a *known* law be sustained. And only if they have a reality distinct from that of events can the assumption of a *natural* necessity be justified. On this view laws are not empirical statements, but statements about the forms of activity characteristic of the things of the world. And their necessity is that of a natural connection, not that of a human rule. There is a distinction between the *real* structures and mechanisms of the world and the *actual* patterns of events that they generate. And this distinction in turn justifies the more familiar one between *necessary* and *accidental* sequences. For a necessary sequence is simply one which corresponds to, or is in phase with, a real connection; that is, it is a real connection actually manifest in the sequence of events that occurs.

The world consists of mechanisms not events. Such mechanisms combine to generate the flux of phenomena that constitute the actual states and happenings of the world. They may be said to be real, though it is rarely that they are actually manifest and rarer still that they are empirically identified by men. They are the intransitive objects of scientific theory. They are quite independent of men – as thinkers, causal agents and perceivers. They are not unknowable, although knowledge of them depends upon a rare blending of

intellectual, practico-technical and perceptual skills. They are not artificial constructs. But neither are they Platonic forms. For they can become manifest to men in experience. Thus we are not imprisoned in caves, either of our own or of nature's making. We are not doomed to ignorance. But neither are we spontaneously free. This is the arduous task of science: the production of the knowledge of those enduring and continually active mechanisms of nature that produce the phenomena of our world.

Objections may be made to my proposed reconstitution of an ontological realm, which question in turn the intransitivity and the structured character of the postulated objects of scientific inquiry, i.e. the ideas of their categorical independence from men and events respectively. I will consider the two kinds of objections in turn.

Thus, it might be objected that the very idea of a world without men is unintelligible because the conditions under which it is true would make its being conceived impossible. But I can think of a world without men; and I can think of a world without myself. No-one can truly say 'I do not exist' but that does not mean that 'I do not exist' is unintelligible; or that it cannot be meaningfully, just because it cannot be truly said. It is no objection to the intelligibility of a statement that it is counter-factual. Indeed it is only because it is intelligible that we can say that it is counter-factual.

Someone might hold that to think of a world without men is not so much unintelligible as impossible; that we must picture ourselves in any picture. Now it is a fact about human beings that we can do this. But we do not have to do it, any more than an artist must initial his work. The idea may be perhaps that a thought must always contain, or at least be accompanied by, a thought of the thinker of the thought thinking the thought. Clearly if this were so, an infinite regress would be impossible to avoid. However, to be aware of the fact that I am thinking of a particular topic x, it is not necessary for me to be thinking of that fact. Such awareness may be expressed in thought, but when it is the topic is no longer x but my thought of x. It is possible for A to think ε and to be aware of thinking ε without thinking about thinking ε; and unless this were so no-one could ever intelligently think. Moreover it is possible for A to think about thinking ε without thinking about his (A's) thinking ε. Thinking about thinking about a particular topic must be distinguished from thinking about the thinker of the topic.[32]

There is no absurdity in the supposition of a world without men. Rather it is a possibility presupposed by the social activity of science. It is important to establish this fact. For we are too liable to underestimate the power of the pictures, often unconscious, which underpin philosophical theories. Such pictures indeed often hold our philosophical imagination 'captive'.[33] Our philosophy of science is heavily anthropocentric, which is why it is important to consider what it would be possible to say about our world if there were no men, given that we know that our world is one in which science is as a

matter of fact possible. For example things would still act, be subject to laws and preserve their identity through certain changes.

A second kind of objection might focus on the structured character of the postulated objects of scientific inquiry, questioning not so much the idea itself but the interpretation I have given to it; and in particular the explanatory value of the particular ontology proposed. Thus it might be objected that, while the transcendental argument from experimental activity in §3 establishing the distinctiveness of causal laws and patterns of events, is sound, the introduction of the concept of generative mechanisms to provide a real basis for causal laws is gratuitous.

What does it mean to say that a generative mechanism endures and acts in its characteristic way? It does not *mean,* we have seen, that a regular sequence of events occurs or is experienced; though the occurrence of such a sequence may, in special circumstances, provide empirical *grounds* for the hypothesis of the existence of the mechanism. For the intelligibility of experimental activity entails that the particular mechanism endures and at least some mechanisms act through the flux of conditions that determine whether they are active and co-determine the manifest outcome of their activity. That is to say, it entails that generative mechanisms endure even when inactive and act even where, as in open systems, there is no one-to-one relationship between the causal law representing the characteristic mode of operation of the mechanism and the particular sequence of events that occurs. In particular, it entails that mechanisms act in their normal way outside the closed conditions that enable us to experimentally identify them and whether or not we do so; i.e. whether or not the results of their operations are modified, and whether or not these results are perceived by men. (In the former case we could talk of a disjuncture between the domains of the real and the actual; in the latter case of a disjuncture between the domains of the real and the empirical.)

Now the reason why the concept of a causal law cannot itself be taken as ontologically basic is because its analysis presupposes a 'real something' over and above and independent of patterns of events; and it is for the status of this real something that the concept of a generative mechanism is groomed. But then does to say that a generative mechanism endures and acts in its characteristic way mean anything more than to say that a thing goes on acting in a certain way? As stated the reformulation is ambiguous. For the continuance of a form or pattern of activity can be interpreted in an empirical or a non-empirical way. The intelligibility of experimental activity requires the latter non-empirical interpretation. For it entails, as we have seen, that causal laws persist and are efficacious in open systems, i.e. outside the conditions that enable us to empirically identify them. Now accepting this non-empirical interpretation means that reference to causal laws involves centrally reference to *causal agents*; that is, to things endowed with causal powers. On this interpretation then the generative mechanisms of nature

exist as the causal powers of things. We now have a perfectly acceptable ontological basis for causal laws. For if it is wrong to reify causal laws, and it is wrong to reify generative mechanisms, it cannot be wrong to reify things! However, the fact that the transcendental analysis of experimental activity showed that generative mechanisms must go on acting (i.e. that causal laws must be efficacious) outside the closed conditions that permit their identification means that causal laws cannot be simply analysed as powers. Rather they must be analysed as tendencies. For whereas powers are potentialities which may or may not be exercised, tendencies are potentialities which may be exercised or as it were 'in play' without being realized or manifest in any particular outcome. They are therefore just right for the analysis of causal laws.[34]

If the analysis of causal laws (and generative mechanisms) is to be given by the concept of things and not events (a possibility which I have already rejected by demonstrating in §3 their categorical independence from events), the consideration that they not only persist but are efficacious in open systems, which is presupposed by the intelligibility of experimental activity, entails that causal laws must be analysed as tendencies. For tendencies are powers which may be exercised without being fulfilled or actualized (as well as being fulfilled or actualized unperceived by men). It is by reference not just to the enduring powers but the unrealized activities or unmanifest (or incompletely manifest) actions of things that the phenomena of the world are explained. It is the idea of continuing activity as distinct from that of enduring power that the concept of tendency is designed to capture. In the concept of tendency, the concept of power is thus literally dynamized or set in motion.

