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A REALIST THEORY OF SCIENCE

Roy Bhaskar

From Marx to Mao



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Contents

<i>Preface</i>	7
<i>Preface to the Second Edition</i>	11
<i>Introduction</i>	12
<i>Chapter 1. Philosophy and Scientific Realism</i>	21
1. Two Sides of ‘Knowledge’	21
2. Three Traditions in the Philosophy of Science	24
3. The Transcendental Analysis of Experience	30
A. The Analysis of Perception	31
B. The Analysis of Experimental Activity	33
4. The Status of Ontology and Its Dissolution in Classical Philosophy	36
5. Ontology Vindicated and The Real Basis of Causal Laws	45
6. A Sketch of a Critique of Empirical Realism	56
<i>Chapter 2. Actualism and the Concept of a Closure</i>	63
1. Introduction: On the Actuality of the Causal Connection	63
2. Regularity Determinism and the Quest for a Closure	69
3. The Classical Paradigm of Action	79
4. Actualism and Transcendental Realism: The Interpretation of Normic Statements	91
5. Autonomy and Reduction	105
6. Explanation in Open Systems	118
<i>Appendix. Orthodox Philosophy of Science and the Implications of Open Systems</i>	127
<i>Chapter 3. The Logic of Scientific Discovery</i>	143
1. Introduction: On the Contingency of the Causal Connection	143

<i>4 A Realist Theory of Science</i>	
2. The Surplus-Element in the Analysis of Law-like Statements: A Critique of the Theory of Models	148
3. Natural Necessity and Natural Kinds: The Stratification of Nature and The Stratification of Science	163
4. The Social Production of Knowledge by Means of Knowledge	185
5. Objections to the Account of Natural Necessity Proposed	199
6. The Problem of Induction	215
<i>Appendix. Natural Tendencies and Causal Powers</i>	229
<i>Chapter 4. Metaphysics and the Philosophy of Science</i>	239
<i>Postscript to the Second Edition</i>	251
<i>Bibliography</i>	263
<i>Index of names</i>	273
<i>Index of subjects</i>	275

To my mother

Preface

It has often been claimed, and perhaps more often felt, that the problems of philosophy have been solved. And yet, like the proverbial frog at the bottom of the beer mug, they have always reappeared. There was a phase in recent philosophy when it was widely held that the problem was the problems and not their solution. In practice, however, this interesting idea was usually coupled with the belief that termination of philosophical reflection of the traditional kind would be in itself sufficient to resolve the problems to which, it was held, philosophical reflection had given rise.

Whatever the merits of such a view in general, it is quite untenable for any philosopher who is concerned with science. For in one science after another recent developments, or in some cases the lack of them, have forced old philosophical problems to the fore. Thus the dispute between Parmenides and Heraclitus as to whether being or becoming is ultimate lies not far from the centre of methodological controversy in physics; while the dispute between rationalists and empiricists over the respective roles of the a priori and the empirical continues to dominate methodological discussion in economics. Sociologists are making increasing use of the allegedly discredited Aristotelian typology of causes. And the problem of universals has re-emerged in an almost Platonic form in structural linguistics, anthropology and developmental biology. The spectre of determinism continues to haunt many of the sciences; and the problem of 'free-will' is still a problem for psychology.

In this context one might have expected a ferment of creative activity within the philosophy of science, and to a degree this has occurred. But the latter's capacity for autonomous growth is limited. For the critical or analytical philosopher of science can only say as much as the philosophical tools at his disposal enable him to say. And if philosophy lags behind the needs of the moment then he is left in the position of a Priestley forced,

by the inadequacy of his conceptual equipment, to think of oxygen as ‘dephlogisticated air’;¹ or, of a Winch baffled by an alien sociology.²

Hegel may have exaggerated when he said that philosophy always arrives on the scene too late.³ Yet there can be little doubt that our theory of knowledge has scarcely come to terms with, let alone resolved the crises induced by, the changes that have taken place across the whole spectrum of scientific (and one might add social and political) thought. In this respect our present age contrasts unfavourably with both Ancient Greece and Post-Renaissance Europe, where there was a close and mutually beneficial relationship between science and philosophy. It is true that in the second of these periods there was a progressive ‘problem-shift’ within philosophy from the question of the content of knowledge to the meta-question of its status as such.⁴ This shift was in part a response to the consolidation of the Newtonian world-view, until by Kant’s time its fundamental axioms could be regarded as a priori conditions of the possibility of any empirical knowledge. However, those philosophers of the present who insist upon their total autonomy from the natural and human sciences not only impoverish, but delude themselves. For they thereby condemn themselves to living in the shadow cast by the great scientific thought of the past.

Anyone who doubts that scientific theories constitute a significant ingredient in philosophical thought should consider what the course of intellectual history might have been if gestalt psychology had been established in place of Hartley’s principle of the association of ideas; or if the phenomena of electricity and magnetism had come to be regarded as more basic than those of impact and gravity; or if sounds and smells had been taken as constitutive of the basic stuff of reality and the rich tapestry of the visual-tactile world had been regarded, like a Beethoven symphony or the perfume of a rose, as a mere effect of those primary powers. Suppose further that philoso-

¹ See e.g. S. E. Toulmin, ‘Crucial Experiments: Priestley and Lavoisier’ *The Journal of the History of Ideas*, Vol. XVIII (1957), pp. 205–20; and J. B. Conant, *The Overthrow of the Phlogiston Theory*.

² P. Winch, *The Idea of a Social Science*, p. 114.

³ G. W. F. Hegel, *Philosophy of Right*, Preface.

⁴ Cf. G. Buchdahl, *Metaphysics and the Philosophy of Science*, p. 2

phers had taken biology or economics as their paradigm of a science rather than physics; or 16th not 17th century physics as their paradigm of scientific activity. Would not our philosophical inheritance have been vastly different? As this is primarily a problem for the philosophy of philosophy rather than the philosophy of science, I shall not dwell on this point further here. Its significance for our story will emerge in due course.

The primary aim of this study is the development of a systematic realist account of science. In this way I hope to provide a comprehensive alternative to the positivism that has usurped the title of science. I think that only the position developed here can do full justice to the rationality of scientific practice or sustain the intelligibility of such scientific activities as theory-construction and experimentation. And that while recent developments in the philosophy of science mark a great advance on positivism they must eventually prove vulnerable to positivist counter-attack, unless carried to the limit worked out here.

My subsidiary aim is thus to show once-and-for-all why no return to positivism is possible. This of course depends upon my primary aim. For any adequate answer to the critical meta-question 'what are the conditions of the plausibility of an account of science?' presupposes an account which is capable of thinking of those conditions as special cases. That is to say, to adapt an image of Wittgenstein's, one can only see the fly in the fly-bottle if one's perspective is different from that of the fly.⁵ And the sting is only removed from a system of thought when the particular conditions under which it makes sense are described. In practice this task is simplified for us by the fact that the conditions under which positivism is plausible as an account of science are largely co-extensive with the conditions under which experience is significant in science. This is of course an important and substantive question which we could say, echoing Kant, no account of science can decline, but positivism cannot ask, because (it will be seen) the idea of *insignificant experiences* transcends the very bounds of its thought.⁶

This book is written in the context of vigorous critical activity in the philosophy of science. In the course of this the twin

⁵ L. Wittgenstein, *Philosophical Investigations*, 309.

⁶ I. Kant, *Critique of Pure Reason*, Preface to the 1st Edition.

templates of the positivist view of science, viz. the ideas that science has a certain base and a deductive structure, have been subjected to damaging attack. With a degree of arbitrariness one can separate this critical activity into two strands. The first, represented by writers such as Kuhn, Popper, Lakatos, Feysabend, Toulmin, Polanyi and Ravetz, emphasises the *social* character of science and focuses particularly on the phenomena of scientific change and development. It is generally critical of any monistic interpretation of scientific development, of the kind characteristic of empiricist historiography and implicit in any doctrine of the foundations of knowledge. The second strand, represented by the work of Scriven, Hanson, Hesse and Harré among others, calls attention to the *stratification* of science. It stresses the difference between explanation and prediction and emphasises the role played by models in scientific thought. It is highly critical of the deductivist view of the structure of scientific theories, and more generally of any exclusively formal account of science. This study attempts to synthesise these two critical strands; and to show in particular why and how the realism presupposed by the first strand must be extended to cover the objects of scientific thought postulated by the second strand. In this way I will be describing the nature and the development of what has been hailed as the ‘Copernican Revolution’ in the philosophy of science.⁷

To see science as a social activity, and as structured and discriminating in its thought, constitutes a significant step in our understanding of science. But, I shall argue, without the support of a revised ontology, and in particular a conception of the world as stratified and differentiated too, it is impossible to steer clear of the Scylla of holding the structure dispensable in the long run (back to empiricism) without being pulled into the Charybdis of justifying it exclusively in terms of the fixed or changing needs of the scientific community (a form of neo-Kantian pragmatism exemplified by e.g. Toulmin and Kuhn). In this study I attempt to show how such a revised ontology is in fact presupposed by the social activity of science. The basic principle of realist philosophy of science, viz. that perception gives us access to things and experimental activity access to structures that exist independently of us, is very simple. Yet the

⁷ R. Harré, *Principles of Scientific Thinking*, p. 15.

full working out of this principle implies a radical account of the nature of causal laws, viz. as expressing tendencies of things, not conjunctions of events. And it implies that a constant conjunction of events is no more a necessary than a sufficient condition for a causal law.

I do not claim in this book to solve any general problems of philosophy. It is my intention merely to give an adequate account of science. Philosophers, including philosophers of science, have for too long regarded the philosophy of science as a simple substitution instance of some more general theory of knowledge. This is a situation which has worked to the disadvantage of both philosophy and knowledge. If, however, we reverse the customary procedure and substitute the more specific 'science' (or even better 'sciences') for 'knowledge', considerable illumination of many traditional epistemological problems can, I think, be achieved. And some even, in so far as the 'knowledge' we are concerned with is that produced by 'science', become susceptible of definitive solution. The result of this reversal will also be a philosophy which has a greater relevance than is the case at present for scientific practice. In this sense my objective could be said to be a 'philosophy *for* science'. For I willingly confess to Lockean motives. That is to say, I believe it to be an essential (though not the only) part of the business of philosophy to act as the under-labourer, and occasionally as the mid-wife, of science.⁸ I have therefore tried in this study both to relate the philosophy of science to the more general historical concerns of philosophy; and at the same time to indicate more precisely than is usual the consequences for scientific practice of the methodological strategies implied by different philosophies of science.

We are too apt to forget the frailty of both our science and our philosophy. There can be no certainty that they will survive and flourish; or, if they do, that they will benefit mankind. Civilisation is, like man himself, perhaps nothing more than a temporary rupture in the normal order of things.⁹ It is thus also part of the job of the philosopher to show the limits of science. And, in this broader sense, to seek to ensure that the Owl of Minerva takes flight before the final falling of the dusk.

⁸ J. Locke, *Essay Concerning Human Understanding*, Epistle to the Reader. ⁹ Cf. M. Foucault, *The Order of Things*, p. XXIII.

I would like to take this opportunity to express my thanks to Alan Montefiore and Rom Harré for reading earlier versions of this work; to Rom Harré and Hilary Wainwright for their continual encouragement; to many other colleagues and friends for their help; and to Mrs E. Browne for typing the manuscript.

ROY BHASKAR
University of Edinburgh
April 1974

Preface to the 2nd edition

This edition includes a postscript and an index. The postscript enables me to critically comment on the book. The index fills a major lacuna in the first edition of the work. Francis Roberts and Robin Kinross helped me to compile it.

ROY BHASKAR
University of Edinburgh
September 1977

Introduction

The aim of this book is the development of a systematic realist account of science. Such an account must provide a comprehensive alternative to the positivism which since the time of Hume has fashioned our image of science. Central to the positivist vision of science is the Humean theory of causal laws. It is a principal concern of this study to develop some new arguments and show how they relate to more familiar ones against this still widely accepted theory. In particular I want to argue that not only is a constant conjunction of events not a sufficient, it is not even a necessary condition for a scientific law; and that it is only if we can establish the latter that we can provide an adequate rationale for the former. It has often been contended that a constant conjunction of events is insufficient, but it has not so far been systematically argued that it is not necessary. This can, however, be shown by a transcendental argument from the nature of experimental activity.

It is a condition of the intelligibility of experimental activity that in an experiment the experimenter is a causal agent of a sequence of events but not of the causal law which the sequence of events enables him to identify. This suggests that there is a *ontological* distinction between scientific laws and patterns of events. Obviously this creates a *prima facie* problem for any theory of science. I think that it can be solved along the following lines: To ascribe a law one needs a theory. For it is only if it is backed by a theory, containing a model or conception of a putative causal or explanatory 'link', that a law can be distinguished from a purely accidental concomitance. The possibility of saying this clearly depends upon a non-reductionist conception of theory. Now at the core of theory is a conception or picture of a natural mechanism or structure at work. Under certain conditions some postulated mechanisms can come to be established as real. And it is in the working of such mechanisms that the objective basis of our ascriptions of natural necessity lies.

It is only if we make the assumption of the real independence of such mechanisms from the events they generate that we are justified in assuming that they endure and go on acting in their normal way outside the experimentally closed conditions that enable us to empirically identify them. But it is only if we are justified in assuming this that the idea of the universality of a known law can be sustained or that experimental activity can be rendered intelligible. Hence one of the chief objections to positivism is that it cannot show why or the conditions under which experience is significant in science. Most critics have emphasized its depreciation of the role of theory; this argument shows its inadequacy to experience. Moreover it is only because it must be assumed, if experimental activity is to be rendered intelligible, that natural mechanisms endure and act outside the conditions that enable us to identify them that the applicability of known laws in open systems, i.e. in systems where no constant conjunctions of events prevail, can be sustained. This has the corollary that a constant conjunction of events cannot be necessary for the assumption of the efficacy of a law.

This argument shows that real structures exist independently of and are often out of phase with the actual patterns of events. Indeed it is only because of the latter that we need to perform experiments and only because of the former that we can make sense of our performances of them. Similarly it can be shown to be a condition of the intelligibility of perception that events occur independently of experiences. And experiences are often (epistemically speaking) 'out of phase' with events – e.g. when they are misidentified. It is partly because of this possibility that the scientist needs a scientific education or training. Thus I will argue that what I will call the domains of the real, the actual and the empirical are distinct. This is represented in Table 0.1 below:-

Table 0.1

	<i>Domain of Real</i>	<i>Domain of Actual</i>	<i>Domain of Empirical</i>
<i>Mechanisms</i>	√		
<i>Events</i>	√	√	
<i>Experiences</i>	√	√	√

The real basis of causal laws are provided by the generative mechanisms of nature. Such generative mechanisms are, it is argued, nothing other than the ways of acting of things. And causal laws must be analysed as their tendencies. Tendencies may be regarded as powers or liabilities of a thing which may be exercised without being manifest in any particular outcome. The kind of conditional we are concerned with here may be characterised as normic. They are not counter-factual but trans-factual statements. Nomic universals, properly understood, are trans-factual or normic statements with factual instances in the laboratory (and perhaps a few other effectively closed contexts) that constitute their empirical grounds; they need not, and in general will not, be reflected in an invariant pattern or regularly recurring sequence of events.

The weakness of the Humean concept of laws is that it ties laws to closed systems, viz. systems where a constant conjunction of events occurs. This has the consequence that neither the experimental establishment nor the practical application of our knowledge in open systems can be sustained. Once we allow for open systems then laws can only be universal if they are interpreted in a non-empirical (trans-factual) way, i.e. as designating the activity of generative mechanisms and structures independently of any particular sequence or pattern of events. But once we do this there is an ontological basis for a concept of natural necessity, that is necessity in nature quite independent of men or human activity.

In science there is a 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 dialectic is illustrated in Diagram 0.1 below. If a classical empiricist tradition in the philosophy of science stops at the first stage, a rival neo-Kantian or transcendental idealist tradition (discernible in the history of the philosophy of science) stops at the second. If and only if the third step is taken and developed in the way indicated above can there be an adequate rationale for the use of laws to explain phenomena in open systems, where no constant conjunctions prevail. It is the unthinking presupposition of closed systems together with the failure to analyse experimental activity (which presupposes open systems) that accounts for the most glaring

weakness of orthodox philosophy of science: viz. the non-existence in science of Humean causal laws, i.e. of universal empirical generalizations, and hence the inadequacy of the criteria of explanation, confirmation (or falsification), scientific rationality etc., that are based on the assumption that a closure is the universal rule rather than the rare and (for the most part) artificially generated exception that I contend it is. It is because

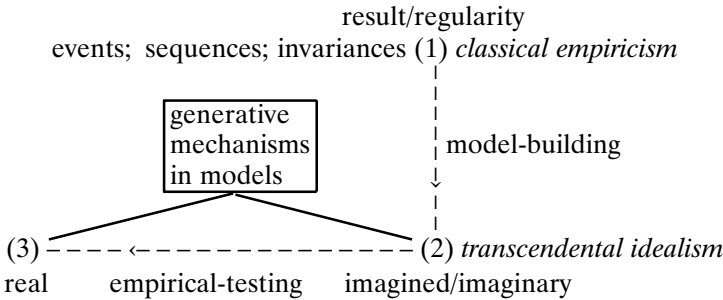


Diagram 0.1. *The Logic of Scientific Discovery*

our activity is (normally) a necessary condition of constant conjunctions of events that the philosophy of science needs an ontology of structures and transfactually active things.

The position advanced here is characterized as transcendental realism, in opposition to the empirical realism common to the other two traditions. Both the neo-Kantian or transcendental idealist tradition and transcendental realism see the step between (1) and (2) in Diagram 0.1 as involving creative model building, in which plausible generative mechanisms are imagined to produce the phenomena in question. But transcendental realism sees the need for the step between (2) and (3) also, in which the reality of the mechanisms postulated are subjected to empirical scrutiny. Transcendental realism differs from empirical realism in interpreting (1) as the invariance of an (experimentally produced) result rather than a regularity; and from transcendental idealism in allowing the possibility that what is imagined in (2) need not be imaginary but may be (and come to be known as) real. Without such an interpretation it is impossible to sustain the rationality of scientific growth and change.

A conception of science is argued for in which it is seen as a

process-in-motion, with the dialectic mentioned above in principle having no foreseeable end. Thus when a new stratum or level of reality has been discovered and adequately described science moves immediately to the construction and testing of possible explanations for what happens at that level. This will involve drawing on whatever cognitive equipment is available and perhaps the design of new experimental techniques and the invention of new sense-extending equipment. Once the explanation is discovered science then moves on to the construction and testing of possible explanations for it. At each level of reality law-like behaviour has to be interpreted normically, i.e. as involving the exercise of tendencies which may not be realised.