In the full analysis of law-like statements we are thus concerned with a new kind of conditional: which specifies the exercise of possibilities which need not be manifest in any particular outcome. Such conditionals are *normic*,[35] rather than subjunctive. They do not say what would happen, but what is happening in a perhaps unmanifest way. Whereas a powers statement says A would ψ, in appropriate circumstances, a normic statement says that A really is ψ'ing, whether or not its actual (or perceivable) effects are counteracted. They are not counter-factuals, but *transfactuals*; they take us to a level at which things are really going on irrespective of the actual outcome. To invoke a causal law is to invoke a normic conditional. A full analysis of normic and tendency statements will be provided later. For the moment, it should be noted that normic statements provide the correct analysis of the normic indicative form. A normic statement is a transfactual statement, with actual instances in the laboratory that constitute its empirical grounds.

The world consists of things, not events. Most things are complex objects, in virtue of which they possess an ensemble of tendencies, liabilities and powers. It is by reference to the exercise of their tendencies, liabilities and powers that the phenomena of the world are explained. Such continuing

activity is in turn referred back for explanation to the essential nature of things. On this conception of science it is concerned essentially with what kinds of things they are and with what they tend to do; it is only derivatively concerned with predicting what is actually going to happen. It is only rarely, and normally under conditions which are artificially produced and controlled, that scientists can do the latter. And, when they do, its significance lies precisely in the light that it casts on the enduring natures and ways of acting of independently existing and transfactually active things.

There is nothing esoteric or mysterious about the concept of the generative mechanisms of nature, which provide the real basis of causal laws. For a generative mechanism is nothing other than a way of acting of a thing. It endures, and under appropriate circumstances is exercised, as long as the properties that account for it persist. Laws then are neither empirical statements (statements about experiences) nor statements about events. Rather they are statements about the ways of acting of independently existing and transfactually active things.

It is now possible to give a positive interpretation of our characterization in §3 of the objects of scientific investigation, at least in so far as they are causal laws, as 'structured intransitive'. 'Structured' in so far as it is the activities of mechanisms and causal structures, not the occurrence of events, that are designated in statements of causal law. 'Intransitive' in so far as the mechanisms and causal structures, whose activity is designated, endure and act quite independently of men. To discover the independently existing and transfactually active machinery of nature is not, it should be stressed, the aim of an independent inquiry of metaphysics. Rather, it is the end to which all the empirical efforts of science are directed. Ontology has been vindicated not as providing a set of necessary truths about a mysterious underlying physical realm, but as providing a set of conditionally necessary truths about our ordinary world as investigated by science. It is important to be clear about what philosophical argument can achieve. Thus as a piece of philosophy we can say (given that science occurs) that some real things and generative mechanisms must exist (and act). But philosophical argument cannot establish which ones actually do; or, to put it the other way round, what the real mechanisms are. That is up to science to discover. That generative mechanisms must exist and sometimes act independently of men and that they must be irreducible to the patterns of events they generate is presupposed by the intelligibility of experimental activity. But is up to actual experiments to tell us what the mechanisms of nature are. Here, as elsewhere, it is the task of philosophy to analyse notions which in their substantive employment have only a syncategorematic use. Thus whenever a scientist refers to a thing or event, structure or law, or says that something exists or acts in a certain way he must refer to it under some particular description; he is using the notion of thing, law, existence, etc. But it is the task of the philosopher to analyse the concept as such. To argue that this task is both

legitimate and necessary is not to populate the world with (or to suppose that there is a world of) things without names or events-in-general.

I am now in a position to tidy up my analysis of experimental activity. The experimental scientist must perform two essential functions in an experiment. First, he must trigger the mechanism under study to ensure that it is active; and secondly, he must prevent any interference with the operation of the mechanism. These activities could be designated 'experimental production' and 'experimental control'. The former is necessary to ensure the satisfaction of the antecedent (or stimulus) conditions, the latter to ensure the realization of the consequent, i.e. that a closure has been obtained. But both involve changing or being prepared to change the 'course of nature', i.e. the sequence of events that would otherwise have occurred.[36] In a simple electrical experiment designed to illustrate say Ohm's Law, the wiring of an electric circuit and the generation of an electric current would constitute 'experimental production'; maintaining the appropriate resistance levels, ensuring that no new magnetic field is suddenly placed in the neighbourhood of the circuit, etc. would then constitute 'experimental control'.

Only if the mechanism is active and the system in which it operates is closed can scientists in general record a unique relationship between the antecedent and consequent of a law-like statement. The aim of an experiment is to get a single mechanism going in isolation and record its effects. Outside a closed system these will normally be affected by the operations of other mechanisms, either of the same or of different kinds, too, so that no unique relationship between the variables or precise description of the mode of operation of the mechanism will be possible. In general, experimental activity requires a degree of plasticity of the antecedent (stimulus) and circumambient conditions to human manipulation and control. Such plasticity is not easily won. 'Experimental design' is a substantial theoretical labour in itself.