Empirical realism is underpinned by a metaphysical dogma, which I call the epistemic fallacy, that statements about being can always be transposed into statements about our knowledge of being. As ontology cannot, it is argued, be reduced to epistemology this mistake merely covers the generation of an implicit ontology based on the category of experience; and an implicit realism based on the presumed characteristics of the objects of experience, viz. atomistic events, and their relations, viz. constant conjunctions. (These presumptions can, I think, only be explained in terms of the need felt by philosophers for certain 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 tends to be out of joint with science.

It is argued in Chapter 1 that the very concept of the empirical world embodies a category mistake, which depends upon a barely concealed anthropomorphism within philosophy; and leads to a neglect of the important question of the conditions under which experience is in fact significant in science. In general this depends upon antecedent social activity. Neglect of this activity merely results in the generation of an implicit sociology, based on an epistemological individualism in which men are regarded as passive recipients of given facts and recorders of their given conjunctions.

Against this it is argued that knowledge is a social product, produced by means of antecedent social products; but that the

objects of which, in the social activity of science, knowledge comes to be produced, exist and act quite independently of men. These two aspects of the philosophy of science justify our talking of two dimensions and two kinds of 'object' of knowledge: a transitive dimension, in which the object is the material cause or antecedently established knowledge which is used to generate the new knowledge; and an intransitive dimension, in which the object is the real structure or mechanism that exists and acts quite independently of men and the conditions which allow men access to it. These dimensions are related in Chapter 3. Two criteria for the adequacy of an account of science are developed: (i) its capacity to sustain the idea of knowledge as a produced means of production; and (ii) its capacity to sustain the idea of the independent existence and activity of the objects of scientific thought.

It is the overall argument of this study then that knowledge must be viewed as a produced means of production and science as an ongoing social activity in a continuing process of transformation. But the aim of science is the production of the knowledge of the mechanisms of the production of phenomena in nature that combine to generate the actual flux of phenomena of the world. These mechanisms, which are the intransitive objects of scientific enquiry, endure and act quite independently of men. The statements that describe their operations, which may be termed 'laws', are not statements about experiences (empirical statements, properly so called) or statements about events. Rather they are statements about the ways things act in the world (that is, about the forms of activity of the things of the world) and would act in a world without men, where there would be no experiences and few, if any, constant conjunctions of events. (It is to be able to say this *inter alia* that we need to distinguish the domains of the real, the actual and the empirical.) Although the primary aim of this book is constructive, it is an important subsidiary aim to situate the conditions of the plausibility of empirical realism and to show it as depending upon what is in effect a special case. These conditions are briefly: a naturally occurring closure, a mechanistic conception of action and the model of man referred to earlier. The attempt to reduce knowledge to an individual acquisition in sense-experience and to view the latter as the neutral ground of

knowledge that (literally) defines the world results in the generation of an ontology of atomistic and discrete events, which if they are to be related at all (so making general knowledge possible) must be constantly conjoined. (Hence the presupposition of a closure.) On this view the causal connection must be contingent and actual; by contrast I want to argue that it is necessary and real.

Chapter 1 establishes the necessity for an ontological distinction between causal laws and patterns of events (see esp. 1.3) and contains a sketch of a critique of empirical realism (see esp. 1.6). Chapter 2 develops in detail the conditions required for the Humean analysis of laws and provides an analysis of normic statements (see esp. 2.4.). Determinism is shown to be an immensely implausible thesis; and the central tenets of orthodox philosophy of science – such as the principle of instance-confirmation (or falsification), the Humean theory of causality, the Popper-Hempel theory of explanation, the thesis of the symmetry between explanation and prediction, the criterion of falsifiability, etc. – to be manifestly untenable. Chapter 3 sets out to give a rational account of the process of scientific discovery; in which both nature and our knowledge of nature are seen as stratified, as well as differentiated (see esp. 3.3). A theory of natural necessity is developed which it is claimed is capable of resolving inter alia the problems of induction and of subjunctive conditionals and Goodman's and Hempel's paradoxes (see 3.6). Chapter 4 rounds off the argument and summarises some of the main themes of this study.

Moving towards a conception of science as concerned essentially with possibilities, and only derivatively with actualities, much attention is given to the analysis of such concepts as tendencies and powers. Roughly the theory advanced here is that statements of laws are tendency statements. Tendencies may be possessed unexercised, exercised unrealised, and realized unperceived (or undetected) by men; they may also be transformed. Although the focus of this study is natural science something is said about the social sciences and about the characteristic pattern of explanation in history.

If the first half of this work is concerned with establishing the necessity for an ontological distinction between causal laws and patterns of events and tracing the implications of the

distinction between open systems and closed, that is, of the differentiation of our world, the second is concerned principally with showing how science can come to have knowledge of natural necessity a posteriori. The differentiation of the world implies its stratification, if it is to be a possible object of knowledge for us. If generative mechanisms and structures are real then there is a clear criterion for distinguishing between a necessary and an accidental sequence: a sequence E_a . E_b is necessary if and only if there is a generative mechanism or structure which when stimulated by the event described by ' E_a ' produces E_b . If we can have empirical knowledge of such generative mechanisms or structures then we can have knowledge of natural necessity a posteriori. In showing how this is possible a non-Kantian 'sublation' of empiricism and rationalism is achieved.

In the transitive process of science three levels of knowledge may be distinguished. At the first (or Humean) level we just have the invariance of an experimentally produced result. Given such an invariance science moves immediately to the construction and testing of possible explanations for it. If there is a correct explanation, located in the nature of the thing whose behaviour is described in the putative law or the structure of the system of which the thing is a part, then we do have a reason independent of its behaviour as to why it behaves the way it does. Now such a reason may be discovered empirically. And if we can deduce the thing's tendency from it then the most stringent possible (or Lockean) criterion for our knowledge of natural necessity is satisfied. For example, we may discover that copper has a certain atomic or electronic structure and then be able to deduce its dispositional properties from a statement of that structure. We may then be said to have knowledge of natural necessity a posteriori. At the third (or Leibnizian) level we may seek to express our discovery of the electronic structure of copper in an attempted real definition of the thing. This is not to put an end to enquiry, but a stepping stone to a new process of discovery in which we attempt to discover the mechanisms responsible for electronic structure.

In 3.5 the grounds for inductive scepticism are examined and shown to be fundamentally mistaken and in 3.6 the problem, which arises from the ontology of atomistic events (and closed

systems), resolved. Dynamic realist principles of substance and causality are shown to be a condition of the intelligibility of experimental activity and the stratification of science. Science, it is argued, is concerned with both taxonomic and explanatory knowledge: with what kinds of things there are, as well as how the things there are behave. It attempts to express the former in real definitions of the natural kinds and the latter in statements of causal laws, i.e. of the tendencies of things. But it is concerned with neither in an indiscriminating way. It is concerned with things only in as much as they cast light on reasons; and reasons only in as much as they cast light on things. A realist theory of the universals of interest to science complements the realist theory of scientifically significant invariances, i.e. invariances generated under conditions which are artificially produced and controlled.

It is the argument of this book that if science is to be possible the world must consist of enduring and transfactually active mechanisms; society must consist of an ensemble of powers irreducible to but present only in the intentional actions of men; and men must be causal agents capable of acting self-consciously on the world. They do so in an endeavour to express to themselves in thought the diverse and deeper structures that account in their complex manifold determinations for all the phenomena of our world.

1. Philosophy and Scientific Realism

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

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. 176–77.

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 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 an 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

⁴ Thomson, *Notes of Lectures on Molecular Dynamics*, p. 132.

⁵ See A. S. Eve, *Rutherford*.

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'.⁵

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

⁶ Cf. I. Kant, *On the Distinctiveness of the Principles of Natural Theology and Morals*.

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 24 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 dimensions 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 succinctly 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 it's being experienced or experienceable 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

⁷ J. S. Mill, *A System of Logic*, Bk. III, Chap. 3, Sect. 1.

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 \rightarrow 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

⁸ Cf. N. R. Hanson, *Patterns of Discovery*, Chap. 1.

⁹ Cf. J. J. C. Smart, *Philosophy and Scientific Realism*, pp. 38–9.

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.

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.

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

¹² G. E. M. Anscombe, *Causality and Determination*, p. 22; and G. H. von Wright, *Explanation and Understanding*, pp. 60–64.

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

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.

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 informed¹⁵ 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

¹⁵ Cf. F. Dretske, *Seeing and Knowing*, Chap. 3.

¹⁶ Cf. G. H. von Wright, *op. cit.*, Chap. 2.

‘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 dis-

¹⁷ L. Wittgenstein, *Tractatus Logico-Philosophicus*, 6.35.

solution 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 principle 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

¹⁸ I. Kant, *Critique of Pure Reason*, B.724.

¹⁹ See e.g. A. J. Ayer, *Language, Truth and Logic*, pp. 31–41.

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

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 or 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).

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

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.

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.

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.

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

²⁷ See e.g. M. Hesse, *In Defence of Objectivity*. p. 14.

²⁸ See e.g. G. Buchdahl, *op. cit.*, p. 31.

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 strong *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

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 east 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 how 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.

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 re-constitution.

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

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*, p. 115.

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

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.

particular outcome. Such conditionals are *normic*,⁵ 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 nomic indicative form. A nomic 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

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.

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.

³⁶ Formally we could say that in experimental production by doing ϕ we change a 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 $\beta_1 \dots \beta_n$ 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.

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 con-

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

struction 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

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.

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 1.1 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,

Table 1.1

	Domain of <i>Real</i>	Domain of <i>Actual</i>	Domain of <i>Empirical</i>
<i>Mechanisms</i>	√		
<i>Events</i>	√	√	
<i>Experiences</i>	√	√	√

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

For empirical realism $d_r = d_a = d_e \dots$ (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).

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.

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 over-emphasize 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 meta-perspective 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 scientific 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

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.

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 Mendeleev'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

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.

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.

2. Actualism and the Concept of a Closure

1. INTRODUCTION : ON THE ACTUALITY OF THE CAUSAL CONNECTION

(i) 'We have no knowledge of anything but phaenomena; and our knowledge of phaenomena is relative not absolute. We know not the essence, nor the real mode of production, of any fact, but only its relations to other facts in the way of succession or similitude. These relations are constant; that is, always the same in the same circumstances. The constant resemblances which link phaenomena together, and the constant sequences which unite them as antecedent and consequent, are termed their laws. The laws of phaenomena are all we know respecting them. Their essential nature, and their ultimate causes, either efficient or final, are unknown and inscrutable to us.'¹

(ii) 'To give a causal explanation of an event means to deduce a statement, using as premises of the deduction one or more universal laws, together with certain singular statements, the initial conditions.'²

(iii) 'Since in a fully-stated D-N [deductive-nomological] explanation of a particular event the explanans logically implies the explanandum, we may say that the explanatory argument might have been used for the deductive prediction of the explanandum-event if the laws and the particular facts adduced in its explanans had been known and taken into account at a Suitable earlier time. In this sense a D-N explanation is a potential D-N prediction.'³

(iv) 'Criteria of refutation must be laid down beforehand: it

¹ J. S. Mill, *Auguste Comte and Positivism*, p. 6.

² K. R. Popper, *The Logic of Scientific Discovery*, p. 59.

³ C. G. Hempel, *Aspects of Scientific Explanation*, p. 366.

must be agreed which observable situations, if actually observed, mean the theory is refuted.⁴

(v) 'Important though other considerations may be, the acid test of a theory is its predictive power.'⁵

It is the argument of this chapter that there is a distinction between open and closed systems, which most existing philosophy of science ignores; that closed systems are a condition of its most important doctrines, such as those expressed in (i) – (v); and that once the significance of this distinction is grasped the plausibility of these doctrines collapses.

(i) – (v) possess a family connection, in that they all depend upon the Humean theory of law. This theory has often been criticized on the grounds that a constant conjunction of events cannot be sufficient for a law. But most of its critics have been content to allow that it is at least necessary.⁶ It is this notion, viz. that laws are constant conjunctions of events (plus some disputed contribution of mind), that I intend to challenge. It arises as follows: If atomistic events or states of affairs constitute the world then, for general knowledge to be possible, the relations between such events or states of affairs must be constant. (This is the assumption that the concept of a closure is designed to mark.) On the other hand if, as I intend to argue, they are not in general constant, then atomistic events cannot provide the only basis of ontology. And the philosophical theories based on the identification of causal laws with empirical regularities plus must all be radically wrong.

I shall use the term 'actualism' to refer to the doctrine of the actuality of causal laws; that is, to the idea that laws are relations between events or states of affairs (which are thought to constitute the objects of actual or possible experiences).⁷ Behind this idea of course lies the notion that only the actual (identified as the determinate object of the empirical) is real. Given it, the constant conjunction plus analysis of laws must

⁴ K. R. Popper, *Conjectures and Refutations*, p. 39, n 3.

⁵ J. Gibbs and W. Martin, *Status, Integration and Suicide*, p. 197.

⁶ See e.g. N. R. Hanson, *Observation and Explanation*, p. 45.

⁷ M. R. Ayers, *The Refutation of Determinism*, p. 6 and passim uses the term 'actualism' to refer to the doctrine that only the actual is possible. The connection between the two concepts will become clear in due course.

follow. In this chapter I shall not be concerned with the 'plus'. Moreover for convenience I shall use the term 'empiricism' in a generic way so as to cover the entire post-Humean tradition of empirical realism, and in particular both its positivist and neo-Kantian wings. No harm will be done by this usage as I am here attacking an assumption, viz. that a constant conjunction is necessary for a law, common to both.

The argument of this chapter is both simple and, I think, novel. Leaving aside astronomy, it is only under conditions that are experimentally produced and controlled that a closure, and hence a constant conjunction of events, is possible. The empiricist is now caught in a terrible dilemma: for to the extent that the antecedents of law-like statements are instantiated in open systems, he must sacrifice either the universal character or the empirical status of laws. If, on the other hand, he attempts to avoid this dilemma by restricting the application of laws to closed systems (e.g. by making the satisfaction of a *ceteris paribus* clause a condition of their applicability), he is faced with the embarrassing question of what governs phenomena in open systems. If he refuses the question, he is still left with the problem of accounting for experimental activity, and thus the establishment of 'laws', however restricted, in the first place. His only options here are to deny either that men are causal agents or that experimental activity plays any role in science. For if laws are sequences of events and men, being causal agents, can bring about and prevent such sequences, there can be no rationale for according one rather than another sequence the status of law. A sequence of events can only function as a criterion for a law if the latter is ontologically irreducible to the former. And so we come back to the argument of 1.3, where I showed how the intelligibility of experimental activity presupposes the ontological distinctiveness of causal laws from the patterns of events. But it can now be seen that not only the experimental establishment but the practical application of our knowledge depends upon this same ontological distinction. For unless causal laws persisted and operated outside the context of their closure, i.e. where no constant conjunctions of events obtained, science could not be used in the explanation, prediction, construction and diagnosis of the phenomena of ordinary life.

The empiricist makes matters worse for himself by the fact

that he not only ties laws to closed systems, but typically ties the activities of explanation, prediction and the identification of causes to our knowledge of laws. A *reductio ad absurdum* quickly follows. For to the extent that we seek to explain, predict and identify the causes of phenomena that occur in open systems, these activities become impossible. And to the extent that they are necessary for our social life, empiricism does. Thus there is no necessity that we should exist. But, given that we do, if our social life is to be possible we must ascribe causal responsibility in open systems. And given this, the Humean theory just cannot apply. Now I want to argue both that laws apply in open and closed systems alike; and, in a subsidiary thesis, that these other activities do not necessarily depend upon (though they may make use of) a knowledge of laws. From this perspective the Popper-Hempel theory of explanation, for example, may be seen to involve a double mistake: first, that explanation always involves laws; and secondly, that laws are or depend upon empirical regularities.

My overall aim, it will be remembered, is to argue that the ultimate objects of scientific understanding are neither patterns of events nor models but the things that produce and the mechanisms that generate the flux of the phenomena of the world. Scientists attempt to discover the way things act, a knowledge typically expressed in laws; and what things are, a knowledge (to be discussed later) typically expressed in real definitions. Statements of laws, I have suggested, are statements about the tendencies of things which may not be actualized, and may not be manifest to men; they are not statements about conjunctions of events, or experiences. But in developing this theory I do not attach any great importance to the word or even the concept 'law'. Rather what is essential to the realism developed here is the idea that the things and mechanisms of nature, that constitute the intransitive objects of scientific theory, both exist and act independently of the conditions, normally produced by men, that allow men access to them. For experimental science to be possible the world must be at least partially open. But if there is a real distinction between open systems and closed and our intuitions about the rationality of science are to be preserved there must be a real distinction between structures and events. In this respect the differentiation

of phenomena still provides the best argument for the stratification of the world.

In isolating the special conditions under which a regular sequence or pattern of events occurs; that is, in which (to adopt the realist mode) there is a correspondence between causal laws and the pattern of events, I will be leaving it up to the epistemologist whether he wants to sustain the universality of laws (and inter alia the intelligibility of experimental activity) by postulating a categorical ontological distinction between them. If this is done by the development of a non-empiricist ontology and an analysis of laws as non-empirical and normic along the lines indicated in 1.5 above, the way is also open for an adequate theory of natural necessity and natural kinds. On the other hand without this, I shall argue in Chapter 3, our intuitions about the lack of sufficiency of the Humean criteria for law (and the theories of science based on it) cannot in the last instance be sustained.

In showing how a closure is a condition of the intelligibility of empirical realism my primary intention in this chapter is critical. For it is when confronted with the fact and implications of open systems that the limitations of this approach to science – with its flat ontology of undifferentiated experience – become most apparent. But, in dealing with the problems posed by the largely unanalysed phenomena of open systems, I will also be compelled to develop new and more general alternatives to the theories, such as those expressed in (i) – (v) above, that are based on the tacit assumption that a closure is the universal rule in nature; rather than the rare exception I shall contend it is.