It has often been said, metaphorically speaking, that in an experiment we put a question to nature. But it has not been said that the question we put is a practical one – with our hands, so to speak. The weakness of previous analyses of experimental activity is that they have not appreciated the significance of the fact that conjunctions of phenomena have to be worked for practically (as well as in thought); that conjunctions are not given to, but *made by* us. In an important study, von Wright has seen this. But he has not drawn the correct conclusion from it: which is that, just because the experimenter is a causal agent of the sequence of events, there must be an ontological distinction between the sequence he generates and the causal law it enables him to identify. Any other conclusion renders experimental activity pointless. (Why generate that sequence?) The reason for von Wright's failure to see this stems from his unfortunate initial assumption of (as he puts it) a 'Tractatus-world', i.e. a world of logically independent atomistic states of

affairs (which astonishingly he seems to regard as a harmless simplification);[37] which precludes him from seeing laws as anything other than conditional statements about atomistic states of affairs. It is of course something of a scandal that empiricists who invoke experience as the sole ground of knowledge and scientific knowledge as their paradigm should not have undertaken an analysis of the conditions under which experience is significant in science. It should be stressed that the result that there is an ontological distinction between causal laws and patterns of events depends upon only two premises: (i) that men are causal agents capable of interfering with the course of nature and (ii) that experimental activity, the planned disruption of the course of nature, is a significant feature of science.

In stressing the practical component of experimental activity, it is important not to forget the theoretical side. In an experiment men put a question to nature. But they must put it in a language that nature understands, as well as in a form that makes possible an unambiguous reply. It is difficult to overestimate the importance for modern science of the development of instruments such as clocks and telescopes, which may be seen as devices designed to decipher the vocabulary of nature. Both the construction and the interpretation of such instruments depended upon theory. Hooke's law, for example, is literally built into the construction of spring balances.[38] Experimental confirmation of Galilean dynamics was delayed for a long time by the difficulty of measuring 'the most fundamental magnitude of dynamics', i.e. time. But when the Huyghens eventually succeeded in building such a clock in 1659 it was only by basing it on the new dynamics (the very dynamics it was designed to vindicate) and in particular the theory of the isochronous curve of the pendulum.[39] Similarly it has been convincingly argued that the development of cosmology in the early 17th century was held up by the absence of an adequate theory of telescopic vision.[40] In short, experimental activity depends crucially upon the adequacy of the theories (sometimes referred to as 'auxiliary') according to which the experimental equipment is constructed and its results interpreted.

Two problems are raised by my analysis of experimental activity. First, we know that much science, of what might be called a fundamental kind, has proceeded by way of 'thought' rather than by actual experiment. As Dijksterhuis has put it: 'In general one has to take stories about experiments by Galileo, as well as his opponents with some reserve. As a rule they were performed mentally, or they are merely described as possibilities.'[41] It seems that Einstein too was not averse to the occasional 'Gedankexperimente'.[42] This raises the question of whether, and if so how, pure thought can anticipate a law? And the problem of how, if it can, we then avoid the rationalist conclusion that provided only our axiom base is strong enough we could deduce all the laws of nature without recourse to experience. Secondly, we know that in many fields, most notably history and the human sciences and in the biological sciences in aspects of their work, experimental activity is

impossible. This raises the question of whether there are, or it is possible to devise for them, surrogates of the experimental establishment of closed systems in physics and chemistry? And here again there lurks an unacceptable rationalist implication. Both pose prima facie problems for transcendental realism, which I hope to be able to resolve at a later stage in this study.

6. A sketch of a critique of empirical realism

I have argued that the causal structures and generative mechanisms of nature must exist and act independently of the conditions that allow men access to them, so that they must be assumed to be structured and intransitive, i.e. relatively independent of the patterns of events and the actions of men alike. Similarly I have argued that events must occur independently of the experiences in which they are apprehended. Structures and mechanisms then are real and distinct from the patterns of events that they generate; just as events are real and distinct from the experiences in which they are apprehended. Mechanisms, events and experiences thus constitute three overlapping domains of reality, viz. the domains of the *real*, the *actual* and the *empirical.* This is represented in Table 1 below. The crux of my objection to the doctrine of empirical realism should now be clear. By constituting an ontology based on the category of experience, as expressed in the concept of the empirical world and mediated by the ideas of the actuality of the causal laws and the ubiquity of constant conjunctions, three domains of reality are collapsed into one. This prevents the question of the conditions under which experience is in fact significant in science from being posed; and the ways in which these three levels are brought into harmony or phase with one another from being described.

Table 1

	Domain of Real	*Domain of Actual*	*Domain of Empirical*
Mechanisms	√		
Events	√	√	
Experiences	√	√	√

Note. For transcendental realism $d_r \geqslant d_a \geqslant d_e$ . . . (i) where d_r, d_a, and d_e are the domains of the real, the actual and the empirical respectively.

For empirical realism $d_r = d_a = d_e$ . . . (ii).

Comment: (ii) is a special case of (i), which depends in general upon antecedent social activity, and in which

(a) for $d_a = d_e$ the events are known under epistemically significant descriptions, which depends upon skilled perception (and thus a skilled perceiver);

(b) for $d_r = d_a$ an antecedent closure has been obtained, which depends upon skilled experimentation (and thus the planned disruption of nature).

Now these three levels of reality are not naturally or normally in phase. It is the social activity of science which makes them so. Experiences, and the facts they ground, are social products; and the conjunctions of events, that, when apprehended in experience, provide the empirical grounds for causal laws, are, as we have seen, social products too. It can thus be seen that underlying and necessary for the implicit ontology of empirical realism is an *implicit sociology* in which facts and their conjunctions are seen as given by nature or spontaneously (voluntaristically) produced by men. In this chapter I have outlined an answer to the question 'what must the world be like for science to be possible?'. In Chapter 3 I will ask 'what must society be like for science to be possible?'; i.e. I shall attempt a transcendental deduction of certain basic sociological categories from an investigation of the conditions for the possibility of science. The answer to these two questions will constitute the interwoven themes of this work. It is impossible to overemphasize how closely they are connected. For once, for example, we reject the doctrine that there are everywhere in nature such things as spontaneously occurring parallel cases and see rather that in general they have to be assiduously worked for and artificially produced in the social activity of science, we are forced to constitute an ontology of structures distinct from events.

For us, for the moment, it is sufficient merely to note that the most important feature of science neglected by the doctrine of empirical realism is that it is *work*; and hard work at that. Work consists, paradigmatically, in the transformation of given products. Scientific change is an integral feature of science, in which what is transformed is a part of the formally accredited stock of scientific knowledge. In a scientific training the object transformed is not knowledge but man himself. But in both cases what is transformed is itself already a social product. The peculiar significance of experimental activity is that man qua material object (rather than simply thinker or perceiver) exercises his causal powers to transform the natural world itself, of which he is also a part. Now corresponding to the dissolution of ontology in philosophy, there has been a parallel denegation of the social character of science. In Chapter 3 I will set out to vindicate sociology in an attempt to render intelligible scientific change. This will enable me to reconstitute a transitive dimension, as complementary to the intransitive one established here.