Underlying the widespread, if tacit, acceptance in philosophy of the idea of the ubiquity of constant conjunctions in nature (an idea which is not confined to the empiricist tradition)⁸ and hence of the doctrine of the actuality of causal laws is the notion that the universe is at rock bottom deterministic; that, in the image of Leibniz, the present is big (in the sense of pregnant) with the future; that it, as it were, already contains it now. It is the job of science to discover the iron laws that uniquely determine its motion. Once these laws are discovered, given only a knowledge of any complete state-description, ‘nothing would

⁸ Leibniz’s pre-established *harmony* of monads may be usefully compared with Hume’s *constant* conjunctions of atomistic events.

be uncertain [to science] and the future, as the past, would be present to its eyes.⁹ What accounts for the hold of this fantastic conception on our philosophical imagination? The philosophical arguments for it are, taken on their own, as we shall see, pretty poor. Why then do we feel the force of this picture? Partly no doubt because many things are de facto predictable, many processes are effectively isolated and many systems more or less closed; so that, given that rough-and-ready regularities are everywhere at hand, it seems plausible to suppose that underlying them there must be more exact ones. Partly no doubt because of an obsession with the consequences and a neglect of the conditions of the experimental paradigm, the single case that the hypothetico-deductive view of science fits. Above all perhaps because of the misconception created by the celestial closure secured by Newtonian science, and in particular by the idea that this closure embodied both a model of phenomena and a model of science. This was a double mistake. For it was not the human mind, as Laplace thought,¹⁰ that gave its special perfection to astronomy. Rather it was the peculiar conditions of the planets, and in particular the constancy of both their intrinsic states and the external forces on them, that made possible the observed regularities. Moreover for Newtonian, as for any other, mechanics celestial phenomena functioned merely as evidence that bodies tend to act in certain ways. The laws of motion, for example, describe actions which are unobservable in principle. But the tendencies of the bodies to which they apply are real; and would account for any disruption in the established order of our solar system.

But, it might be objected, is not the universe in the end nothing but a giant machine with inexorable laws of motion governing

⁹ P. S. de Laplace, *A Philosophical Essay on Probabilities*, p. 4. Positivists still pay obeisance to this concept of knowledge. See e.g. Brodbeck's characterisation of it, in an echo of Laplace, as 'perfect knowledge' (M. Brodbeck, 'Methodological Individualism: Definition and Reduction', *Readings in the Philosophy of the Social Sciences*, p. 289); and Hempel's wry admission that classical deterministic (i.e. Laplacean) systems conform 'best' to his model of explanation as deductive subsumption under universal laws (C. G. Hempel, *Aspects of Scientific Explanation*, p. 351). It is but a short step to identifying such systems with theories. (See e.g. M. Brodbeck, *op. cit.*, p. 288; C. G. Hempel, *ibid.*; and R. Rudner, *The Philosophy of Social Science*, p. 91) ¹⁰ P. S. Laplace, *ibid.*

everything that happens within it? I want to say three things: First, that the various sciences treat the world as a network of 'machines', of various shapes and sizes and degrees of complexity, whose proper principles of explanation are not all of the same kind as, let alone reducible to, those of classical mechanics. Secondly, that the behaviour of 'machines', including classical mechanical ones, cannot be adequately described, let alone understood, in terms of the 'whenever x, then y' formula of regularity determinism. Thirdly, that even if the world were a single 'machine' this would still provide no grounds for the constant conjunction idea, or a fortiori any of the theories of science that depend upon it. Regularity determinism is a mistake, which has been disastrous for our understanding of science.

6. REGULARITY DETERMINISM AND THE QUEST FOR A CLOSURE

So far I have been content merely to identify a closed system as one in which a constant conjunction of events obtains. But we must now establish exactly what this entails. It might be thought that the idea of a closed system could be elucidated quite simply as a fragment or sector of the world effectively cut off for a period of time from non-constant external influences. Although this gives one clear sense of 'a closed system' such a system would not necessarily satisfy the criterion of invariance implicit in the empiricist analysis of law. For one thing conditions would have to be placed on the individuals composing the system and the way in which the states of the system were to be specified. But even if this were done there would still be no guarantee that the criterion of invariance would be satisfied. The assumption that it would be depends upon the metaphysical thesis of regularity determinism. This may be defined as follows: For every event y there is an event x or set of events $x_1 \dots x_n$ such that x or $x_1 \dots x_n$ and y are regularly conjoined under some set of descriptions.¹¹ That is, the world is so constituted that there are descriptions such that for every event the simple formula, 'whenever this, then that' applies.

¹¹ The concept 'event' functions here syncategorematically. Its purpose is, in context, to generate the appropriate redescriptions of the events concerned.

Such a thesis stands to the practice of science as a regulative principle. Such principles are, as is well known, neither empirically nor theoretically refutable (or confirmable). But I will contend that they are metaphysically so. My procedure will be to see what this thesis entails about the nature of the world and about the nature of science; and to assess its adequacy in these respects in relation to other possible regulative standpoints. To do so I will work out critical or test conditions for the thesis of regularity determinism; that is, conditions such that *if* they were known to be satisfied and the constant conjunction formula was not vindicated the regularity determinist would be bound to admit his thesis refuted. In this way I hope to show just how restricted in its ontological presuppositions and restrictive in its methodological responses regularity determinism is. In developing these limit conditions for a closure I will thus be developing the conditions under which, on the supposition that regularity determinism is true, a constant conjunction of events *must* obtain. However, I will define a 'closed system' simply as one in which a constant conjunction of events obtains; i.e. in which an event of type a is invariably accompanied by an event of type b. Clearly the possibility of such a system does not depend upon the truth of regularity determinism. Nor need such a system be 'closed' in any more picturesque sense of the word.

Regularity determinism must be straightaway distinguished from two other forms of determinism: which may be called 'ubiquity' and 'intelligibility' determinism. Ubiquity determinism asserts that every event has a real cause; intelligibility determinism that every event has an intelligible cause; regularity determinism that the same (type of) event has the same (type of) cause. The concepts of 'cause' involved in the three determinisms are of course distinct. For the ubiquity determinist the cause is that thing, material or agent which is productive of an effect; for the intelligibility determinist it is simply that which renders an event intelligible to men;¹² for the regularity determinist it is the total set of conditions that regularly proceeds or accompanies an event.¹³ Of the three determinism, regularity

¹² See e.g. W. Kneale, *Probability and Induction*, p. 60.

¹³ See e.g. J. S Mill, *A System of Logic*, Vol. I, Bk. III, Chap. 3, Sect. 3, or A. J. Ayer, *Foundations of Empirical Knowledge*, Chap. 4, Sect. 17. As has been frequently pointed out, by Mill and Ayer among others, this

determinism is clearly the most restrictive; and ubiquity determinism is more general than intelligibility determinism, because it licenses no presumption that the real cause of an event will always be intelligible to men. The realist, intelligibility and regularity concepts of cause are of course naturally associated with the transcendental realist, transcendental idealist and classical empiricist philosophies of science.

All three determinisms must be distinguished from the idea of 'computational' determinism. This is the supposition that for each characteristic or trait of any material body at any moment of time there exists at least one set of statements from which, together with the relevant antecedent state-descriptions, that trait is deducible. It is important to realize that because no restriction is placed on the statements used in the deduction of traits, computational determinism is a *truism*. It says merely that given any system it is possible to work out an algorithm for the successful computation of its traits; or, in other words, that there is a consistent way of describing the development of any system over any finite period of time. Moreover in general there will be an infinite number of ways of doing so. It is therefore an uninteresting truism – save perhaps to remind us that the notion of disorder or chaos is always relative to a particular type of order or class of functions;¹⁴ and that the criterion of deducibility is too easily satisfied to be capable of functioning alone as a decision rule for the selection of 'law-like' or 'theoretical' statements and requires at the very least supplementation by criteria that place some restriction on the number, type or interpretation of the statements concerned.

It is especially important to distinguish regularity from computational determinism. For it is at least part of the intention of the former to assert (a) that the same cause and effect sometimes as a matter of fact recurs and (b) that the same cause and effect could always logically recur. For unless (a)

concept does not accord well with our normal usage; so in practice the Humean tends to modify it in the direction of the intelligibility concept by making the cause an individually critical factor in a jointly sufficient set. On the other hand, to the extent that the intelligibility theorist is committed to the doctrine of empirical realism he must rely on a background of empirical generalizations to justify his citation of the cause.

¹⁴ Cf. e.g. E. Nagel, *The Structure of Science*, p. 334.

were true instances *would* not fall under it and unless (b) were true they *could* not fall under it; so that it would be at best vacuous and at worst false. On the other hand computational determinism is consistent with 'law-like' formulations which are so specific and detailed as to reduce the practical likelihood of the event's recurrence towards zero or which mention the spatio-temporal location within which it occurred or which individuate it with a definite description or a proper name (e.g. The Battle of Edge Hill). We need not dwell on these possibilities here. For they have been thoroughly explored by philosophers concerned to defend the autonomy of historiography from (positivist) science.¹⁵ For the regularity determinist the necessity for such formulations merely indicates the ignorance of the describer. The anti-regularity-determinist, on the other hand, may take it as a sign of emergence or novelty in nature or even of the self-determination of some agent or structure. Unlike computational determinism, regularity determinism is not trivially satisfied. It does however share with it the feature that if there is one set of law-like statements which satisfies it there will be an infinite number of such sets. Hence it too requires supplementation by additional criteria, such as simplicity, intelligibility or realism, if it is to be capable of yielding a unique decision procedure for the selection of 'laws'.

The total cause (in Mill's sense) of an event will normally be a complex set of conditions $x_1 . . . x_n$ rather than a single event x . One could distinguish here between the individual or component events or states and the total or conjunct event or state; and refer to the case where more than one factor is at work, following Mill, as that of 'multiple causation'. In the same way the consequent event will also normally be complex, i.e. $y_1, . . . y_n$ rather than simply y ; and so we could talk of a corresponding 'multiplicity of effects'. There is a genuine 'plurality of causes' when the same effect arises from different (i.e. alternative) sets of conditions.¹⁶ Is a plurality of causes consistent with regularity determinism? Note it is consistent with

¹⁵ See e.g. P. Gardiner, *The Nature of Historical Explanation*; W. Dray, *Laws and Explanation in History*; C. B. Joynt and N. Rescher, 'The Problem of Uniqueness in History', *History and Theory*, Vol. 4; and M. Scriven, *op. cit.*

¹⁶ Cf. M. Bunge, *Causality*, p. 122.

predictability. For given a knowledge of the conditions which actually prevail, the effect is uniquely predictable. But it is not consistent with retrodictability. For given the effect, we cannot uniquely infer the cause. And this requirement is explicit both in the Laplacean ideal, which places the past on a par with the future, and the Humean definition of cause ('in other words where if the first object had not been, the second never had existed'), which makes the cause both necessary and sufficient for the effect.¹⁷ To satisfy this requirement the idea that every event has one and only one cause (or set of causes) must be incorporated into our definition of regularity determinism. This must now read: the same cause always has the same effect and the same effect always has the same cause; so eliminating both the possibility of a disjunctive plurality of causes and of a disjunctive plurality of effects.

Now suppose we had a system such that events of type a were invariably followed by events of type b. We could then say that a closure had been obtained. A closure is of course always relative to a particular set of events and a particular region of space and period of time. Now supposing that at some time *t'* an event of type a was not followed by an event of type b we would have to say that the system was 'open', our criteria of open-ness just being the fact that events of type a had not been invariably followed by events of type b *under their given descriptions*; i.e. the instability, in space or over time, of actually recorded empirical relationships. Should we say that the system had been closed but was now open or that it was open all along? There is nothing at stake here – it depends entirely on the time period for which 'the system' is defined: if and only if it includes *t'* it is open. 'System' here carries no independent semantic force. Either way the natural response of the regularity deter-

¹⁷ Cf. Newton's 2nd Rule of Reasoning in Philosophy: 'to the same natural effects we must, as far as possible, assign the same natural causes', *ibid.* And Hume: 'the same cause always produces the same effect, and the same effect never arises but from the same cause', *A Treatise on Human Nature*, p. 173. The rationale for this requirement lies in the counter-intuitive nature of the implication that the future be better known than the past. Moreover given the logical reversibility of the connective and the classical concept of time a disjunctive plurality of causes could be transformed into a disjunctive plurality of effects so as to produce a radical indeterminism in nature, i.e. given *x* then y_1 or $y_2 \dots$ or y_n !

minist to this situation would be to suppose that we had left out of our state-description an individual or variable that made a difference: that the conjunct events referred to under the same description 'a' before and after *t*' were not really the same in all relevant respects; in short that the system had been incompletely described (or enumerated). For example if the system was a classical mechanical one, where the presupposition was that mass, position and velocity were the only relevant variables, it would be natural to suppose that a relevant individual had been omitted from the specification of the overall state of the system, i.e. the total conjunct event.

To fix the point, imagine a universe composed of a finite number of different kinds of knives, forks and spoons. Suppose that we attempted to work out a general rule which would enable us to predict that when the knives and forks were in a certain position (including naturally the datum of whether they were on the dining-room table or in the kitchen drawer) they were invariably followed by another constellation of positions at the next meal. We might find this impossible, unless we took into account the positions of the spoons; so that we could say that our system had been incompletely described in the former case, owing to the omission of a causally relevant (in the Humean sense) variable. Of course in time we might find that we could only satisfy the demands of regularity determinism by taking into account further variables, e.g. the shapes and number of glasses at the meal; or even variables of an entirely different kind, e.g. the room temperature – to distinguish say between winter breakfasts when porridge was the rule and summer ones when grapefruit was.

Subject to an important qualification to be discussed below it can thus be seen that regularity determinism implies a particular kind of response to the phenomenon of open systems, i.e. to the instability of empirical relationships, viz. to assume that some causally relevant individual or variable has been left out of the description. On the other hand, even if this were the case, stable empirical relationships might still be possible as long as the values of the omitted variables remained constant. A closure thus depends upon either the actual isolation of a system from external influences or the constancy of those influences.

Assuming that a system was effectively isolated from non-constant external influences and regularity determinism was true would this then ensure the satisfaction of the 'whenever this, then that' formula? Let us suppose that we are interested in explaining (in the sense of Hempel and Hume) the behaviour of some individual N, say an elephant. Would a knowledge of the total antecedent state-description enable us to predict its behaviour? No – for if N is characterized by internal structure and complexity it may behave differently in the same external circumstances in virtue of its different internal states. Thus what happens when I prod an elephant depends at least in part upon what state it is in, e.g. whether it is asleep or not; and thus to that extent the total state of the universe, of which the elephant occupies a part, will be a variable. So either the absence or the constancy of internal structure must also be a condition for a closure. And the regularity determinist now has another possible response to the condition of open-ness, namely to assume that the individuals of the system have not been given a simple or atomistic enough description.

It is easy to see that an actual isolation and atomistic individuals will be preferred, on epistemic grounds, by the regularity determinist to constant external and internal conditions. For the regularity determinist has no warrant for assuming that these conditions will remain constant.¹⁸ Whether they do or not will depend upon a whole host of factors concerning which ex hypothesi he has no knowledge and about which he is not therefore in a position to make any kind of claim. Only with an actual isolation of atomistic individuals will the regularity determinist be able to categorically predict the future; without it, it always remains on the cards that an unpredicted change in the external circumstances of the system or the internal states of its individuals will occur so as to upset an established regularity and so render inapplicable any hypothetical predictions, formulated subject to two *ceteris paribus* clauses.

I have been tacitly assuming up till now that the overall states of the system can be represented as an additive function of the states of the individual components of the system. This

¹⁸ Regularity determinism does not make a claim about the constancy of conditions. Its claim concerns the constancy of the conjunction between conditions, whether conditions should happen to be changing or not.

represents a third kind of requirement for a closure. Here again a closure is possible if the principle of organization is non-additive, provided it remains constant; though here again the regularity determinist will prefer the alternative of additivity on epistemic grounds. Behind the assumption of additivity lies of course the idea that the behaviour of aggregates and wholes can always be described in terms of the behaviour of their component parts. The assumptions of atomicity and additivity are closely connected. For to say of some system that it is irreducible (in this sense) to its component parts is presumably to say that it must be viewed as a thing in its own right, at its own level (I am not concerned with the grounds for this now.) Conversely to suppose that it is always possible to give an atomistic description of *prima facie* complex things is to suppose that they can always be viewed as systems or parts of systems which can be analysed in terms of their component parts, conceived as atomistic individuals.

The critical conditions for a closure are set out in Table 2.1. The satisfaction of one each of the system, individual and organizational conditions is sufficient, on the supposition that regularity determinism is true, for a closure; but not necessary for it. If a recorded regularity breaks down the regularity determinist must assume that it is because one of these conditions is not satisfied. Until now in developing the conditions for a closure I have been using the categories 'internal' and 'external'. But the categories 'intrinsic', and 'extrinsic' are better in that they are not explicitly tied to a spatial characteristic and

Table 2.1
Limit Conditions for a Closure, i.e. for the Stability
of Empirical Relationships

Conditions for a Closure	(1) Epistemically Dominant Case	(2) Epistemically Recessive Case
(A) System	Isolation	Constancy of Extrinsic Conditions
(B) Individuals	Atomicity	Constancy of Intrinsic Conditions
(C) Principle of Organisation	Additive	Constancy of Non-Additive Principle

hence to things of a certain type. Thus the category 'intrinsic' includes some properties of things which lie outside their spatial envelope, e.g. a magnet's field, and others which cannot be identified spatially at all, e.g. a person's charm. And it excludes others which do lie within their spatial envelope, e.g. properties belonging to things of another type. 'Isolation' must also be interpreted metaphorically; but there is a sense in which 'atomicity' must be taken literally.

Now it is easy to see that once an actual isolation and an atomistic description are set up as norms two regresses are initiated, viz. to systems so vast that they exclude nothing and to individuals so minute that they include nothing. These regresses are typically manifest in research programmes, characteristic of positivistic science, which could be dubbed 'interactionism' and 'reductionism' respectively. It is clear that they can only be halted by constituting a level of autonomous being, somewhere between the universe and atomistic individuals. But for the empiricist committed to regularity determinism to do so involves an enormous risk. For it means he must be prepared to snap the Humean link that ties the justified performance of cognitive acts such as the ascription of causes to a knowledge of empirical invariances; and to say 'yes I know a is not invariably followed by b, yet a caused b here'.

These regresses generate notorious paradoxes in their wake. For since in the first case there are at the limit no conditions extrinsic to the system a full causal statement would seem to entail a complete state-description (or a complete history) of the world. Similarly as in the second case there can be no conditions intrinsic to the thing a causal statement entails a complete reduction of things into their presumed atomistic components (or their original conditions). In the first case we cannot make (or can at best only make in a pragmatic way) the distinction between causes and conditions; in the second case that between individuals and variables or between a thing and its circumstances. In neither case do we have the key concept of a causal agent; i.e. the thing that produced or the mechanism that generated, in the circumstances that actually prevailed, the effect in question.

Open systems situate the possibility of two kinds of possibility statements: epistemic and natural. The regularity determinist

can accommodate the former but not the latter. For he may allow that an event may be uncertain due to the describer's ignorance of the complete atomistic state-description necessary to deduce it. But he cannot allow that there is a sense to a statement about what an individual can do independently of whether or not it will do it. For natural possibility statements to be possible condition B1 must be unfulfilled; i.e. there must be complex things, possessing intrinsic structure, to which the natural possibility is ascribed. Without this, the distinction between a power and its exercise would be indeed, as Hume supposed, entirely 'frivolous'. So in part the issue between the regularity determinist and realist turns on the question of whether there are objects not susceptible of an atomistic analysis and in what way this is significant for science.

A special and very important case of the individual conditions for a closure is thus given by: B1' the absence of powers, which is dependent upon the absence of intrinsic structure (implied by atomicity); and B2' the constancy of powers, which is dependent upon the constancy of intrinsic structure. If there are complex things then it becomes important to distinguish between the subjects and the conditions of action. For conditions is an epistemic, not an ontological category. The conditions change, but they do not have the power to change. Only things and materials and people have 'powers'.

I have argued in effect in Chapter 1 that for experimental science to be possible the world must be open but susceptible to regional closures. Now corresponding to the view of the world as open and the view of the world as closed we have two entirely different conceptions of science. The transcendental realist sees the various sciences as attempting to understand things and structures in themselves, at their own level of being, without making reference to the diverse conditions under which they exist and act, and as making causal claims which are specific to the events and individuals concerned. And he sees this not just as a tactic or manoeuvre or mechanism of knowledge; but as according with the ways things really are, the way things must be if our knowledge of them is to be possible. The regularity determinist, on the other hand, will seek in his quest for a closure, if the very special conditions specified in Table 2.1 are not satisfied, to incorporate more and more elements

into his descriptions and/or to break down his units of study into finer and finer constituents in an effort to stabilize his field. And he must see this merely as an attempt to vindicate to himself the rule, which may be fairly styled a dogma, that like follows like or whenever x then y .

3. THE CLASSICAL PARADIGM OF ACTION

In developing the critical conditions for a closure I have argued that epistemological considerations tilt them in favour of the cases of atomicity and additivity, i.e. cases B1 and C1 in Table 2.1 above. I now want to bring out the concept of action implied by these conditions, which will be seen to be as special as the conditions themselves.

If individuals are atomistic, then all causes must be extrinsic. And if systems are lacking in intrinsic structure (i.e. are exhausted by the properties of the individuals composing them), then there can be no action at a distance (nor can distance be a variable in action), so all action must be by contact. But as atomistic individuals cannot contribute to or be affected by the action, it must consist in the communication of received properties. But as the only property possessed by such individuals is their position in space at a moment in time, the only property they can communicate is their motion, i.e. their movement through space in time.

Thus a particular physical conception of action, involving a *corpuscularian* view of matter and a *mechanical* view of causality, in which all causes are regarded as efficient and external to the thing in which the change occurs, is implied by the limit conditions for a closure. These form the twin pivots of what I am going to call the *classical paradigm of action*.

Now this paradigm has three variations: a physical one; a metaphysical one; and a distinctively positivistic epistemological one. It is important to keep them distinct. For the prestige of the epistemological variation, which underpins the Humean theory of laws, has stemmed in large part from its misidentification, by Locke and Newton among others, with the physical one.

On the *physical* concept, matter is viewed as composed of rigid corpuscles whose motion accounts for the aggregative and

observed behaviour of things. These corpuscles exchange momenta and redistribute velocities among themselves by impact; and they move through space according to the strict laws of mechanics. Action is seen as consisting in the impression of external forces upon these corpuscles, which merely pass on their received motion by direct impact. Events are nothing but the displacement of these basic units of matter in space and time, they are not the transformation of pre-formed substances. Large scale events or macroscopic changes are merely the surface effects of such displacements; qualitative variety and change are similarly the effects of different arrangements and motions of the corpuscles and their aggregates. The properties of aggregates are essentially the same as those of their component parts, though they can manifest themselves to observers in different, i.e. genuinely emergent ways (as in the case of secondary qualities).

Now this physical concept of action encourages, though it does not imply, a *metaphysical* concept, which was especially prominent in seventeenth century rationalist thought. On it, matter is viewed as essentially passive and inert (which is strictly speaking irreconcilable with the role played in classical mechanics by the concept of inertia); and causation is viewed as a process which is linear and unidirectional, as well as external and inconsistent with real novelty. In contrast to matter is mind. As matter is passive, mind is active. And qualitative variety and change, denied a place in the sphere of matter, are seen as contributions of the human mind.

Historically associated with this ensemble of physical and metaphysical ideas is another distinctively positivistic *epistemological* concept. On this concept, things are viewed as ultimately resolvable into simple qualities apprehended in sense-experience, rather than as aggregates of elementary units of matter in motion. The ontology is one of atomistic (and independent) events rather than one of atomistic (and rigid) corpuscles. And causality is seen as the regular concomitance of such events, rather than the impression of external forces upon such individuals. For the positivist, the concept of action is but a gloss put on our apprehension of such sequences. It can have no application in a world of independent events.

It should be noted that the epistemological variation on the

classical paradigm only makes full sense if it is given a phenomenalistic interpretation. For if and only if we start from punctiform sense-experiences and reconstruct the world from them, is it at all plausible to suppose that the world consists of independent and atomistic events and states of affairs, constituting the surrogates of such sense-experiences, with relations that are as external to one another as the sense-experiences that ground them are held to be distinct. Conversely, if we reverse this presupposition and situate sense-experience as a natural process occurring in the world then it always makes sense, on being told that some event or happening occurred, to ask: 'to what thing?' (One could then of course go on to explain the change, along the lines of the corpuscularian/mechanical programme, in terms of the arrangements and motions of the basic individuals.)

On the physical concept it is the principles of action (the strict laws of mechanics) that explain macroscopic behaviour, including observed regularities; i.e. the relationship between individuals and systems is one of explanation, not analysis. Now I have argued in §1 that these laws cannot be plausibly construed as empirical regularities or constant conjunctions of events. Moreover, if it is the behaviour of the individuals that is required to account for the constant conjunctions of events rather than the other way round the point of the Humean analysis of cause and the Hempelian analysis of explanation is lost. If the laws cannot be regarded as empirical regularities, the concepts in them cannot be regarded as given in or abstracted from experience. Firstly, because we ordinarily experience motion in terms of transitive verbs such as 'pushing' and 'pulling' which cannot be explicated ostensively.¹⁹ Secondly, because the concepts that figure in them demanded a radical break with the pre-existing Aristotelian scheme which certainly was then, and may still be now, closer to our ordinary way of thinking.²⁰ Consider, for example, the radical change embodied in the principle of inertia, that only change in motion and not motion itself (i.e. change in position) requires explanation. Thus the notion that 'the concepts of classical physics are just a refinement

¹⁹ Cf. J. R. Aronson, 'Explanation without Laws', *The Journal of Philosophy*, Vol. 66, (1969), pp. 541–57.

²⁰ See e.g. A. Koyré, *op. cit.*, Chaps 1–2.

of the concepts of daily life'²¹ must be rejected, and with it the notion that laws are inductive generalizations from everyday experiences or at any rate descriptions of them or instruments for predicting them abandoned. For the experiences are literally different – an idea which will only seem paradoxical to someone who is implicitly misusing the category of experience to define the world.

The key concept of atomicity is given radically different interpretations in the physical and epistemological variations. In the physical variation its primary identification is size; in the epistemological one simplicity. Physical atoms are (or were) necessarily unobservable, theoretical entities; epistemological ones were the raw data of experience. The former were the ultimate entities of the world, the latter the basic building blocks of knowledge. Physical atoms were explainers; epistemological ones justifiers. Physical atoms were Parmenidean individuals, epistemological ones Heraclitean instants.

Epistemic atomicity requires that events should be apprehended in raw sense-experience. Hence it restricts the possible numbers of a cause and an effect set to one. But the constant conjunction analysis of cause requires that the cause and effect properly so-called should be constantly conjoined. The requirements of epistemic atomicity and empirical invariance can only be reconciled if the sequence of events is a *linear* process and each event is a member of a homogeneous series of determinations. If this were the case then the apprehension of the cause event would indeed license the expectation of the effect event. But this leaves the odd assumption of the linearity of processes to be justified. In the formula 'whenever x then y', it is only if 'x' and 'y' are taken to refer to the objects of actual or possible experiences that we have an empiricist (in the broad sense) analysis of the causal modality; and it is only if these experiences are taken to be atomistic that we have a specifically Humean one. But it is only if 'x' and 'y' constitute complete as well as atomistic state-descriptions that the conjunction between x and y will (supposing regularity determinism to be true) be constant. These desiderata can only be reconciled if all causal sequences are linear processes; an idea which it would seem extraordinarily difficult to defend. Finally, it should be noted that whereas the

²¹ W. Heisenberg, *Physics and Philosophy*.

constant conjunctions are for the most part unknown (as the regularity determinist will be bound to concede), the events they are supposed to conjoin are regarded on the Humean analysis as being intuitively ascertainable!

I have dwelt at some length on the differences between the physical and epistemological concepts; as well as hinting in the last paragraph at internal difficulties in the latter (which will be developed in Chapter 3). I now want to say something about the general character of the paradigm as such and the limitations of its various ingredients. The essential features of the classical corpuscularian/mechanical world-view can be summarized as follows:-

- (i) the externality of causation;
- (ii) the passivity of matter, and the immediacy of effects;
- (iii) the atomicity of fundamental entities (whether corpuscles, events or sense-data);
- (iv) the absence of internal structure and complexity;
- (v) the absence of pre-formation, and of material continuity;
- (vi) the subjectivity of transformation and of apparent variety in nature (i.e. metaphysically, qualitative diversity and change are 'secondary qualities').

It has already been seen that at the limit conditions for a closure the distinction between intrinsic and extrinsic conditions collapses. For atoms possess no intrinsic conditions. An atom is distinguishable only with respect to its position (or some higher order derivative of position such as motion, acceleration etc.) in space at a moment in time. Consider now the old mechanistic prejudice crystallized in Hobbes' dictum that 'nothing taketh a beginning from itself'.²² Does this mean that nothing taketh *any part* of its beginning from itself; i.e. that none of the total set of necessary and sufficient conditions for an event is intrinsic to the thing (whether or not the trigger that sets it off, so to speak – what we should ordinarily call 'the cause' – is intrinsic)? If it does, this is tantamount to assuming that the event occurs, on the satisfaction of certain antecedent conditions, whatever the thing. It encourages, as it were, a picture of a cloud of smoke every time we pour out a drop from the bottle marked 'ACID',

²² T. Hobbes, *On Human Nature*, reprinted in *Body, Man and Citizen*, ed. R. S. Peters.

irrespective of the nature of the substance or material onto which it is poured; or the sound of breaking glass every time a cricket ball hits a stationary target at 30 m.p.h., irrespective of whether it hits a greenhouse, a sand pit or a granite wall. Such 'explanations' border of course on the fatalistic; or rather we could say that they are fatalistic with respect to things. For if there were substance or material conditions for the event such that were they not satisfied it would not have occurred, then they must be included as part of the total cause (in Mill's sense) of the event. Once we allow that an event would have occurred, whatever the intrinsic conditions, we are bound to end up denying the principle of material continuity. This is the principle that events are changes in things, never replacements of one kind of thing by an *entirely* new kind of thing.²³ But if all causes are extrinsic there can be no material continuity through change: either because there is no continuity (positivism) or because there is no change (corpuscularianism).

Ordinarily we think of the world as consisting of things which endure through some but not other changes. Events we think of primarily as changes in things, i.e. as the transformation of substances, rather than the displacement of physical masses in space and time. What is transformed is already given as complex and pre-formed. If it is partially transformed, material continuity is preserved through the change. If it is totally transformed, we seek for a new kind of substance, or level of 'thing', which will allow us to preserve this principle.²⁴ A chemical atom preserves continuity through chemical reactions; a gene-pool through species change; and historically, physicists have tended to treat as 'substantive' precisely that which tends to be conserved, e.g. matter or energy. In our ordinary ascriptions of change then a material as well as an efficient cause is normally essential. The classical paradigm directs exclusive attention to the latter. And yet in scientific contexts it is often the search for hidden entities, leading to the identification of novel kinds, set

²³ Cf. M. Bunge's genetic principle or principle of productivity: 'nothing can arise out of nothing or pass into nothing', *op. cit.*, pp. 25-6. Cf. also Kant's 1st Analogy of Experience, *op. cit.*; and W. H. Walsh, 'Categories', *Kant-studien Band 45* (1954), reprinted in *Kant*, ed. R. P. Wolff, pp. 54-70.

²⁴ Cf. S. Körner, 'Substance', *P.A.S. Supp. Vol. 38* (1964), pp. 79-90.

off by what appears *prima facie* to be a case of *ex nihilo* production, that is most important. (Cf. the discovery of the neutrino.) It is the absence of the notion of material continuity through change, as it is manifest in the epistemological variation of the classical paradigm, where it results in the generation of an ontology of atomistic and independent events, that underpins the idea of the contingency of the causal connection, which we turn to in the next chapter after rejecting the idea of its actuality here.

Perhaps under the influence of the classical paradigm we tend to view events as happenings to passive things. But events are also the results of actions. Panes of glass do not shatter without being hit by things such as cricket balls; nothing could be magnetized unless there were magnets. Thus some things must be agents as well as patients; just as some conditions must be intrinsic as well as extrinsic. Yet we feel the source, the stimulus, the trigger is always extrinsic. But this is a pure prejudice. For not all efficient causes are extrinsic and not all extrinsic causes are mechanical. Thus the structure of a field or the organization of an environment may be the cause of what happens within it. There is no reason why the properties of wholes should not explain those of their component parts. But neither is there any reason why all things should be conceived as either wholes or parts. I am going to suggest later that we are radically misled by spatial metaphor and imagery here. Societies, people and machines are not collectivities, wholes or aggregates of simpler or smaller constituents (just as intentionality is not an inner urge or push). In the classical world view it was the function of matter to occupy space;²⁵ so it was natural to assume that all 'things' properly so-called were just more or less highly differentiated aggregates of matter, and so could be viewed either as wholes or parts (or as both).

The victories of the corpuscularian/mechanical programme were never as complete as its propagandists made out. Neither gravity positing action at a distance nor magnetism in which distance was a variable in action could be assimilated to the paradigm. The fact that Newton could not find a contact explanation for gravity provided the basis for much early criticism of his work; and the theory of a connecting medium –

²⁵ Cf. M. Čapek, *op. cit.*, p. 54.

the aether – has its post-relativistic advocates even today.²⁶ Paradoxically the very fact that Newton could not find an explanation for gravity strengthened the positivist variant of the paradigm – for it gave a degree of credibility to the belief that science eschews hypotheses'.²⁷ Nevertheless the prestige of Newtonian mechanics was such that from Hume's time onward scientific explanation came to be identified with mechanical explanation; and a reduction of all other branches of learning to mechanics was loudly proclaimed by Helvetius and its other propagandists to be at hand. Of course we know that after mechanics had been fully developed and applied successfully in domain after domain there was bound to come a time when, under the stimulus of internal inconsistencies and irrefutable counter-instances, the principles of mechanics would themselves have to be explained non-mechanistically. Moreover we know as a matter of fact that the Newtonian system has been replaced and not just subsumed;²⁸ and that in one respect after another the classical world view has been abandoned by modern physics.²⁹ But given this, it is still important to ask (in view of its perennially attractive features) whether it provided a logically coherent schema for the conceiving of fundamental explanations in physics or indeed any other science.

Now both Boscovitch and Kant argued, anticipating later developments in physics, that contact explanations could not be fundamental; but that they themselves required explanation in terms of forces – of attraction and repulsion – acting at a distance.³⁰ For, they argued, since action by contact proceeds by compression, the bodies involved cannot be absolutely rigid. If they are, then there cannot be any transfer of motion. For a

²⁶ See e.g. I. Dirac, 'Is there an aether?', *Nature* 168, pp. 906–7.

²⁷ Cf: 'Hitherto we have explained the phenomena of the heavens and our sea by the power of gravity, but we have not yet assigned the cause of this power. . . . To us it is enough that gravity does really exist, and act according to the laws we have explained, and abundantly serves to account for all the motions of the celestial bodies, and of our sea.' I. Newton, *op. cit.*, Book III, General Scholium

²⁸ See e.g. P. K. Feyerabend, 'Problems of Empiricism', *Beyond the Edge of Certainty*, ed. R. G. Colodny, esp. pp. 168–70; and T. S. Kuhn, *op. cit.*, esp. pp. 99–103.

²⁹ See e.g. M. Čapek, *op. cit.*, Pt. II.

³⁰ R. J. Boscovitch. *A Theory of Natural Philosophy*, pp. 10–3 and 19–68 and I. Kant, *The Metaphysical Foundations of Natural Science*.

transfer depends upon the individuals being deformed to some extent. But basic individuals cannot be deformed. For to be deformed is to lose a property once possessed, but atoms have no properties to lose. Thus the most fundamental interactions cannot be by contact between corpuscles or atoms. But could there be non-mechanical, i.e. non-contact, interactions between atoms? If there could, atoms might still be the ultimate 'things'. Now any such action would of necessity have to be at a distance. But aside from spatio-temporal location (and its derivatives), the only attribute such 'atoms' could possess would be the power of affecting things at a distance. In short 'atoms' would be playing a purely nominal role. Such atoms would not be 'atoms' at all, but potentials or bare powers; that is, point-centres of mutual influence distributed in space.³¹

So far I have not discussed one important aspect of our normal concept of action; and that is the idea that things possess powers and liabilities³² to do and suffer things that they are not actually doing and suffering and that they may never actually do or suffer. It remains true to say of a Boeing 727 that it can (has the power to) fly 600 m.p.h. even if it is safely locked up in its hangar; just as it would remain true to say of a person that he could (would be liable to) get hurt if he happened to be in the way of a herd of stampeding buffaloes, even if as a matter of fact we knew he had no intention of ever going to the Prairies. The elucidation of this concept will occupy us much later on, so I will be brief with it here. It is sufficient for our purpose to note that it depends not only upon the idea of the complexity and pre-formation of things, which we found to be necessary to understand change, but upon the further idea of the stratification of their properties.

Now if things are at any moment of time complex and pre-formed then it always makes sense to suppose that they might have behaved in ways that they did not. But the 'might' here is susceptible of a purely conditional analysis, viz. as meaning merely that they would have behaved in those ways, if the actual conjunction of intrinsic and extrinsic conditions had in fact been different. The potentiality involved remains purely epistemic: it is still predicated essentially of events and only

³¹ Cf. R. Harré, *op. cit.*, p. 308.