The concept of the empirical world is anthropocentric. The world is what men can experience. But the couple of this concept, and from a realist metaperspective necessary to sustain it, is the absence of the concept of the antecedent social activity necessary to make experience significant in science. And this has the objectionable ideological consequence (from the point of view of the practice of science) that whatever men currently experience is unquestionably the world. Now it is central to the argument of this study that the concepts 'empirical' and 'sense-experience' belong quite unequivocally to the social world of science. Experiences are a part, and when set in

the context of the social activity of science an epistemically critical part, of the world. But just because they are a part of the world they cannot be used to define it. An experience to be significant in science must normally be the result of a social process of production; in this sense it is the end, not the beginning of a journey. But only transcendental realism can explain why scientists are correct in regarding experience as in the last instance the test of theory. For it is by means of it that, under conditions which are artificially produced and controlled, skilled men can come to have access to those enduring and active structures, normally hidden or present to men only in distorted form, that generate the actual phenomena of our world. Empirical realism depends upon a reduction of the real to the actual and of the actual to the empirical. It thus presupposes the spontaneity of conjunctions and of facts. And in doing so presupposes a closed world and a completed science.

It is important to stress that I am not saying that experiences are *less* real than events, or events less real than structures. This is the kind of mistake that is encouraged by the way in which Eddington formulated his problem of the relationship between the familiar and the scientific worlds; in which he described the situation as one in which there were 'duplicates' of every object: two tables, two chairs, two pens, etc.[43] Since then the problem has always seemed to be that of saying *which* object is real. For the ordinary language instrumentalist the scientific object is an artificial construct;[44] for the scientistic super-realist the familiar object a mere illusion.[45] For the transcendental realist however this formulation of the problem is bogus. For if there is a relationship between the worlds it is one of natural generation, not an interpretation of man. The relationship is not between a real and an imaginary object, but between two kinds of real object, one of which is very small. The relationship between electrons and tables has to be understood in terms of causal connections, not correspondence rules. Consequents are not less real, or the statements describing them less true, in virtue of their being effects; any more than causes, in virtue of being recondite, must be imaginary. In particular, the fact that the properties of everyday objects, at what has been picturesquely described as the zone of the middle dimensions,[46] can be explained in terms of the very small (or the very large) does not render them less real than the entities that account for them; anymore than zinc and sulphuric acid cease to react in a certain way when we explain their reaction in terms of their atomic structure.

For the transcendental realist laws, though not our knowledge of them, are categorically independent of men – as thinkers, causal agents and perceivers. Transcendental realism can thus accommodate both Locke's view that there are (or may be) laws which are unknowable;[47] and Kneale's suggestion that there are (or may be) laws whose instances are unperceivable.[48] But it allows in addition the possibility of known laws, whose instances are perceivable, but which, when not instanced in closed systems, remain unmanifest to men. However, my interpretation of these possibilities is different from Locke's

(and Kneale's). For the transcendental realist, our knowledge, perceptual skills and causal powers are set in the context of the ongoing social activity of science; and in the course of it they are continually being extended, to which process there can be no a priori limits. Thus though it may be necessary, to the extent that science is always incomplete, that at any moment of time some laws are unknowable; it is not necessary that any particular laws are.

Locke's mistake in failing to appreciate the possibility that the 'sad experience' of chemists who 'sometimes in vain, search for the same qualities in one parcel of sulphur, antimony or vitriol, which they have found in others'[49] might come to be transformed in the course of the development of science into a knowledge of the 'constitution of their insensible parts, from which flow those sensible qualities, which serve us to distinguish one from another'[50] was not a scientific mistake. It did not consist in his failure to foresee the development of the theory of atomic number and valency or to predict Mendeleyeev's predictions. His scepticism over the possibility of a scientific knowledge of real essences was a philosophical mistake, rooted in his theory of ideas. For if all our knowledge is acquired in perception and perception constitutes the world, there can be no place for an antecedent cause of knowledge (or of perception). But as only what is seen as socially produced can be seen as putatively socially transformable, this leads inevitably to an a-historical view of science.

Locke's error was not therefore based on an inadequate knowledge of chemistry. But on an inadequate concept of the transitive dimension of science, which prevented him from seeing the current state of chemistry as what it was, viz. the *current* state of a science; and which thus allowed him to be influenced by it into propounding a general philosophical thesis about knowledge – and in particular of course about the impossibility of a certain kind of knowledge, viz. of real essences. Locke's case has a general moral. For without a concept of science as a process-in-motion and of knowledge as possessing (in the sense indicated in §1 above) a material cause, it is easy to argue from the current state of a science to a philosophical thesis about knowledge. Consider, for example, the Copenhagen interpretation of Quantum theory. More important perhaps, the influence of Newtonian mechanics on 18th century philosophy led to a kind of stasis in thought from which the philosophy of science has still to recover. Action-by-contact as a paradigm of causality, the celestial closure as a model of knowledge, gravity as the template of our ignorance all had a disastrous effect. The underdevelopment of the sciences of substance in comparison with the science of motion (of the time), and the form that the latter took, thus had, at a decisive moment in the history of philosophy, through the generation of a static philosophical conception of knowledge, a permanent effect on all subsequent 'philosophy of science'. It is in this sense that in philosophy we are still prisoners of the scientific thought of the past.

The anthropocentric and epistemic biases of classical philosophy have resulted in the dominance, in philosophy, of what might be styled 'idols' of a Baconian kind. These are false conceptions which cause men to see, in philosophy, everything in relation to themselves (cf. the concept of the empirical world) and their present knowledge. Six hundred years ago, Copernicus argued that the universe does not revolve around man. And yet in philosophy we still represent things as if it did. In the philosophy of science there must be two Copernican Revolutions. The first establishing a transitive dimension in which our knowledge is seen to be socially produced, and as such neither an epiphenomenon of nature nor a convention of man. The second establishing an intransitive dimension, based on the reconstitution of a philosophical ontology, in which the world of which, in the social activity of science, knowledge is obtained is seen to be in general quite independent of man. These Copernican Revolutions must be given a Copernican interpretation (for Philosophy has its Osianders too); which is why we need the metaphysics of transcendental realism, which will be vindicated by its capacity to render intelligible the underanalysed phenomenon of science.