³² By a 'liability' I mean simply what Hobbes called a 'passive power'.

derivatively of things. To say that a thing has a power to do something is, by contrast, to say that it possesses a structure or is of such a kind that it would do it, if the appropriate conditions obtained. It is to make a claim first and foremost about the thing; and only subsidiarily, if at all, about events. It is to say something essentially about what the thing is, and only derivatively about what it will do. It is to ascribe a natural possibility to the thing, whose actualization will depend upon the flux of conditions. An old Austin 7 can go 105 m.p.h. if it is towed by a Jaguar; but it does not possess the power to do so. And yet, if it did, the two events if conceived in themselves (without reference to the intrinsic structure of the thing) would be the same.

The ascription of powers differs from the simple ascription of complexity to things in that it presupposes a non-conventional distinction between those properties of the thing which are essential to it and those which are not. The essence of hydrogen is its electronic structure because it is by reference to it that its powers of chemical reaction are explained; the essence of money is its function as a medium of exchange because it is by reference to this that e.g. the demand for it is explained. Not all properties of a thing are equally important because it is by reference to some but not others that its causal powers are explained. In general it is these that constitute its identity and allow us to talk of the same thing persisting through change.

Now Locke was wrong to construe the fundamental distinction on which the corpuscularian/mechanical world view was based, viz. between the manifested qualities of things and the configuration and motion of their parts (by which the former were explained), as one between two types of qualities. For, in the first place, though some properties of the explanatory stratum, such as solidity and motion, could plausibly be modelled on the properties of observed things (so that one might say their ideas 'corresponded'), others, such as a point mass or a frictionless surface, had no such analogues. Thus Locke did not sufficiently appreciate that their concepts could not be read directly onto experience, but rather had to be produced as a result of the theoretical work of science (when they might subsequently come to inform and direct experience). Secondly, Locke should have articulated the distinction as one between the causal powers that material things possess in virtue of their hypothesized internal

structure and the manifestation of those powers, including their manifestation as the qualities of observed things in sense-experience. If he had done this, it would have obviated the need to find an immediate reflection in sense-experience of the hypothesized basic or 'primary' properties of the world – a move which provided the grist for Berkeley's attack on his ontology and has been the staple diet of phenomenologists ever since. At the same time, it would have both left open the possibility of a qualitative account of primary properties and removed the necessity to view the latter as ultimate. This would also have undermined the basis of his scepticism over a knowledge of real essences. Locke's pessimism about the possibility of future technological and theoretical advance is really the inverse of his philosophical under-statement, implied by his theory of ideas, of the conceptual advances made by the corpuscularian/mechanical world-view. For unless a philosophy of science acknowledges the existence of a past and the possibility of a future, it cannot pay tribute to the achievements of the present.

Science has travelled far since Bacon could take it as axiomatic that 'nature knows only mechanical causation, to the investigation of which all our efforts should be directed' and Boyle roundly declare that 'the phenomena of nature are caused by the local motion of one part of matter hitting against another'. Philosophy unfortunately has lagged behind. We have seen how the two basic interactions of the Newtonian system contributed – the one by its success, the other by its failure – to the grip of positivistic epistemology. Paradoxically, the very breakdown of the system coincided with, and indeed contributed to, a revival of positivistic-sensationalist and operationist thought. For one thing the new 'fundamental entities' seemed both event-like and statistical in character. And the apprehension of scientific change seemed merely to underline the necessity for a subjectivist ontology. (Later I shall show how, on the contrary, scientific change provides in fact the best possible argument for the objectivity of things.) As for the programme of reduction, historically associated with the corpuscularian/mechanical world-view, two questions raised by it survive the eclipse of that world-view: First, whatever the nature of the basic entities postulated by physics at any time, how are we to understand the status of apparently emergent things and properties? (Are

people, for example, metaphysically 'secondary qualities'?) And how are we to understand the phenomena of diversity and change? Secondly, what does it mean to say that the subject matter of one science can be 'reduced to' (in the sense of 'explained in terms of') that of another? How does such a reduction proceed? And how, if at all, does it affect the ontological status of the entities of the reduced science?

Finally, something should be said about the significance of the psychological studies of Piaget, Michotte and others for the classical paradigm.³³ These studies show that we ordinarily experience mechanical causation, i.e. the displacement of physical masses in space and time, in terms of transitive verbs such as 'pushing' and 'pulling' which cannot be explicated ostensively; but rather embody an intensional relationship between cause and effect. Such verbs take an objective complement: We understand 'pushing' as 'pushing away', 'pulling' as 'pulling towards'. Jack does not just fall, he falls down; and the 'down' is an essential part of what the 'falling' means. Ink bottles do not only get knocked, they get knocked over; doors do not only get slammed, they get slammed shut. And this is how we come to understand the meaning of these verbs.³⁴ Now it seems to me that this raises points against Hume, though not against Newton. In the first place, it undermines Hume's psychological account of the genesis of our idea of 'connection'. For it is now not regular concomitances, but completed movements, transitively understood, that provide the source of the latter. Secondly, it shows that the concepts in terms of which scientists come to understand motion could not have been given in or abstracted from sense-experience (a) because our ordinary concepts cannot be explicated ostensively; (b) because Newtonian ones cannot either; and (c) because the concepts are radically different anyway. Thirdly, it shows the poverty of the Humean account of our understanding of the basic interactions of mechanics. Because we have to ask why for three hundred years scientists and philosophers found this paradigm so compelling. And if today we must resist this compulsion, it is as well to be aware of the source of its power.

³³ See J. Piaget, *The Child's Perception of Physical Causality*, and A. Michotte, *The Perception of Causality*.

³⁴ Cf. J. R. Aronson, *op. cit.*, pp. 551-5.

4. ACTUALISM AND TRANSCENDENTAL REALISM : THE INTERPRETATION OF NORMIC STATEMENTS

In §2 the critical conditions for a closure have been developed and in §3 the concept of action implied by them has been brought out. Now two questions of great significance may be asked of any closure:

- (i) are the conditions for the closure universally satisfied or is the antecedent of the law-like statement for which the closure is defined instantiated in some open system?
- (ii) were the conditions for the closure artificially produced or did they occur naturally or spontaneously, i.e. without the active intervention of men?

One could usefully distinguish here between ‘universal’ and ‘restricted’ closures; and between ‘artificial’ and ‘spontaneous’ ones. Only a universal closure is consistent with the empiricist concept of a law as a universal empirical regularity. For to say that the antecedent is instantiated in an open system is just to say, according to the criterion of §2 above, that given the antecedent the consequent fails to materialize on at least one occasion in the space-time region for which the system is defined. In general if a closure has been artificially established it cannot also be universal.³⁵ It is of course precisely the ubiquity of open systems in nature that makes necessary an experimental rather than a merely empirical science. Once this is accepted, the idea of invariance over space-time must give way to the idea of invariance under experiment as a criterion of the empirical basis of a science. Moreover, strictly speaking, the invariance is that of a result, not a regularity. In general the result will be invariant *to* space and time, but not over them. On the other hand, it is clear that if the notion of laws as universal empirical

³⁵ However a closure might be both universal and artificial if a generative mechanism had endured as a latent potentiality of nature until awakened by science under experimentally controlled conditions or if it had never been activated in its experimental range. But to the extent that the sciences are concerned with structures that not only exist but act independently of them (and so explain what goes on in the world outside the laboratory) the first possibility will be exceptional; and to the extent that they are concerned with the conditions under which these structures act the second possibility will.

regularities is retained, then the same logic that led to the regress of interactionism will lead to the demand for a closure of all interacting systems until – if everything is assumed to be in interaction – we have what might be called a ‘global’ or ‘Laplacian’ closure. (Such a slide can only be avoided if it is supposed that a non-interacting eternally closed system can be found – without this affecting anything in the system.)

Now confronted with the instantiation of the antecedents of laws in open systems, i.e. in systems where their consequents are not invariably realized, the empiricist must abandon *either* the laws or his concept of them, viz. as universal empirical generalizations. For whatever is empirical must be actual. And in open systems laws if they are to be actual cannot be universal; and if they are to be universal cannot be actual. So he must say either that they are not laws; or that laws are not universal; or that laws are not empirical. The first position, which may be characterized as ‘*strong actualism*’, was in effect adopted by Mill in his doctrine of laws as ‘unconditional sequences’.³⁶ The trouble with it is that there are no unconditional sequences known to science. The second position, which may be characterized as ‘*weak actualism*’, involves restricting the application of laws to closed systems. This may be done by making the satisfaction of a *ceteris paribus* clause a condition of the law’s applicability. The trouble with it is that it leaves unanswered the question of what governs phenomena in open systems. Moreover it cannot provide a rationale for either the experimental establishment or the practical application of our knowledge (ironically in view of its sponsorship by self-styled ‘empiricists’ and ‘pragmatists’). The third position is that of *transcendental realism*. It rejects the idea, common to both forms of actualism, that laws are empirical statements or statements about events. Instead, it regards them as normic or transfactual statements that apply in open and closed systems alike. On this view, closures are important in the experimental establishment of our knowledge. But they do not affect the ontological status of laws. On the contrary, the transcendental realist asserts, it is just because the things to which laws are ascribed go on acting in their normal way independently of whether or not a closure obtains that the scientific investigation of nature is possible.

³⁶ J. S. Mill, *A System of Logic*, Vol. I, p. 378.

The empiricist, when confronted with the phenomena of open systems, i.e. the non-availability of universal closures, is faced with the trilemma of choosing one of the forms of actualism (which involves either preserving his philosophical integrity at the expense of science or abandoning his integrity to justify science) or succumbing to transcendental realism. My strategy will be to argue that weak actualism is not a genuine alternative and if pushed must collapse into one of the other two.

One way of describing these options is in terms of their different responses to the identifying mark of an open system, viz. the non-realisation of the consequent, given the instantiation of the antecedent of a law-like statement. For the strong actualist this means that the statement must be false, for the weak actualist it may be inapplicable, for the transcendental realist it can be both applicable and true. It must be false for the strong actualist because a law-like statement asserts the invariance of the conjunction between antecedent and consequent. It may be inapplicable rather than false for the weak actualist, if the *ceteris paribus* clause, subject to which it is regarded as being formulated, is not satisfied. It can be both applicable and true for the transcendental realist, if it correctly describes the working of a generative mechanism and the mechanism was really at work in that instance. Moreover for the transcendental realist the statement can be *known* to be both applicable and true, namely if the statement has been independently verified (e.g. under experimentally closed conditions) and there is no reason to suppose that the nature of the thing possessing the tendency whose operation is described in the law has changed.

The weak actualist is immediately faced with a problem here. For although the law-like statement may be inapplicable, viz. if the CP condition was not satisfied, rather than false, viz. if it was satisfied, there is no way on actualist lines that he can decide between these alternatives. The way in which this is normally settled is to see if the consequent is realized; if the consequent is not realized this means that the CP clause was not satisfied. But this involves using the law (thus presupposing both its truth and applicability – the latter in virtue of the satisfaction of the explicitly mentioned antecedent conditions) as a criterion of the stability of the circumambient conditions. Hence any attempt to use the stability of the circumambient conditions as a criterion

of the applicability of the law is viciously circular – as the law’s applicability would be already presupposed in the test for the stability of the conditions. The situation in which the weak actualist finds himself has been expressed as follows:-

When a prediction turns out to be false, the situation as regards the general laws used in making it is indeterminate: it cannot be known with certainty whether one or all the general laws have been disconfirmed or whether the *ceteris paribus* condition has not been fulfilled.³⁷

It might be thought that the situation would be improved if an independent means of verification for the law was available. But supposing a law were experimentally verified its use in open systems would presuppose a general principle sanctioning the applicability of laws when their consequents were unrealized. But this is precisely what is in question here and what both forms of actualism deny.

Now the strong actualist can only justify the retention of a law-like statement whose antecedent is instantiated in an open system as a temporary proxy or stand-in for the yet to be discovered unconditionally universal statement. But can the weak actualist do any better? For we are bound to ask him: if laws are restricted to closed systems what governs or accounts for phenomena in open ones? His options here are limited: either nothing does or something does. The former entails complete indeterminism. But the latter sets the weak actualist on the road to strong actualism. For to suppose that something accounts for the phenomena and to hold that current laws are inapplicable only makes sense on the assumption that open systems may be eventually closed. So it seems that the incomplete or non-atomistic descriptions that we currently call ‘laws’ must be replaceable in time by complete atomistic ones which (given only that regularity determinism is true) will after all be both strictly universal and still empirical. So that the weak actualist too comes to regard present ‘laws’ as temporary stand-ins for the Laplacean hour.

The trouble with weak actualism is that it is prepared to acknowledge the fact of open systems without generating the means for science to cope with it; that is, it is prepared to differentiate but not to stratify reality, thus removing the possi-

³⁷ E. Grunberg, ‘The Meaning of Scope and External Boundaries of Economics’, *The Structure of Economic Science*. ed. S. R. Krupp, p. 151.

bility that in our ascription of laws we are referring to a way of acting or a level of structure that is not confined to closed Systems. The necessity to view the satisfaction of the CP clause as a condition of a law's applicability vanishes once we realize that it is precisely a key function of the concept of law to apply transfactly, in open and closed systems alike. The satisfaction of the CP clause is, on the other hand, a condition for a decisive test situation (its verification depending necessarily upon the applicability of 'auxiliary' or bridge laws). But the truth of any normic statement is in general determined quite independently of, and antecedently to, its explanatory and other uses in open systems.

Given only a knowledge that the antecedent is instantiated and the absence of specific reasons for supposing that the tendency is no longer possessed by the thing we can then be justifiably sure that the tendency is being exercised or as it were in play; although only if we have grounds for supposing the system closed does that certainty license the prediction of its fulfilment. The citation of a law presupposes a claim about the activity of some mechanism but not about the conditions under which the mechanism operates and hence not about the results of its activity, i.e. the actual outcome on any particular occasion. This will in general be co-determined by the activity of other mechanisms too. Indeed it is precisely because it is non-committal about the nature of the circumambient conditions that a statement of law does not in general justify a claim about events, let alone experiences.

Strong actualism regards the appearance of open systems as a mark of ignorance and initiates interactionist and reductionist regresses in an attempt to overcome it. Weak actualism acknowledges the de facto existence of open systems but then proceeds to fence them off from science. For strong and weak actualism alike, open systems fall outside the pale of science. In this way empiricism understates its potential scope of application. Lacking from both forms of actualism is the concept of generative mechanisms which endure, so that the laws they ground continue to prevail, in open and closed systems; so making possible the scientific understanding of things and structures which exist and act quite independently both of our descriptions and the exercise of our causal powers.

Braithwaite falls into the same trap as weak actualism by arguing that a tendency statement is a conditional with an unspecified antecedent.³⁸ For if it is unspecified we cannot know when to apply it. It is in fact vital to distinguish the explicit conditions in the protasis of the law-like statement from the unknown conditions that the CP clause may be required to cover. The satisfaction of the former is a condition for the applicability~ of a law. But neither a knowledge (strong actualism) nor the stability (weak actualism) of the latter can be a condition for the applicability of a law. There are three reasons for this. First, it is in principle impossible to specify all the conditions that the CP clause may be required to cover. Indeed, if one could do so, there would be no need for the CP clause in the first place. Second, as has been seen, the satisfaction of the CP clause cannot normally be verified independently of the actualization of the consequent; hence to make it a condition for the applicability of the law is circular. Thirdly, as the satisfaction of the CP clause is time-dependent (being trivially satisfied instantaneously), acceptance of it as a condition for the law's applicability generates absurd and totally counter-intuitive results. For example, on it a law may be applicable for every five-minute interval in a day, but not for the day overall. The proper place of the phrase 'other things being equal' is not as part of the protasis but at the tail-end of the statement as a reminder that, because the system in which the thing's behaviour occurs may not be closed, the tendency postulated in the statement may not be actualized.

Satisfaction of the CP clause is not a condition for the applicability of a law. It is, however, a condition for the actualization of the tendency designated in the statement (for which it is sufficient, although not strictly necessary). And from this are derived its two main roles: first and foremost, as a signal of the normic nature of the proposition being expressed, as a reminder that the tendency designated may not be actualized; and secondly and derivatively, as a warning to historicists and pseudo-falsifiers, cautioning the former that the prediction of the tendency is not deductively justified and the latter that if the tendency is unfulfilled the statement should not – on that ground alone – be held to have been falsified. Thus the CP clause does not place a condition on explanation, for one can explain an

³⁸ R. B. Braithwaite, *Scientific Explanation*, p. 302.

event in terms of tendencies when the latter are never realized. Rather it places a condition on prediction and falsification.

This account needs qualifying in two ways. First, if we distinguish between the constancy of intrinsic and extrinsic conditions (as suggested in §2 above) and between the constancy of more and less important intrinsic ones (as suggested in §3) then the constancy of intrinsic structure is a condition for the applicability of a law. Tendencies are only possessed, and hence can only be exercised, as long as the nature of their possessor remains unchanged. But this does not vitiate my account. For law-like behaviour is predicated essentially of things, which are typically referred to in the protasis. There is a real asymmetry, which is reflected in the structure of law-like statements, between the intrinsic structure or essential nature of a thing (which in general constitutes its identity or fixes it in its kind) and the conditions under which it acts in that a change in the former but not the latter leads to a change in the thing's tendencies, liabilities and powers.

Secondly, I have said that the CP clause functions as a reminder and a warning. But such reminders and warnings are only necessary as long as law-like statements continue to be formulated and thought of in the actualist mode. If there were no historicists or pseudo-falsifiers there would be no need for reminders to them. Hence a fully realist philosophy of science could in principle dispense entirely with the CP clause (at least in this aspect of its work).³⁹ For whatever is conveyed by 'This happens CP' can be equally well conveyed by 'This tends to happen'. (To add CP to this statement would be to qualify the tendency, not its fulfilment.) This is not a shallow, equivocal, sloppy or mean formulation; but the logical form of all the laws of nature known to science.

I want to turn now to consider in more detail the character of normic statements. A full analysis of the logic of tendency statements must, however, be postponed until Chapter 3.

On the view of science advanced here, power and tendency statements are categorical rather than, as maintained by Hume and Ryle, hypothetical. Hypotheticals provide the empirical

³⁹ There is another possible use for a *ceteris paribus* clause, viz. as a protective device in the early stages of a science's development. This will be considered in Chapter 3.

grounds for our ascriptions of powers and tendencies, but they do not capture their meaning. Tendencies are roughly powers which may be exercised unfulfilled. They are thus well adjusted to cope with open systems. If a system is closed then a tendency once set in motion must be fulfilled. If the system is open this may not happen due to the presence of ‘offsetting factors’ or ‘countervailing causes’. But there must be a reason why, once a tendency is set in motion, it is not fulfilled; in a sense in which it would be dogmatic to postulate that there must be a reason why the tendency is set in motion. Once a tendency is set in motion it is fulfilled unless it is prevented.