Corresponding to the two criteria advanced on page 24 above two acid tests for a philosophy of science may be developed:

(1) is knowledge regarded as socially produced, i.e. as having a material cause of its own kind? or is it read straight onto the natural world or out of the human mind?
(2) are the objects of knowledge regarded as existing and acting independently of men? or do they depend implicitly or explicitly upon men for their existence and/or activity?

Scientists try to discover the reasons for things and events, patterns and processes, sequences and structures. To understand how they do so one needs both a concept of the transitive process of knowledge-production and a concept of the intransitive objects of the knowledge they produce: the real mechanisms that generate the actual phenomena of the world, including as a special case our perceptions of them.

Notes

1 See Aristotle, *Metaphysics*, 1.3.
2 See J. R. Ravetz, *Scientific Knowledge and its Social Problems*, pp. 116–19.
3 Cf. R. Harré, *Philosophies of Science*, pp. 176–7.
4 W. Thomson, *Notes of Lectures on Molecular Dynamics*, p. 132.
5 See A. S. Eve, *Rutherford.*
6 Cf. I. Kant, *On the Distinctiveness of the Principles of Natural Theology and Morals.*
7 J. S. Mill, *A System of Logic*, Bk. III, Chap. 3, Sect. 1.
8 Cf. N. R. Hanson, *Patterns of Discovery*, Chap. 1.
9 Cf. J. J. C. Smart, *Philosophy and Scientific Realism*, pp. 38–9.

10 Cf. J. J. Gibson, *The Senses Considered as Perceptual Systems.*
11 Cf. M. Hollis, 'Reason and Reality', *P.A.S. Vol. LXVIII* (1967–8), p. 279.
12 G. E. M. Anscombe, *Causality and Determination*, p. 22; and G. H. von Wright, *Explanation and Understanding*, pp. 60–4.
13 Cf. G. E. M. Anscombe, *op. cit.*, p. 21.
14 Cf. Ravetz's '4th law of thermo-dynamics': no experiment goes properly the first time. See J. R. Ravetz, *op. cit.*, p. 76.
15 Cf. F. Dretske, *Seeing and Knowing*, Chap. 3.
16 Cf. G. H. von Wright, *op cit.*, Chap. 2.
17 L. Wittgenstein, *Tractatus Logico-Philosophicus*, 6.35.
18 I. Kant, *Critique of Pure Reason*, B.724.
19 See e.g. A. J. Ayer, *Language, Truth and Logic*, pp. 31–41.
20 A recent book, A. Quinton's *Nature of Things*, is vitiated by a failure to distinguish these two questions. From the outset Quinton tends to identify the problem of fundamental entities with that of the foundations of knowledge (p. 5). This leads him to argue that 'if all possible evidence for the existence of theoretical entities is provided by common observables it follows . . . that the logically indispensable evidence, and thus the sense, of assertions about theoretical entities must be capable of being expressed in terms of those common observables and thus that theoretical entities can have only a derived and dependent existence' (p. 285).
21 D. H. Mellor, 'Physics and Furniture', *American Philosophical Quarterly, Studies in the Philosophy of Science*, p. 184.
22 See W. V. O. Quine, 'Designation and Existence', *Readings in Philosophical Analysis*, ed. H. Feigl and W. Sellars, p. 50; *Methods of Logic*, p. 224; and *From a Logical Point of View*, Chap. 1 and passim.
23 D. Hume, *Treatise on Human Nature*, p. 632.
24 D. Hume, *op. cit.*, p. 132. Cf. Newton's 4th rule of reasoning in philosophy: 'propositions inferred by general induction from phenomena [are to be regarded as] true . . . till such time as other phenomena occur by which they may either be made more accurate or liable to exceptions', I. Newton, *Principia Mathematica*, Bk. III.
25 D. Hume, *op. cit.*, p. 231.
26 T. S. Kuhn, *The Structure of Scientific Revolutions*, Chaps. II–IV.
27 See e.g. M. Hesse, *In Defence of Objectivity*, p. 14.
28 See e.g. G. Buchdahl, *op. cit.*, p. 31.
29 The structure of such a critique would be as follows: If the subject matter of social psychology is such that a science of social psychology is possible and social psychologists are to have knowledge of it, then social psychologists should do φ, ψ, etc. rather than x, ω, etc. The transcendental realist could thus not accept the notorious definition of economics as what economists do. For him, whether or not they actually do economics is at least in part a contingent question. Notice that the formula I have used leaves the question of whether a science of social psychology is possible open. This is important because for the transcendental realist it is the nature of the object that determines the possibility of a science. Thus he can allow, without paradox, that there may be no humanly intelligible pattern to be discovered in the stars or politically intelligible pattern in voting behaviour. So that no science of astrology or psephology is possible, no matter now scrupulously 'scientific method' is adhered to.
30 K. R. Popper, *Conjectures and Refutations*, p. 192.
31 Cf. J. J. C. Smart, *op. cit.*, pp. 149–51.
32 In fact men have the capacity to be self-conscious in two ways: first, in being

conscious of what they are doing; and secondly, in being conscious of their doing it. That these two are not equivalent is shown by the fact that in some contexts a person may know what he has done but not that he has done it and vice-versa.

33 L. Wittgenstein, *Philosophical Investigation*, 115.

34 A recent antecedent of the view that causal laws should be analysed as tendencies is contained in P. T. Geach, 'Aquinas', *Three Philosophers*, G. E. M. Anscombe and P. T. Geach, pp. 101ff. Important works in the recent development of the concept of powers are W. D. Joske, *Material Objects*, Chaps. 4 and 5; M. R. Ayers, *The Refutation of Determinism*, Chaps. 3–5; and R. Harré, *Principles of Scientific Thinking*, esp. Chap. 10.