The following is my interpretation of the mode of application of lawlike statements. Such statements, when their initial conditions are satisfied, make a claim about the activity of a tendency, i.e. about the operation of the generative mechanism that would, if undisturbed, result in the tendency’s manifestation; but not about the conditions in which the tendency is exercised and hence not about whether it will be realized or prevented. Because the operation of the generative mechanism does not depend upon the closure or otherwise of the system in which the mechanism operates, the mode of application of lawlike statements is the same in open and closed systems; what does differ is the inference that can be drawn from our knowledge of the applicability of the statements in the two cases. Notice that although the application of a normic statement warrants a subjunctive conditional about what would have happened if the system were to have been closed, the full force of its meaning cannot be understood or captured in this way. It has to be interpreted categorically and indicatively to the effect that a generative mechanism was really at work; which helps to account for, though it does not completely determine, whatever actually happened.

The ‘thing’ which possesses the tendency is not necessarily the same ‘thing’ as that whose behaviour is recorded in the lawlike statement. Indeed it is characteristic of science to postulate novel entities as the bearers of the tendencies and powers manifest in the behaviour of observed things. The class of ‘things’ is far wider than that of ‘material objects’: it includes fluids, gases, electronic structures, fields of potentials, genetic codes, etc.; so we must try to divest the concept of its normal material

object connotations. The idea of a tendency exercised unfulfilled seems strange if we think of ordinary material objects such as tables and chairs. People provide in this respect a better model for the entities discovered and investigated by science. There is nothing mysterious about tendency ascriptions to people. We know what it is like to be in a situation where we tend to lose our patience or temper and we know what it is like keeping it. Tendencies exercised unfulfilled; shown, perhaps, but unrealized in virtue of our self-control.

Now when a tendency is exercised unfulfilled two things are not in doubt: (a) that something actually happens, towards explaining which the exercise of the tendency goes some way; and (b) that something is really going on, i.e. there is a real generative mechanism at work, which accounts for the influence of the factor the tendency represents in the generation of the event. In the case of (a) there are two conceptual traps. The first is to think of the exercise of the tendency unfulfilled as an action without results, rather than as an action with modified results. Something does happen; and the tendency, as one of the influences at work, helps to explain what. The second is to think of it as if it were an action fulfilled, i.e. in terms of its fulfilment. It is a mistake to think of the exercise of a tendency in terms of the imagery, metaphors or descriptions appropriate to its fulfilment. Yet Mill in his unofficial doctrine of tendencies in effect does this when he argues that 'although two or more laws interfere with one another, and apparently frustrate or modify one another's operations, yet in reality all are fulfilled, the collective effect being the exact sum of the causes taken separately'.⁴⁰ Mill's mistake here is to suppose that whenever a tendency is set in motion the effect must be in some sense (or in some realm) occurring (as if every time we ran fast we had to be in some way winning). But Geach (and following him Ryan) in ridiculing this position make the converse mistake of supposing that whenever no effect (of a given type) occurs, nothing can be in motion or really going on.⁴¹ But here Mill is right and Geach is wrong. Balaam's ass is pulled in two ways; we do just

⁴⁰ J. S. Mill, *op. cit.*, Bk. III, Chap. 10, Sect. 5.

⁴¹ P. T. Geach 'Aquinas', *Three Philosophers*, G. E. M. Anscombe and P. T. Geach, p. 3; and A. Ryan, *The Philosophy of John Stuart Mill*, pp. 65-6.

manage to keep our tempers; the market equilibrium is explained in terms of an exact balance of buying and selling; when the beam finally collapses it is due to the real cumulative effect of the woodrot. Mill's mistake is to think of the exercise of the tendency under the description of its fulfilment, as if Balaam's ass, in order to be pulled two ways, had actually to go in both directions. Geach's mistake is to suppose that because neither tendency is fulfilled neither tendency can be in play. In other words, they both make the mistake of seeing the fulfilment of a tendency a condition of its exercise.

Let me stress that the scientist's situation is such that he is never in any doubt that given an effect *something* is producing it; his doubt is only over *what* is. Now clearly this does not mean that he is committed to a realist interpretation of every theory; what it does mean is that as a theorist his task remains essentially incomplete until he has produced a theory which correctly describes the mechanisms by means of which the effect in question is produced. It is in this light that other possible interpretations of normic statements must be considered.

It is misleading to think of normic statements as 'idealizations' or 'abstractions'. For both concepts conceal a crucial ambiguity as to the object idealized or abstracted from, in which the superior reality of events or experiences is tacitly assumed. The conception of the generative mechanism or structure that backs a normic statement need not be 'idealized' or 'abstract' in relation to really existing or the reality of existing structures. Once the necessity for a redefinition of the objects of a science as structures rather than events is accepted then the concept of an idealization must be used in relation to the reality of that object. And it cannot be assumed that all theoretical statements are idealizations in this sense. A model of the intrinsic structure of an atom or a DNA molecule or the solar system is not necessarily more perfect than the intrinsic structure of a real atom, DNA molecule or solar system. The standard of perfection is not set by men. Of course if one takes 'theoretical' as a synonym for 'unreal' (or at any rate 'less real') normic statements will appear as 'ideal' in that the tendency they designate or mechanism they describe is rarely if ever manifest in unmodified form;⁴² and as 'abstract' in that they select from what

⁴² See e.g. E. Nagel, *op. cit.*, p. 493.

is in open systems a mesh of influences and cross-influences just one as the focus of attention.⁴³ But to think like this is to fall into the error of supposing that events are more real than the structures and mechanisms that generate them.

Scriven makes a similar mistake in contending that normic statements are 'guarded generalizations'.⁴⁴ The only thing one need be 'guarded' about in using a normic statement is the assumption that the tendency whose activity is designated in the normic statement will be realized. If such statements have been independently and well confirmed (under experimentally closed conditions) then we may be completely and rationally confident in using them. Such confidence is expressed in, rather than weakened by, our willingness to use the CP clause against naive actualist objections on their behalf. It is only if one tacitly views law-like statements as in the final analysis empirical generalizations that one will feel that (because in asserting a law-like statement one is asserting the realization of the consequent), if one cannot be sure of the realization of the consequent then one can only assert the law-like statement 'guardedly'. But of course in asserting a normic statement one is not asserting the realization of the consequent; but the operation of a mechanism irrespective of its results (which it is precisely the function of the normic statement to be non-committal about).

Both these ideas depend upon an implicit recognition that reality is differentiated in a way that classical empiricism ignores and so requires something more of science than it provides. But the possibility opened up by this recognition is constrained by a continuing commitment to empirical realism. It is this which prevents the acknowledgement that reality is not only differentiated but stratified too. Once the stratification of the world is grasped it is possible to see how our knowledge can be both universally applicable and rarely (empirically) instantiated; and so to resolve Poincaré's problem that 'on the one hand, [laws] are truths founded on experiment and approximately verified so far as concerns isolated systems. On the other hand, they are postulates applicable to the totality of the universe and regarded

⁴³ Cf. Weber's concept of an 'ideal type' as a one-sided exaggeration of an aspect of 'concrete', i.e. empirical, reality. See e.g. M. Weber, *Methodology of the Social Sciences*.

⁴⁴ M. Scriven, *op. cit.*, p. 466.

as rigorously true'.⁴⁵ Normic statements speak of structures not events, the generator not the generated. In asserting a normic statement one is not making a guarded or idealized statement about an empirical reality. Rather one is making a statement, which may be 'guarded' or 'idealized' in its own right, about a different level of reality. Normic statements are not second best kind of empirical generalizations. They are not empirical statements at all.

Two further misinterpretations of normic statements must be guarded against. Normic (or transfactual) statements are not counterfactual statements. They legitimate the latter; and, like them, are only validatable in relation to an antecedently and independently established body of theory. But whereas to say that a statement is a counterfactual is just to say that the conditions specified in the antecedent do not obtain; in the case of a normic statement these conditions may obtain, and if they do (and the statement has been independently verified) it can then be interpreted quite straightforwardly as a statement about what is really going on though in a perhaps unmanifest way. (In the case of counterfactuals antecedents are by definition unsatisfied; in the case of transfactuals it is contingent whether consequents are realized.) It is only if the CP clause is regarded as a component of the protasis that it is plausible to interpret a normic statement as making, in its open systemic uses, a counter-factual claim. This is a position, most naturally associated with weak actualism, that has been argued against above. Normic statements have also sometimes been justified as 'averages' or 'rough approximations'; or alternatively as elliptical probability statements. Both ideas involve a confusion of epistemic and natural possibility. For, on the one hand, I may be quite certain about the activity of a natural mechanism on a particular occasion but incapable of any judgement about the outcome; and, on the other, I may be sure that some rule of thumb will hold though quite uncertain about the reasons why.

I have argued that in open systems consequents may be unrealized but that despite this we may know that a law is applicable (a mechanism is at work) if we know that its antecedent has been instantiated and it has been independently verified. But both antecedents and consequents are events in open systems. Is

⁴⁵ H. Poincaré, *Science and Hypothesis*, p. 98.

there not an asymmetry here? Am I not placing a higher demand on antecedents than consequents? Ontologically no; but epistemically yes. For a mechanism may be set in motion and because of the complexity or opacity of the conditions under which this happens the describer may not know that it has been set in motion; so that a fortiori he cannot know that the law it grounds is applicable. To explain an event by invoking a law I must have grounds for supposing that a mechanism is at work; but the mechanism may be at work, given that its stimulus and other conditions are satisfied, without my knowing it. Some fields may be incapable of detection.

In §2 the critical conditions for a closure were developed and in §3 the concept of action implied by them was brought out. In both cases their restrictedness was noted. In this section a realist account of laws has been counterposed to the actualist account and its superiority clearly demonstrated. Once we are persuaded of the very special conditions presupposed by actualism and the possibility of an alternative, what havoc must we make of the doctrines of orthodox philosophy of science?

In nature, constant conjunctions are the rare exception; not, as supposed by actualism, the universal rule. And in general it requires human activity to generate them. To invoke a law I must have grounds for supposing that the antecedent conditions are satisfied, so that the mechanism designated is active. But it is only if I have grounds for supposing that the system in which the mechanism acts is dosed that the prediction of the consequent event is deductively justified. With this in mind let us return to the theories expressed in statements (i)–(v) on pages 63–4 above. It is only under conditions of a closure that given the antecedent, the deduction of the consequent event is possible, so that the conditions for the Popper-Hempel theory of explanation are satisfied (ii) or those for the symmetry between ‘explanation’ and ‘prediction’ obtain (iii). It is only then that ex ante criteria of refutation can be laid down for a theory (iv) or that it makes sense to judge a theory by its predictive success (v). For it is only then that the resemblances and sequences between phenomena, that Mill identified and so confused with laws, are constant (i).

It is contingent whether some enduring thing or mechanism is activated. And though, given this, it is necessary that a certain

tendency should be 'in play', it is contingent, upon the occurrence of a closure, whether the consequent of the law-like statement is realized. In short, to know that a law is effective I do not need to be in a position to predict any event (and, it might be added, vice versa).

Now once we have grasped the ubiquity of open systems in nature we will be in a better position to understand the embarrassment with which textbooks in the philosophy of science gloss over their failure to produce a single law or explanation which satisfy the criteria they so laboriously develop and defend; a fact which bears eloquent witness to the non-availability of universal closures of any epistemic significance. We will also be in a better position to understand not just this failure, but their *absurdity*, when they seek to apply these same criteria to fields such as history and the human sciences, where the conditions for even a restricted closure (of a non-trivial kind) are not naturally and cannot be experimentally satisfied, and where the concept of action implied by these criteria is patently inapplicable.

For a closure one each of the system, individual and organizational conditions must be satisfied. Reflection on the conditions set out on page 76 above and the concept of action implied by them (see (i)–(vi) on page 83) shows the patent absurdity of trying to apply the constant conjunction formula to the domain of social life. Consider the conditions for a closure as applied to e.g. the category of persons. Remember that people are individuals, which means that they are complexly structured and pre-formed in different ways, so that they will respond differently in the same external circumstances (i.e. to the same stimulus). Remember too that they are subject to a continuing flow of contingencies, none of which can be predicted with deductive certainty. And, without calling into question the applicability of the classical paradigm (with its assumption that the stimulus conditions for action are always extrinsic), that they are engaged in activities such as writing and cooking, bar billiards and chess, which cannot be plausibly analysed in terms of atomistic components. In short, where the subjects, conditions or forms of action are characterized by structure, diversity or change, the Humean theory of the actuality of causal laws, and ipso facto the theories of science that are based on it, just cannot apply.

Conversely, it is just because the very special conditions for a

closure are sometimes satisfied in physics and chemistry (though they are not normally possible in the other natural sciences – from cosmology to biology) that accounts for the prima facie plausibility of these theories there. But the transcendental analysis of experience allow us to turn the tables on actualism and empiricism here. For it is not given conjunctions of events (or experiences) but structures which are normally out of phase with the patterns of events (and experiences) that emerge from it as the true objects of scientific understanding. This raises the question of whether there are analogous structures at work in fields other than physics and chemistry. If there are, we must bear in mind that it would not even be plausible to misconstrue them as empirical generalizations. On the other hand, if we continue to confuse laws and empirical generalizations we shall never be able to identify them.

5. AUTONOMY AND REDUCTION

Laws we already know do not describe the patterns of events. But how do they stand to the world of our everyday action and of perceived things?

Reflect for a moment on the world as we know it. It seems to be a world in which all manner of things happen and are done, which we are capable of explaining in various ways, and yet for which a deductively justified prediction is seldom, if ever, possible. It seems, on the face of it at least, to be an *incompletely described world of agents*. A world of winds and seas, in which ink bottles get knocked over and doors pushed open, in which dogs bark and children play; a criss-cross world of zebras and zebra-crossings, cricket matches and games of chess, meteorites and logic classes, assembly lines and deep sea turtles, soil erosion and river banks bursting. Now none of this is described by any laws of nature. More shockingly perhaps none of it seems even governed by them. It is true that the path of my pen does not *violate* any laws of physics. But it is not *determined* by any either. Laws do not describe the patterns or legitimate the predictions of kinds of events. Rather it seems they must be conceived, at least as regards the ordinary things of the world, as situating limits and imposing constraints on the types of action possible for a given kind of thing.

Laws then not only predicate tendencies (which when exercised constitute the normic behaviour) of novel kinds (or of familiar things in novel or limit situations); they impose (more or less absolute) constraints on familiar things. In this section I want to reconcile these aspects of laws by arguing that familiar things are comprehensive entities which may be controlled by (or subject to the control of) several different principles at once; and that they may be said to be agents. Laws ascribe possibilities which may not be realized and impose necessities which constrain but do not determine; they ascribe the former to novel kinds and impose the latter on familiar things. These features cannot be explained away as an imperfection of knowledge; but must be seen as rooted in the nature of our world. They are therefore inconsistent with the thesis of regularity determinism which underpins the doctrine of the actuality of causal laws, and to which I must now return.

So far I have discussed regularity determinism merely as an epistemological thesis to the effect that our knowledge of the world can be cast in a certain form. But of course this presupposes that the world is such that our knowledge of it can be cast in that form. To deal with regularity determinism I must thus draw out this ontological presupposition; i.e. cast the thesis itself in ontological form. The main work for this has already been done. For I have already shown in §§2 and 3 that regularity determinism makes a claim about what would happen (and the way it would happen) if certain highly restrictive conditions were satisfied. These were, it will be remembered, conditions such that *if* we knew they were satisfied and the constant conjunction formula was not vindicated, the regularity determinist would be bound to admit his thesis refuted. Now regularity determinism's ontological claim is simply that the world is such that these conditions are satisfied and his thesis is not refuted. Now of course because we can never know that these conditions are satisfied we can never refute regularity determinism in this way. But I have also asserted that regularity determinism is metaphysically refutable. How can this be done? In the only way open to transcendental realism: that is by showing that if the world were as claimed by regularity determinism science would be impossible. But as science is possible (which we know, because as a matter of fact it occurs) the world

must be such that either the critical conditions are not satisfied and/or the constant conjunction formula is abrogated. In short, the ontological untruth of regularity determinism is a condition of the possibility of science.

Close to the appeal of determinism lies the following error: to think that because something happened and because it was caused to happen, it had to happen before it was caused. Now if we take determinism to assert that all events are determined before they happen and conceive their determination as lying in the satisfaction of antecedent sufficient conditions for them then we have a picture of a chain of antecedent sufficient conditions for events stretching back infinitely into the past (assuming that conditions can be analysed as events or vice-versa). So if we ask how long is an event determined before it actually happens the answer must be at any (i.e. at every) time before it happens. And so if we now take cause in the ordinary sense, we have the result that every event is determined before it was caused (or made) to happen. At play here are of course two concepts of cause: qua causal agent (cause₁) and qua antecedent condition (cause₂). I am going to argue that the former is irreducible to the latter and essential to science. To say that something is *determined* before it has been caused to happen is either to say that it can be *known* before it has been caused₁ to happen (epistemic determinism) or that it has been *caused*₂ before it has been caused₁ to happen (ontological determinism). The former depends upon a closure, the latter depends upon the critical conditions for it being satisfied. Now I want to argue that at any (and every) time the world consists of things which are already complexly structured and pre-formed wholes; which may be simultaneously constituted at different levels and simultaneously controlled by different principles. It is because things cannot be reduced to the conditions of their formation that events are not determined before they are caused to happen. This fact accounts for both the temporal asymmetry of causes and effects and the irreversibility of causal processes in time. And it is because things cannot be reduced to atomistic components that when events are caused to happen it is by the thing which acts (i.e. the agent), the event being produced in the circumstances that prevail.