35 I owe this term to M. Scriven, 'Truisms as the Grounds for Historical Explanation', *Theories of History*, ed. P. Gardiner, pp. 464ff. Scriven uses it to refer to generalizations grounding historical explanations which contain modifiers such as 'normally', 'tendency', 'usually', etc. My use of the term is substantially different. But it is the nearest thing to an antecedent for the kind of conditional I am concerned with.

36 Formally we could say that in experimental production by doing φ we change α to a so altering the state that would otherwise have prevailed; and in experimental production by doing or being prepared to do ψ we exclude the intervention of elements $\beta_1 \ldots \beta_n$ so allowing the mechanism M set in motion by a to generate b. The sequence a.b thus appears as a consequence of the results of our actions. It is in this sense that a closure is normally a human product.

37 See G. H. von Wright, *op. cit.*, pp. 43–45.

38 Cf. N. R. Hanson, *Observation and Explanation*, p. 56.

39 See e.g. A. Koyré, *Metaphysics and Measurement*, Chap. 4.

40 V. Ronchi, 'Complexities, advances and misconceptions in the development of the science of vision: what is being discovered?', *Scientific Change*, ed. A. Crombie, pp. 542–61.

41 E. J. Dijksterhuis, *The Mechanisation of the World Picture*, p. 338.

42 See K. R. Popper, *The Logic of Scientific Discovery*, App. XI.

43 A. S. Eddington, *The Nature of the Physical World*, p. xi. Stebbing substituted the idea of 'counterparts' for that of 'duplicates' in her rendering of the problem. See L. S. Stebbing, *Philosophy and The Physicists*, p. 60.

44 See e.g. L. S. Stebbing, *op. cit.*, p. 66; and G. Ryle, *Dilemmas*, p. 80.

45 See e.g. W. Sellars, 'The Language of Theories', *Current Issues in the Philosophy of Science*, ed. H. Feigl and G. Maxwell, p. 76; and P. K. Feyerabend, 'Explanation, Reduction and Empiricism', *Minnesota Studies in the Philosophy of Science*, Vol. III, ed. H. Feigl and G. Maxwell, p. 83.

46 M. Čapek, *The Philosophical Impact of Contemporary Physics*, p. 294.

47 J. Locke, *Essay Concerning Human Understanding*, esp. Bk. IV, Chap. III.

48 W. Kneale, *Probability and Induction*, pp. 97–103. Kneale's point could be strengthened by an argument to show that in the case of physical theories the basic entities must be unperceivable. For if they were perceivable it would seem possible to ask what caused them to manifest themselves to us as perceivable; in which case they could not be basic. This is a general argument in favour of a field-theoretic interpretation of basic entities in physics. Cf. Dingle's comment that if photons could be seen they would get in the way (J. J. C. Smart, *op. cit.*, p. 38).

49 J. Locke, *op. cit.*, Bk. III, Chap. 6.9.

50 J. Locke, *op. cit.*, Bk. IV, Chap. 3.7.

3

THE LOGIC OF SCIENTIFIC DISCOVERY

Roy Bhaskar

1. Introduction: on the contingency of the causal connection

In Chapter 2 I assumed the existence of a body of knowledge and asked how it could be applicable to the world. My particular concern was to establish its universality (transfactuality). I now want to turn to the question of how such knowledge, given that it is transfactually applicable to the world, comes to be produced; and in particular to the question of how law-like statements come to be established as necessary. My concern shifts here then from the synchronic to the diachronic aspects of science, and in particular to the question of how, in the social activity of science, natural necessity comes to be ascribed. In the course of this chapter I will consider to what universality and necessity is properly ascribed, and what must be the case for these ascriptions to be possible.

In order to show how the concept of natural necessity is possible I will need to turn from a critique of the ontology of closed systems to a critique of the ontology of atomistic events that implies it; and hence from a critique of the idea of the actuality of the causal connection to a critique of the idea of its contingency. In Chapter 4 I will ask what accounts for the assumption of the atomicity of the events conjoined that entails a closed system and generates, in its wake, a host of philosophical problems.

The connection between my concerns in this and the preceding chapter is clear. For once an ontology of atomistic events is constituted, it follows that, for general knowledge to be possible, events must be always conjoined (under appropriate descriptions) and never connected.[1] That is, order in the world must consist of an unfailing or invariant order of the co-existence of events in space and their succession in time. Conversely once it is appreciated that events, though caused (and consisting in transformations), are very rarely

Source: *A Realist Theory of Science*, London: Verso, 1997, chap. 3, sections 3.1–3.3, pp. 143–84, and sections 3.5 and 3.6, pp. 199–228.

conjoined, it can be seen why order in the world must be pitched at a level categorically distinct from events. Now I have argued in effect that we produce conjunctions to discover connections and apply connections in a world of non-conjunctions; so that events, though rarely conjoined, are sometimes connected. In this chapter I want to consider the nature of the connection that holds between events (when it does) and the nature of the necessity implicit in the concept of law. I will thus be shifting my attention from the differentiation of the world as such to the nature of the stratification that, if we are to render intelligible the experimental establishment and practical application of our knowledge, it implies. Science attempts, I will argue, in its essential movement, to capture the stratification of the world. In order to describe this movement I will need to reconstitute the other dimension of the Copernican Revolution in the philosophy of science, viz. the transitive (or sociological) dimension in which men come, in their social activity, to acquire knowledge of the enduring and transfactually acting mechanisms of nature, in virtue of which some but not other sequences of events are necessarily connected and some but not other statements are universally applicable. The idea that there are no necessary connections between matters of fact occupies an analogous position in underpinning the doctrine of the contingency of the causal connection, as the idea that there are always descriptions for events such that the formula 'whenever this, then that' applies does in underpinning the doctrine of its actuality. And I will argue that just as for science to be possible the world must be open; so there must be necessary connections between matters of fact, if science is to be possible.

In Chapters 1 and 2 I have shown how the intelligibility of the activities of the experimental establishment and the practical application of our knowledge presupposes the categorical independence of causal laws from the patterns of events, and how causal laws must be given an ontological basis in the enduring and transfactually active mechanisms of nature. Modern transcendental idealist philosophies of science, which are perhaps more influenced by Wittgenstein than Kant, stop at what is in effect the second stage of a dialectic or process of discovery in science, by refusing to allow (or inadequately interpreting) the possibility of a realist interpretation of theory.

Thus there is in science a characteristic kind of dialectic in which a regularity is identified, a plausible explanation for it is invented and the reality of the entities and processes postulated in the explanation is then checked. This is the logic of scientific discovery, illustrated in Diagram 1 below. If the classical empiricist tradition stops at the first step, the neo-Kantian tradition sees the need for the second. But it either denies the possibility, or does not draw the full (transcendental realist) implications of the third step. If and only if the third step is taken can there be an adequate rationale for the use of laws to explain phenomena in open systems (where no constant conjunctions prevail) or for the experimental establishment of that knowledge in the first place.

Just as transcendental realism differentiates itself from empiricism by

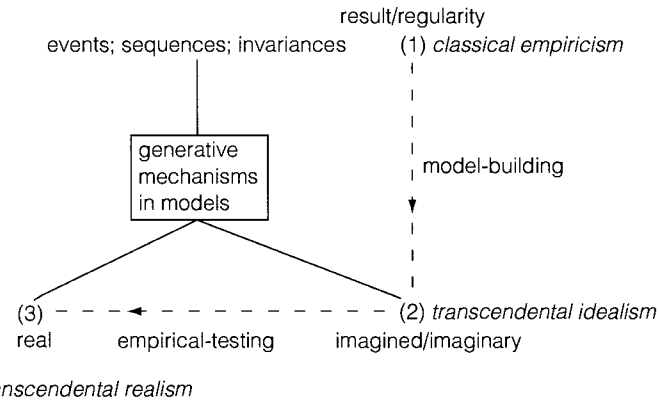

Diagram 1 The logic of scientific discovery

interpreting the first stage of the dialectic as the invariance of a *result* rather than that of a *regularity*, so it differentiates itself from transcendental idealism in its interpretation of the second stage. Both transcendental realism and idealism see the move from (1) to (2) as involving creative model-building, in which plausible generative mechanisms are *imagined* to produce the phenomena in question. But whereas for transcendental idealism the imagined mechanism is *imaginary*, for realism it may be *real*, and come to be established as such. What is imagined may be real; but what is imaginary cannot. 'Imaginary/real' marks an ontological watershed; 'imagined/known to be real' an epistemic one. Now what is imagined at t_1 may come at t_2 to be known to be real. And for transcendental realism the move from (2) to (3) involves experimental production and control, in which the reality of the mechanisms postulated in the model are subjected to empirical scrutiny. For transcendental realism that some real things and generative mechanisms *must* exist can be established by philosophical argument (their existence, and transfactual activity, is a condition of the possibility of science). But it is contingent and the job of substantive science to discover which ones actually do. That is, it is the task of science to discover which hypothetical or imagined mechanisms are not imaginary but real; or, to put it the other way round, to discover what the real mechanisms are, i.e. to produce an adequate account of them.

Science is a process-in-motion. It involves three distinct stages, which cannot be omitted or collapsed into one another without doing tremendous violence to our understanding of science. But these stages cannot be identified with moments of chronological time; they are phases of science. It should be noted that the move from (1) to (2) just because it involves the postulation of novel entities and processes cannot be given a deductive interpretation. But given this it can only be justified in a non-pragmatic way if we hold out the possibility of a realist interpretation of some of

the hypothetical entities etc. invoked to explain the behaviour. Such an interpretation can in turn only be justified empirically if it is set in the context of the ongoing social activity of science. Thus it is in the planning of future experiences rather than in the ordering of present ones or the memory of past ones that our rational and empirical 'faculties', 'whose unkind and ill-starred divorce' Bacon saw as responsible for all the confusion in 'the affairs of the human family',[2] are most productively combined.

It is only, I shall argue, if we allow the possibility of the move from (2) to (3) that we can, in the end, uphold the legitimacy of the move from (1) to (2). Moreover it is only if we begin to see science in terms of *moves* and are not mesmerized by terminals that we can give an adequate account of science. In this respect much philosophy is still in the same position as a Martian trying to discover what trams are but able only to observe them in open-air museums with children scrambling over them. It is the task of the philosophy of science to capture science's essential movement, not to guess its eventual destination.

Recent work in the philosophy of science has established (i) the fact of scientific change and (ii) the poverty of a purely deductivist analysis of explanation. In this way it has done much towards the establishment of a conception of science as a critical social activity. The case for transcendental realism can, however, be strengthened by considering the limitations of this work. For unless these two insights are taken together and a new ingredient is added to the existing philosophical mix they are, I think, vulnerable to positivist counter-attack. This new ingredient must be in the field of ontology. The argument of Chapter 1 enables us to see why this is so. For the logical empiricism against which recent philosophy of science has reacted contained not only an account of science, but (implicitly) an account of reality, of the world known by science. And it is in this unacknowledged ontological legacy that the weaknesses of both developments lie. My aim in this chapter and the next is to pinpoint these weaknesses. And to show in particular why and how an adequate non-empiricist account of science, capable of accommodating the facts of scientific change and structure, requires an ontology of the kind outlined in Chapter 1 and elaborated in Chapter 2. Indeed, recent philosophy of science illustrates very well the kind of 'ontological tension' that can occur when a fundamental objection is made to a philosophical theory without simultaneously questioning that theory's ontology. The general difference between recent philosophy of science and transcendental realism could be summed up by saying that whereas recent philosophy has asked merely what are the conditions of the possibility of individual experience and found an answer in the intersubjective world of science, transcendental realism asks in addition for the conditions of the possibility of the social activity of science, finding an answer in the intransitive world of things.

I will need in this chapter not only to show the necessity for the philosophical ontology of transcendental realism, but also to begin the

development of the philosophical sociology that I argued in 1.6 is presupposed by any theory of science. Scientific development, I have argued so far, consists in the transformation of social products, antecedently established items of knowledge, which may be regarded as Aristotelian material causes. Certain implications flow from this conception. First, that men never construct their knowledge from scratch. It stands to them always as a given product, a social transmit;[3] which they must themselves reproduce or partially transform. The Copernican Revolution in the transitive dimension of the philosophy of science thus has the profound implication that man never *creates*, but only *changes*, his knowledge, with the cognitive tools at his disposal. Secondly, what is to be changed, has first to be acquired. And what is acquired consists always of an *ensemble* of theoretical and empirical ideas, so that knowledge can never be analysed out as a function of individual sense-experience. Once this is grasped the grounds for the atomistic ontology that generates the idea of the contingency of the causal connection collapse.