Now I want to argue that determinism is ontologically false

(it is not true that events are determined before they are caused to happen, whether in a regularly recurring or non-recurring way) and epistemically vacuous (there are no significant descriptions that satisfy the formula of regularity determinism). This has the methodological corollary that the search for such descriptions is likely to be unrewarding. (And here once again it is necessary to counterpose the investigation of complex pre-formed things to the search for the complete atomistic state-descriptions that it is supposed would enable us to predict their behaviour.) The only sense in which science presupposes 'determinism' is the sense in which it presupposes the ubiquity of causes₁ and hence the possibility of explanations. And the only sense in which it presupposes 'regularity determinism' is the sense in which it presupposes the ubiquity of causes₁ for differences and hence the possibility of their explanation. But it is probably better not to use 'determinism' in this way (nb, cause₁ ≠ cause₂). Now any refutation of regularity determinism as an ontological thesis must depend upon establishing the autonomy of things, in the sense of the impossibility of carrying out the reductions implicit in the vital conditions B1 and C1 of Table 2.1 on page 76 (their being a clear asymmetry, for the realist, between the subjects and the condition of action, and the constancy alternative being recessive). It is here that I will pitch my attack. Thus I am not going to argue that if the critical conditions were satisfied the constant conjunction formula would not be vindicated. Rather, I am going to argue that the critical conditions could not be satisfied in any world containing science. The question of whether or not history *would* repeat itself is one that need not detain us here. A nagging doubt may remain: surely, it might be felt, in the (very) last instance regularity determinism must be true. But this is not so. For once we have established an ontology of structures there is no earthly reason why events should [have to] be constantly; conjoined. There are indeed principles of indifference (as we shall see in Chapter 3). But they do not apply, nor is there any reason why they should, to events, states-of-affairs and the like.

In establishing the autonomy of things I will follow the normal procedure of transcendental realism; that is, I will first analyse some more or less underanalysed feature of science and then ask what the world must be like for this feature to be possible.

The feature I am concerned with are two aspects of scientific laws, viz:-

- (i) their normic and non-empirical character; and
- (ii) their consistency with situations of dual (and multiple) control.

I will argue that for these features to be possible the world must be composed of agents. Agents are particulars which are the centres of powers. In an incompletely described world of other agents powers must be analysed as tendencies. And laws are nothing but the tendencies or ways of acting of kinds of thing. By an agent I mean simply anything which is capable of bringing about a change in something (including itself). A hydrogen atom is, in virtue of its electronic structure, an agent. For it possesses the power to combine with an atom of chlorine to produce, under suitable conditions, a molecule of hydrochloric acid. It should perhaps be said at the outset that I am not going to refer to quantum mechanics in my argument. It seems to me to be always a mistake, in philosophy, to argue from the current state of a science (and especially physics). In general, I have refrained from scoring points against determinism and actualism which turn on the inaccuracy (or imprecision) of our descriptions or the indeterminacy of our measures. This is because they do not in general raise important ontological questions. It is debatable whether quantum mechanics does – but if it in fact requires a reinterpretation of the category of causality in fundamental physics it will not be in the Humean direction and can only strengthen the anti-determinist's hand.

I have already discussed (i) at some length so I will be brief with it here. Contrast the law of conservation of energy or of mass action with a simple empirical generalization like 'all pillar-boxes are red' or 'all blue-eyed white tom cats are deaf'. Whereas the latter, at least so long as they remain unattached to any theory, could be defeated by a single counter-instance, the truth of the former is consistent with almost anything that might happen in the world of material objects and human beings. For they do not attempt to describe this world; i.e. they cannot be interpreted as undifferentiated empirical generalizations. Rather they must be interpreted as principles of theories – of physics and chemistry – which tell us something about the

way things act and interact in the world. As such they specify conditions which we presume are not contravened but rather continually satisfied in the countless different actions and interactions of the world, including those of which we have direct experience. And they are manifest in certain impossibilities, e.g. that of building a perpetual motion machine. Nevertheless they are principles for which any test would require not only fine measurement but closed conditions. As such they are not normally empirically manifest to us or actually satisfied. (For the scientist this feature appears as a difference between the real or corrected and the actual or measured values of the variables he is concerned with.) Thus we could say that relative to these vantage points, viz. of experience and actuality, these principles specify levels of deep structure or (metaphorically) place conditions on the inner workings of the world.

Now it might be said that laws, such as those of mechanics or electricity, do not describe the world as such, but only those aspects or parts of it which fall within their domain, i.e. the mechanical or electrical aspects of it. But this concedes my point. For one can only say which aspects are mechanical or electrical by reference to the antecedently established laws of mechanics and electricity, and such aspects are real. Clockwork soldiers and robots do not more nearly observe the laws of mechanics than real people. Rather their peculiarity stems from the fact that if wound up and left alone their intrinsic structure ensures that for each set of antecedent conditions only one result is possible. But outside the domain of a closure the laws of mechanics are, as Anscombe has put it, 'rather like the rules of chess; the play is seldom determined, though nobody breaks the rules'.⁴⁶

Closely connected with this feature of laws is their consistency with situations of 'dual control'. A game of cricket is only partially controlled by the rules of cricket, language-using by those of grammar. Chemical reactions are only partially controlled by Dulong and Petit's law, black bodies behave in all kinds of ways that are not specified by the Stefan-Boltzmann law. Coulomb's law does not completely describe the action of charged particles, or Faraday's law all that happens to an electrode. Similarly the 'boundary conditions' for the laws of

⁴⁶ G. E. M. Anscombe, *op cit.*, p. 21.

mechanics, the domain within which they apply, are controlled by the operating principles defining a machine.⁴⁷ Laws leave the field of the ordinary phenomena of life at least partially open. They impose constraints on the type of action possible for a given kind of thing. But they do not say which out of the possible actions will actually be performed. They situate limits but do not dictate what happens within them. In short, there is a distance between the laws of science and the ordinary phenomena of the world, including the phenomena of our actual and possible experience. And it is with the investigation of this distance that I am here concerned.

To say that laws situate limits but do not dictate what happens within them does not mean that it is not possible to completely explain what happens within them. The question ‘how is constraint without determination possible?’ is equivalent to the question how ‘can a thing, event or process be controlled by several different kinds of principle at once?’ To completely account for an event would be to describe all the different principles involved in its generation. A complete explanation in this sense is clearly a limit concept. In an historical explanation of an event, for example, we are not normally interested in (or capable of giving an account of) its physical structure.

In deciding to write ‘!’ on this piece of paper I select the conditions under which the laws of physiology and physics are to apply. So that it is absurd to hold that the latter might account for my ‘!’, or that it might have been predicted in the basis of a knowledge of a physical state-description prior to my writing it. On the other hand my neuro-physiological state and the physical conditions must be such that I can write it; they could prevent it (e.g. if I were suddenly to fall asleep or be propelled into orbit around the moon). There is a space between the laws of physics and physiology and what I do within which deliberation, choice and voluntary behaviour have room to apply. The theory of complex determination, in situating persons as comprehensive entities whose behaviour is subject to the control of several different principles at once, allows the possibility of genuine self-determination (subject to constraints) and the special power of acting in accordance with a plan or in the light of reasons.

⁴⁷ Cf. M. Polanyi, ‘The Structure of Consciousness’, *The Anatomy of Knowledge*, ed. M. Grene, p. 321.

Human freedom, on this view, *if* it exists, would not be something that somehow cheats science (as it is normally conceived) or, on the other hand, something that belongs in a realm apart from science; but something whose basis would have to be scientifically understood. As freedom would be analysed as a power of men and science is, for us, non-predictive there is nothing inconsistent or absurd about such an assertion; any more than to say that purposefulness in animals, which is no doubt not the same as intentionality in men, has (still) to be scientifically understood. I suggest that only the theory of complex determination is compatible with agency; and that there are no grounds for assimilating intentional action to the classical paradigm or supposing that intentionality is not a real attribute of men. However, this is peripheral to my main concerns here. Dogs cannot fly or turn into stones, but they can move about the world and bark in all kinds of ways. To deny the latter possibility is as absurd as to deny the former necessity. But the reasons why they behave in canine ways is an open question for a putative science of animal ethology to answer.

The difference between laws of nature and empirical generalizations is analogous to the difference between the rules of cricket and a television recording of the actual play on some particular occasion. Whether or not Boycott scores a century is not determined by the rules of cricket; but by how he bats and how the opposition play. Now it is clearly necessary for the intelligibility of the idea of dual (or multiple) control that the higher-order level is open with respect to, in the special sense of irreducible to, the principles and descriptions of the lower-order level. It is easy to see why this must be so. For it is the operations of the higher-order level that control the boundary conditions of the lower-order level, and so determine the conditions under which the laws of that level apply. It is the state of the weather that determines, in England, when and where the rules of cricket can apply; the state of the conversation that determines the ways in which we can express ourselves in speech; the state of the market that determines the use of machines, the use of machines that determines the conditions under which certain physical laws apply. The use of machines is thus subject to dual control: by the laws of mechanics and those of economics. But it is the latter that determine the boundary conditions of the former.

It follows from this that the operations of the higher level cannot be accounted for solely by the laws governing the lower-order level in which we might say the higher-order level is 'rooted' and from which we might say it was 'emergent'. Now an historical explanation of how a new level came to be formed would not, it is important to see, undermine this principle. Let us suppose that we could explain the emergence of organic life in terms of the physical and chemical elements out of which organic things were formed and perhaps even reproduce this process in the laboratory. Now would biologists lose their object of inquiry? Would living things cease to be real? Our apprehension of them unmasked as an illusion? No, for in as much as living things were capable of acting back on the materials out of which they were formed, biology would not be otiose. For a knowledge of biological structures and principles would still be necessary to account for any determinate state of the physical world. Whatever is capable of producing a physical effect is real and a proper object of scientific study. It would be the task of biologists to investigate the causal powers of living things in virtue of the exercise of which *inter alia* they brought about various determinate states of the physical world. Living creatures *qua* causal agents determine the conditions under which physical laws apply; they cannot therefore already be manifest in the latter. Sentience determines the conditions of applicability of physical laws, but it is also subject to them. If the elements of the lower-order are real then so must be the causes that determine the conditions of their operation, i.e. the comprehensive entities formed out of them. If black bodies are real then so are physicists, if charged particles are real then so are thunderstorms. In short, emergence is an irreducible feature of our world, i.e. it has an irreducibly ontological character.

Reflect once more on the distinctiveness of laws of nature and empirical generalizations. The laws of nature leave the conditions under which they operate open, so the field of phenomena is not closed: it is subject to the possibility of dual and multiple control, including control by human agents. What I can do is constrained by the operation of natural laws. But I can hack my way all over the physical world, defeating empirical generalizations. I can interrupt the operations or break the mechanism of a machine and so falsify any prediction made on the basis of

its past behaviour. But I cannot change the laws that governed and so explained its mode of operation. And I can come, in science, to have a knowledge of such normic and non-empirical statements; and perhaps in time begin to recognize analogous principles at work controlling my own behaviour (marking the site of a possible psychology).

I have argued that complex objects are real (and that the complexity of objects is real); and that the concept of their agency is irreducible. Complex objects are real because they are causal agents capable of acting back on the materials out of which they are formed. Thus the behaviour of e.g. animate things is not determined by physical laws alone. But that does not mean that their behaviour is not completely determined: only that an area of autonomy is marked out which is the site of a putatively independent science. And because the forms of determination need not fall under the classical paradigm this in turn situates the possibility of various kinds of self-determination (including the possibility that the behaviour of men may be governed by rational principles of action).

From the normic and non-empirical nature of laws and their consistency with situations of dual control I conclude that the world is a world of agents incompletely described. Laws neither undifferentially describe nor uniquely govern the phenomena of our world. And this is accounted for by the fact that it is an incompletely described world of agents which are constituted at different levels of complexity and organization.⁴⁸ However it might be objected here that all I have shown is that the laws that we *currently* possess do not describe the world as we currently know it. And that I have not shown that *if* we were in fact able to reduce (apparently) complex things to complete atomistic state-descriptions that we would be unable to predict future physical states of the world without referring to comprehensive entities and principles of behaviour special to them. The final stage of my argument against actualism must thus constitute a critique of strong actualism in which the incoherence of the programme of reduction it envisages for science is demonstrated.

It is important to be clear about the different senses of 'reduction'. There are three distinct ways in which a science

⁴⁸ Cf. M. Bunge, *The Myth of Simplicity*, Chap. 3; and M. Polanyi, *The Tacit Dimension*, Chap. 2.

might be said to be 'reducible' to a more basic one, which ought not to be confused. There is first the idea of some lower-order or microscopic domain providing a basis for the existence of some higher-order property or power; as for example, the neurophysiological organization of human beings may be said to provide a basis for their power of speech. There is secondly the idea that one might be able to *explain* the principles of the higher-order science in terms of those of the lower-order one. This depends upon being able to undertake at least a partial translation of the terms of the two domains; though it is conceivable that they may retain substantially independent meanings and overlap only in some of their reference states. Such a 'reduction' may of course result in modifications of the laws of the higher-order domain.⁴⁹ There is finally the sense in which it is suggested that from a knowledge of the states and principles of the lower-order science we might be able to *predict* behaviour in the higher-order domain. It is important to see that it is to this claim that the strong actualist is committed, if he is to eliminate complex behaviour in favour of its atomistic surrogates. It depends not only upon the establishment of a complete parallelism between the two domains, but upon a closure, i.e. the attainment of a complete atomistic state-description of all the systems within which the events covered by the descriptions of the higher-order science occur.

Now it is especially important to keep the second and third senses distinct. For though it is clear that we can explain the principles and laws of chemistry in terms of those of physics or of classical mechanics in terms of quantum mechanics, we cannot predict physical and chemical events such as the next eruption of Vesuvius on the basis of that knowledge alone. For that we would need an antecedent complete atomistic state-description, i.e. a closure, as well. Now the strong actualist, claiming that the world is in the end closed, must, unless he is to limit himself merely to a dogmatic reassertion of this claim, presumably map out a strategy for the sciences to attain such a closure. The fact that a successful reduction in science does nothing in itself to achieve empirical invariances is something

⁴⁹ Cf. P. K. Feyerabend, 'Explanation. Reduction and Empiricism' *Minnesota Studies in the Philosophy of Science, Vol. III*, eds. H. Feigl and G. Maxwell, pp. 28–95.

of a blow to the programme (as distinct from dogma) of strong actualism. But even if it did there is an even more damaging objection at hand (which carries a more general moral for all those who see in 'reduction' the hope of the 'less developed' sciences). For every historically successful reduction of one science to another has depended upon the *prior* existence of an established corpus of scientific principles and laws in the domain of the reduced science. It is easy to appreciate why this must be so: for without the specification of some already more or less clearly demarcated and well charted domain no programme of reduction could possibly get to work. But this means that as a means of discovery, i.e. of achieving such a body of knowledge, reductionism must fail. For it presupposes precisely what is to be discovered.

I still have not refuted strong actualism as a possible account of the world. This I shall now do by arguing that it is inconsistent with any world containing science, and thus in any world in which science is possible. The only way of reconciling experimental activity with the empiricist notion of law is to regard it as an illusion; that is, to regard the actions performed in it as subsumable in principle under a complete atomistic state-description. In principle this applies not only to experimental activity but to all scientific activity (including theory-construction) in as much as it involves physical effects. Now this has the absurd consequence that the apparent discovery of natural laws depends upon the prior reduction of social to natural science. Or to put it another way, in an actualist world there would be no way of discovering laws which did not already presuppose a knowledge of them. So a closed world entails either a completed or no science. But as 'completion' is a process in time the former possibility is ruled out: so a closed world entails the impossibility of science. But as science occurs the world must be open. This is not the reason why the world is open (though it is the reason for my justified belief that it is). Rather it is because the world is open that science, whether or not (and for how long) it actually occurs, is possible. In an open world all laws must be of normic form; and this is quite independent of our knowledge of them. In short, the complexity of agents and the normic character of laws are irreducible ontological features of the world; that is, they are necessary

features of our world established as such by philosophical argument.

It is relatively easy to show that all (and not just scientific) action depends upon our capacity to identify causes in open systems. For all action depends upon our capacity to bring about changes in our physical environment. Hence we must belong to the same system of objects (nature) on which we act. But we not only act on it, in the sense of bringing about changes that would not otherwise have occurred; we act on it purposefully and intentionally, i.e. *so* as to bring about these changes (as the results and consequences of our actions) and knowing that we are acting in that way. This depends upon our being able to identify features of our environment as the objects of our causal attention and as part of the system to which causality applies. Thus we must be capable of identifying and ascribing causes in our environment, and knowing ourselves as a causal agent among others. Unless we could do this, we could not act intentionally at all. Thus all human action depends upon our capacity to identify causes in open systems (to which of course the Humean theory cannot apply).

I suggested earlier that human freedom is not only compatible with science, but had to be scientifically understood. This is important because it is *inter alia* a precondition for science. For science to be possible men must be free in the specific sense of being able to act according to a plan e.g. in the experimental testing of a scientific hypothesis. Human freedom is not something that stands opposed to or apart from science; but rather something that is presupposed by it. The idea that freedom is opposed to or apart from science stems from the empiricist conception of scientific experience as consisting in the passive observation of repeated sequences rather than in the active intervention of men in the world of things in an endeavour to grasp the principles of their behaviour. Men are not passive spectators of a given world, but active agents in a complex one.

The view of the world as open and the view of the world as closed lead to totally different conceptions of science. The laws of nature, which are painstakingly uncovered by the theoretical work of science supplemented wherever possible by experimental investigation, do not seek to describe the myriad phenomena

of the world, the contents of a biscuit tin or the junk in the builder's yard. They do not seek to trace the path of a squirrel, predict which rafter a sparrow will light on or how many buns the vicar will have for tea.⁵⁰ They can indeed come to explain such things in a certain way, but only on the condition that they are not interpreted as describing them.

6. EXPLANATION IN OPEN SYSTEMS

The fact that closed systems are a presupposition of the actualist account of science is reflected (a) in the absence of a theory of their establishment and (b) in the absence of a clear contrast between *pure* and *applied* phases of scientific activity or, perhaps better, between *science* and its *uses*. It is with the second that I will be concerned here. Now consistency with our conception of the objects of science as the mechanisms that produce phenomena, not the phenomena they produce (which must now be seen as both complex and differentiated), means that we must carefully distinguish between two moments of the scientific enterprise (interpreted broadly): the moment of *theory*, in which closed systems are artificially established as a means of access to the enduring and continually active causal structures of the world; and the moment of its open-systemic *applications*, where the results of theory are used to explain, predict, construct and diagnose the phenomena of the world. Actualism cannot sustain this distinction; or, if we confront it with it, show how the practical application of our knowledge is possible in open systems. This depends upon precisely the same ontological distinction as is necessary to sustain the intelligibility of experimental activity, namely that between causal laws and the patterns of phenomena, the mechanisms of nature and the events they

⁵⁰ A caricature of such an empiricism exists in some of the early experiments conducted under the august auspices of the Royal Society. The following is an example: '1661, July 24: a circle was made with a powder of unicorn's horn, and a spider set in the middle of it, but it immediately ran out several times repeated. The spider once made some stay upon the powder', C. R. Weld, *History of the Royal Society, Vol. I*, p. 113. Among the items of allegedly scientific interest collected by the Society were 'the skin of a moor, tanned with the beard and hair white' and 'an herb which grew in the stomach of a thrush', *ibid.*, p. 219. Quoted in P. K. Feyerabend, 'Problems of Empiricism', *op. cit.*, p. 156.

generate, the domains of the real and the actual. In this way actualism's assumption of an undifferentiated reality is mirrored in the assumption of an undifferentiated science.