Science then is an ongoing social activity which pre-exists any particular generation of scientists and any particular moment of consciousness. Its aim is the production of the knowledge of the independently existing and transfactually active mechanisms of nature. Corresponding to the criterion developed in the intransitive dimension of the philosophy of science, viz. the conceivability of a world without men, we thus have a criterion in the transitive dimension, namely the inconceivability of knowledge without antecedents.

2. The surplus-element in the analysis of law-like statements: a critique of the theory of models

It has often been held that a constant conjunction of events is not a sufficient condition for a causal law. This may be because it is regarded as incapable of sustaining the intuitively obvious and important difference between necessary and accidental sequences or in Johnson's time-honoured terminology between 'universals of law' and 'universals of fact'.[4] Or it may be because it is regarded as incapable of licensing what it is intuitively felt causal laws do licence, namely counter-factual conditionals.[5] It is never seriously denied that we feel, and scientists act as if, some but not other sequences of events are 'necessarily connected'; so that we must possess the concept. What the radical empiricist, in the form of Hume, denies is: (a) that there is any objective basis for this distinction, i.e. that it corresponds to any real difference between the two sequences of events; and (b) that there is any justification, apart from habit or custom, for our ascriptions of natural necessity and accident.[6] Most philosophers since Hume have attempted to show how he was wrong in (b) without objecting to (a). I want to argue that Hume was wrong in (a); and that it is only if we can establish this that we can show why he was wrong in (b) also.

The radical empiricist challenge to philosophers then is to provide an alternative account of the 'surplus-element'[7] in the analysis of law-like statements; that is, that element over and above the (presumed) constant conjunction that explains our ascriptions of necessity; and which will show how, and the conditions under which, a distinction between necessary and accidental sequences and the assertion of counter-factuals can be rationally justified. The usual response to this challenge consists in the attempt to locate the surplus-element in the statement's 'explanation', and more particularly in the 'theory' which explains it. However the terms 'explanation' and 'theory' cover a gamut of philosophical positions, which must now be considered.

The deducibility of a law-like statement from a set of higher order statements is often regarded as a criterion of 'explanation'.[8] However if deducibility is the only criterion for explanation and the source of the surplus-element is its explanation there will be an infinite number of surplus-elements for *any* statement. Hence any statement can be said to be law-like on an infinite number of grounds![9] Deducibility alone cannot explicate the distinction between necessary and accidental or nomic and non-nomic universals. Moreover additional criteria such as simplicity can only reduce the number of possible explanations for a statement which has already been identified as law-like. But they cannot be used to say which statements are law-like and so possess the surplus-element. For even if there were a simplest explanation for every statement, there are no absolutely simple explanations. Thus such criteria can at best be used to explain why we choose one explanation rather than another, but not why one statement rather than another is regarded as law-like.[10]

Of course it might be objected that when everything is explained all factual statements will be law-like. But what would count as an explanation then? Could it be anything other than an inexplicable constant conjunction of events, as in the case of Mill's unconditional laws?[11] If it could not, we are back with Hume, and have done nothing to allay the sting of the radical empiricist challenge. If it could, some alternative non-Humean analysis of the ultimate or highest-order laws must be given which will show how they, as uniquely qualified 'explainers', do possess a genuine surplus-element. We are thus faced with the following dilemma: either explanation is achieved by subsumption under higher-order laws in which case the problem is merely shifted, for a surplus-element must be found for them if they are to qualify as 'laws'; or an alternative analysis of 'explanation' must be given, which does not identify the explanans with a further set of laws, and so provides room for the location of a surplus-element in the analysis of laws, within the context of their explanation, at any one level.

It might be thought that it is in the capacity of the law-like statement to yield successful predictions that the source of the surplus-element lies. But this will not do without an analysis of the 'capacity' or 'power'. For the

Humean it is the past and actual successes of the statement that count, not its potential ones. And these can at best explain, not justify, the surplus-element. It is the surplus-element that must provide our inductive warrant, if we have one; rather than the other way round. Moreover even an accidental generalization is capable of yielding correct predictions, viz. as long as the conditions that account for it persist. This suggests that, even if we were to possess some general inductive warrant, predictive success alone could not differentiate necessary from accidental sequences or license the assertion of counterfactuals.

It seems clear that if we are to get any further in our search for the surplus-element the idea of purely formal differentiae must be abandoned. Inductive considerations prove no better than deductive ones. For accidental generalizations may be inductively confirmed, just as they may be deductively explained. In practice then the non-radical empiricist, if he is not to concede the game, is forced to re-examine the account of science that seems to render any non-Humean conclusion impossible. The fundamental fact about science that has been missing from the discussion so far is the existence at any moment of time of an antecedently established body of theory. And it is here that the non-radical empiricist attempts to locate the surplus-element. But can 'theory' do what experience and deducibility fail to do, i.e. provide a rational ground for our ascriptions of natural necessity? The answer clearly depends upon the extent to which the former contains components irreducible to the latter. And the onus is on the philosopher who attempts to locate the surplus-element in the systematic organization of our knowledge or the capacity of a theory to explain many different laws[12] or to predict novel kinds of facts[13] to show how their concept of theory escapes Humean analysis. Goodman's notion of entrenchment,[14] for example, functions in exactly the same way as Hume's notion of custom and can no more justify our attributions of necessity than the latter could.

In short, unless theory contains elements irreducible to experience and truth-functional operations on it there is no basis for a non-Humean theory of natural necessity.[15] Thus the possibility of the latter depends upon some terms of the theory not being explicitly defined in terms of experience and/or some statements of the theory not being deductively connected and/or some ideas of the theory being non-propositional in logical (or non-sentential in linguistic) form. These establish the possibilities of intensional relationships between predicates, non-deductive (e.g. analogical) relationships between ideas and non-propositional (e.g. iconic) ideas respectively as potential sources of necessity. It is the second of these that has been most thoroughly explored; and it is to Campbell's initial formulation of the theory of models that I now turn.

On Campbell's view a theory must contain not only a 'dictionary' correlating some, but not all, of the theoretical concepts with empirical terms but a 'model' for the hypotheses or theoretical statements of a theory, by means of