It is because of this ontological distinction that theory is never disconfirmed by the contrary behaviour of the uncontrolled world, where all our predictions may be defeated. Meteorology provides an instructive example here. We can have very little confidence in the *ex ante* predictions of weather forecasters, because of the instability of the phenomena with which they have to deal. But we can place a great deal of rational confidence in their *ex post* explanations. For the law-like statements they use to retrodict the antecedent events and states by means of which they both explain what actually happened and excuse their forecasts of it are not meteorological laws. So that meteorology is in this sense not a theoretical science. Rather, mentioning general physical variables, they are physical laws which have been confirmed quite independently of their use to explain and predict the weather. Thus meteorology, like engineering, stands to physics and chemistry as an applied to a pure science, using the experimentally-established results of the latter. (I am not ruling out the possibility that there may be irreducibly meteorological principles.)

Now it is characteristic of open systems that two or more mechanisms, perhaps of radically different kinds, combine to produce effects; so that because we do not know *ex ante* which mechanisms will actually be at work (and perhaps have no knowledge of their mode of articulation) events are not deductively predictable. Most events in open systems must thus be regarded as 'conjunctures'. It is only because of this that it makes sense to talk of a stray bullet or an unhappy childhood affecting 'the course of history'. And it is only in virtue of this that laboratory closures can come to be established. The importance of experimental activity in natural science, conceived as a specific kind of conjunctural occurrence, allows us to stress that the predicates 'natural', 'social', 'human', 'physical', 'chemical', 'aerodynamical', 'biological', 'economic', etc. ought not to be regarded as differentiating distinct kinds of events, but as differentiating distinct kinds of *mechanisms*. For in the generation of an open-systemic event several of these predicates may be simultaneously applicable.

The skills of an applied and a pure scientist are characteristically different. The applied scientist must be adept at analysing a situation as a whole, of thinking at several different levels at once, recognizing clues, piecing together diverse bits of information and assessing the likely outcomes of various courses of action. The pure scientist, on the other hand, deliberately excludes, whereas the applied scientist seeks always to accommodate, the effects of intervening levels of reality. Though he is unafraid of flights of daring (always risky for the practical man), he holds fast to his chosen objects of inquiry. The applied scientist is an instrumentalist and a conservative, the pure scientist a realist and (at the highest level) a revolutionary. Keynes had the rare gift among economists of knowing both how to make money and how money is made.⁵¹

I said in §1 that the activities of explanation, prediction and the identification of causes not only do not presuppose a closure, but they do not necessarily involve, though they may make use of laws. There are two points here. First, there is a difference in general between scientific and lay explanations. That this is so is entailed by one of the most obvious features of science, namely the prolonged period of scientific education and training a novice must normally undergo before he is considered capable of 'scientific explanation'. This has a rationale in the real stratification of the world and a real effort, which is science, needed to penetrate it. Needless to say, however, that stratification cannot justify any particular institutionalized form or any social division e.g. between scientists and non-scientists (the educator and the educated) arising from the latter. Secondly, what primarily distinguishes scientific from lay explanations of events is not their structure but the *concepts* that figure in them. Thus the role played by laws in the scientific explanation of events, a role which is played via the invocation of the concept of the mechanism at work in the generation of the event, which is the function of the citation of the law (and which will be discussed further in the next chapter), is paralleled in lay explanations by other kinds of normic statements such as platitudes, truisms, assumptions of rationality or more crudely or vaguely formulated law-like statements. Moreover, there is a case, which I am now going to examine in some detail, in which

⁵¹ See R. F. Harrod, *The Life of John Maynard Keynes*.

both scientific and lay explanations have exactly the same form and in which they do not involve normic statements at all. This is the transitive verb model to which I have already alluded in §3 above.

‘Why is the door open?’ – ‘Because Tania pushed it open’. ‘The door is open because Tania pushed it [open]’ is a paradigm causal explanation, accomplished without reference to laws, by the redescription of the explanandum event in terms of its cause. It is informative – there are other reasons why the door might be open. But it is also logically necessary; i.e. the explanation is deductive – if Tania pushed the door open, it *must* be open. In this ‘Tania pushed the door open’ differs from ‘Tania pushed the door hard’. ‘Tania pushed the door hard’ may explain why the door is open but it does so only contingently. On the other hand ‘Tania observed the door open’ cannot explain why the door is open because there is no conceivable way in which observing can bring about a change in the object concerned (viz. the state of the door). Now the role of the verb ‘push’ in ‘Tania pushed the door open’ is to link the A-sequence and the B-sequence in Diagram 2.1 by supplying an interpretation of the latter, so that the door’s movement can be seen as the result of a continuous action sequence. Note that though ‘Tania moved up to the door and then the door moved away’ is a true description it does not mean the same as ‘Tania pushed the door open’.

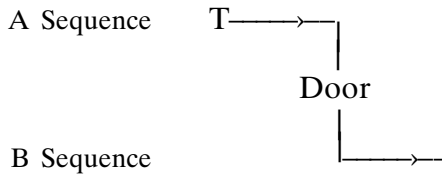


Diagram 2.1

Now transitive verbs such as ‘pushing’, ‘pulling’, ‘knocking’, ‘twisting’, ‘binding’, ‘squeezing’, ‘holding’, ‘forcing’, ‘driving’, ‘turning’, ‘stimulating’, ‘producing’, ‘generating’, ‘bringing about’, ‘making’, etc. lie at the root of our notion of cause.⁵² When something is cited as a cause it is being viewed as that element, paradigmatically an agent, in the total situation then

⁵² Cf. H. L. A. Hart and A. M. Honoré, *Causation in the Law*, Chap. 2, Sect. 2.

prevailing which, from the point of view of the cause-ascriber, 'so tipped the balance of events as to produce the known outcome'.⁵³ Now the importance of the transitive verb model is that it accounts for both the large number of ordinary causal explanations which are deductive (or become so with the addition of a suitable objective complement, perhaps tacitly understood) and the basic interactions of classical mechanics; i.e. the fact that action-by-contact was not itself felt to be in need of explanation. In neither case is there reference to laws or any other general statements. 'Juanita made Xara push the door open', 'The mixture made him sick', 'He drove his wife to despair', 'The sergeant forced him to pull the trigger', 'The elephant crashed into the juggernaut', 'The first billiard ball smacked into the second', 'The irate positivist knocked his ink bottle over', 'The psychoanalyst suggested he open the window' – these are the primaevial explanation forms. It has been suggested that it is the fact that something is subject to human manipulation or control that accounts for our identification of it as the cause.⁵⁴ But apart from obvious counter-examples, it is clear that we could only know ourselves as causal agents in a world of other causal agents and that our notion of cause takes in the possibility of a world without men. It is because men are agents, not because 'other agents' have affinities with men, that the concept of cause would still find application in such a world.

Now if most events in open systems are conjunctures, i.e. are to be explained as the results of a multiplicity of causes, to the extent that basic causal explanations are involved, one would expect a modification of the transitive verb model to be necessary, corresponding and similar to that which required a restatement of the nomological model in normic form. This is so. For if a single influence was responsible for the outcome the event could be seen, as in Diagram 2.1, as the simple pure linear displacement of its cause (and deducibility would be preserved). To the extent however that more than one factor is at work the event will have to be seen as a kind of 'condensation' or 'distillation' of its component causes.

I now want to illustrate this by looking at a fairly typical piece

⁵³ M. Scriven, 'Causes, Connections and Conditions in History', *Philosophical Analysis and History*, ed. W. H. Dray, p. 248

⁵⁴ D. Gasking, 'Causation and Recipes', *Mind*, 1955; pp. 479–87.

of historical narrative. This will also enable me to identify some more general characteristics of explanation in open systems. In the piece of narrative that follows I underline obviously causal notions.

This *pressure* from the Labour Party, with its great *influence* on the industrial workers, combined with the attitude of President Wilson himself, slowly *propelled* Lloyd George in the *direction* of the formulation of war aims. *Hindered* as he was by the obligation of earlier agreements with the European allies, he *ensured* that his declaration, *made* on the 5th January 1918, was only in the vaguest terms. It was, however, not incompatible with the much more specific Fourteen Points enunciated independently by the American President a few days later, and appealed to by the German Government as a basis for peace negotiations at the time of the armistice in November.⁵⁵

The first thing to notice about this piece of historical narrative is its decentralized focus, allowing the emergence, in a series of redescriptions of the event concerned, of a picture of the conjuncture or balance of forces in which it occurred and in terms of which it is explained. The event is in fact known under three different descriptions: E_a, Lloyd George's formulation of his war aims; E_b, his *vague* formulation of these war aims; and E_c his vague yet *compatible* (with the Fourteen Points) formulation of these war aims. Secondly, the indispensable role that causal notions play in both indicating the key variables which brought about the event and in rendering intelligible their efficacy can be seen. Why did Lloyd George formulate his war aims? Because of pressure from the Labour Party and from President Wilson. Here we imagine the event as if it were a simple displacement. But now the simple displacement is modified by the effect of another factor, viz, his previous obligations, and so Lloyd George formulates his war aims vaguely. The event becomes a condensation of the different explanatory linkages. Thirdly, each of these individual linkages could in principle be located within some interpretative schema or theoretical structure. But it is simple displacements, transitively understood, and the role that causal notions play in them that explains the peculiar efficacy of what Dray has called 'continuous series'.⁵⁶ Finally, the non-unified ontology of the

⁵⁵ H. Pelling, *Modern Britain 1885–1955*, p. 77.

⁵⁶ W. H. Dray, *Laws and Explanation in History*, pp. 66ff.

explanation should be noted. The industrial proletariat and President Wilson's attitude co-exist within the same explanation. The pattern of the explanation is illustrated in Diagram 2.2.

A possible misunderstanding must be avoided and a possible puzzle allayed. The physical action causal notions used in the explanation of such an event are of course employed metaphorically. Lloyd George is not literally propelled. In this way they

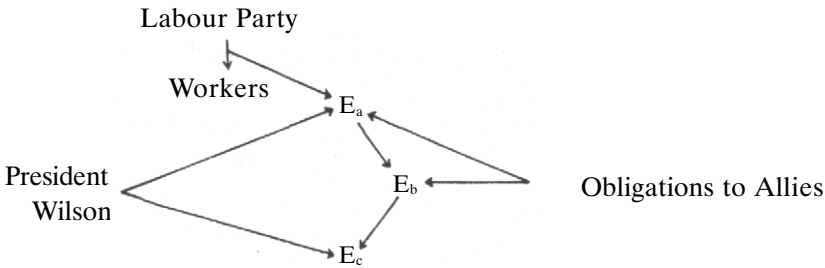


Diagram 2.2

stand in for what some would say are trivial, though I would prefer to say are (as yet) inadequately understood, processes. Now this differs from the kind of criticism that I directed against the action-by-contact paradigm when I argued in §3 that though it may provide the source of our concept of causality, it cannot provide an adequate model for the understanding of ultimate physical actions. For doors do really get pushed open and it is perfectly legitimate to talk in this way. What is illegitimate is to regard corpuscles as acting like doors. (For if the door was a corpuscle it could not retain its shape – it would have to be bent to be ‘opened’.) A puzzle may arise about precisely what event is being explained in our simple historical explanation, when the same event is referred to under three different descriptions. But the puzzle dissolves when it is realized that the phrase ‘the event which occurred (in s_i at t_j)’ is essentially syncategorematic; that is to say that it refers only on the basis of some prior description of the event concerned. And it is precisely the function of the notion of an event to generate redescrptions of events as specified under their original descriptions in their explanation. In this way it also acts as a possible signpost into the language of theory.

I have taken a simple historical explanation because it illustrates some more general features of explanation in open systems. The pattern of explanation, even where well-developed scientific theory can be brought to bear on an event, is substantially the same. In general as a complex event it will require a degree of what might be called 'causal analysis', i.e. the resolution of the event into its components (as in the case above). These components will then require theoretical redescription, so that the theories of the various kinds of mechanism at work in the generation of the event can be brought to bear on the event's explanation. The next step will consist in retrodiction from re-described component events or states to the antecedent events or states of affairs that could have produced them. To the extent that for each determinate effect there is a plurality of possible causes retrodiction alone cannot be decisive. And so it will need to be supplemented by independent evidence for the antecedents until we have eliminated from the total set of possible causes all but the one which, together with the other factors at work, actually produced the effect on the occasion in question. The four stages in the explanation of an open-systemic event may therefore be summarized as follows: (i) causal *analysis* (or resolution) of the event; (ii) theoretical *redescription* of the component causes; (iii) *retrodiction* via normic statements to possible causes of the components; (iv) *elimination* of alternative causes.

Now it is particularly important to beware of the supposition that if we have achieved such a complete explanation of an event (normally of course we will only be interested in one or two of the influences at work) this would put us in a position whereby we could have predicted it. For the different levels that mesh together in the generation of an event need not, and will not normally, be typologically locatable within the structures of a single theory. In general the normic statements of several distinct sciences, speaking perhaps of radically different kinds of generative mechanism, may be involved in the explanation of the event. This does not reflect any failure of science, but the complexity of things and the multiplicity of forms of determination found in the world. The idea that a complete explanation of an event entails a potential prediction of it depends upon the possibility of the reduction of the various sciences to a single

level and a complete description of all the individuals at that level; i.e. it depends upon the idea of an antecedent closure. Now it is not that this represents an unreasonable ideal for science; but rather that it constitutes a conjecture about the nature of the world which is in fact false and which, if acted upon, could have the most deleterious effects on science. If science is to be possible the world must be open; it is men that experimentally close it. And they do so to find out about structures, not to record patterns of events.

Appendix

ORTHODOX PHILOSOPHY OF SCIENCE AND THE IMPLICATIONS OF OPEN SYSTEMS

It may be felt that I have dealt rather summarily in Chapter 2 with some of the most hallowed doctrines of received philosophy of science; so I want here to turn to a more detailed examination of them in the light of the phenomenon of open systems.

The structure of orthodox philosophy of science is based squarely on the Humean theory of the actuality of causal laws. But it is convenient to give Nicod's criterion,¹ which presupposes and implies it co-equal status. I shall formulate them as two principles:

- P₁, the principle of *empirical-invariance*, viz. that laws are or depend upon empirical regularities; and
- P₂, the principle of *instance-confirmation* (or falsification), viz. that laws are confirmed (or falsified) by their instances.

Post-Humean philosophy of science has called into question only the sufficiency, not the necessity of these principles; i.e. it has left the ontology implicit in them intact. Thus in the most advanced recent positions theory is regarded as irreducible; and as supplying at least part of the grounds for laws. The significance of such modifications will be considered in the next chapter. Here, as in the body of Chapter 2, I will not distinguish between philosophers who regard P₁ and/or P₂ as necessary and sufficient and those who regard them as merely necessary. Once more no harm will be done by this conflation as all my objections here turn on the lack of necessity of these principles and ipso facto of the theories they sustain. It is for the sake of explanatory convenience and to avoid repetition

¹ After J. Nicod, *Foundations of Geometry and Induction*, p. 219. See C. G. Hempel, *Aspects of Scientific Explanation*, p. 11.

that I formulate and discuss them in their 'necessary and sufficient' form.

Both P_1 and P_2 depend upon a closure, and hence upon the assumption of a simple undifferentiated reality. P_1 gives rise to the truth-functional concept of natural necessity. This is the idea that the logical status of laws can be explicated, at least in part, by the formula $\forall x(fx \supset gx)$, where the predicates 'f' and 'g' are defined extensionally or are at least given some definite empirical interpretation e.g. by-means of correspondence rules. The definition may be ostensive or operational;² and if ostensive, either sensationalist³ or physicalist.⁴ P_1 is susceptible of descriptivist (Mach)⁵ and instrumentalist (Ryle)⁶ interpretations; and of classical empiricist and transcendental idealist ones. P_2 is susceptible of inductivist (Carnap)⁷ and falsificationist (Popper)⁸ interpretations; and of positivist and conventionalist ones.

I am going to use the term 'deductivism' to refer to the ensemble of theories erected on the basis of P_1 and P_2 . My choice of the term 'deductivism' may not seem an altogether happy one in view of the fact that it is meant to cover philosophers who have regarded themselves, as 'inductivists' and 'instrumentalists', as opposed to 'deductivism'. However, I do not think that this is a serious difficulty. For there is no way in general of getting an inductive policy going without appealing to an antecedently formulated lawlike statement. (The exception is provided by the

² P. W. Bridgman, *The Logic of Modern Physics*, esp. pp. 1–25.

³ 'Physics cannot be regarded as validly based upon empirical data until [light] waves have been expressed as functions of the colours and other sense-data', B. Russel, *Mysticism and Logic*, p. 109.

⁴ See e.g. O. Neurath, 'Universal Jargon and Terminology', *P.A.S. Vol. 41*, pp. 127–48.

⁵ The communication of scientific knowledge involves description: that is, the mimetic reproduction of facts in thought, the object of which is to replace and save the trouble of new experience. This is all that natural laws are', E. Mach, *Popular Scientific Lectures*, p. 192.

⁶ G. Ryle, *The Concept of Mind* esp. pp. 116–22. Cf. also C. S. Pierce, *Collected Papers, Vol. 2*, p. 354; F. P. Ramsey, *The Foundations of Mathematics*, pp. 194–7; and S. Toulmin, *The Philosophy of Science*, Chap. 3.

⁷ R. Carnap, *The Logical Foundations of Probability* or 'Testability and Meaning', *Readings in the Philosophy of Science*, eds. H. Feigl and M. Brodbeck, pp. 47–92.

⁸ K. R. Popper, *The Logic of Scientific Discovery*, esp. Chap. 4 or *Conjectures and Refutations*, Chap. 10.

pure Humean case where the events are intuitively ascertainable atomistic instants, and each event is a member of a linear series.) For one sequence to give support to another the antecedent events must be alike in relevant respects. But to talk of the relevance of the 'respects' already presupposes a tentative (conjectured) or confirmed law. Hence the inductivist in theory must be a deductivist in practice. Similarly a rule of inference can always be recast as the major premise of a syllogism. That is to say, an inference ticket (a 'season ticket', as Ryle revealingly calls it)⁹ remains valid only as long as some empirical generalization is true. Thus the instrumentalist questions only the descriptivist interpretation of the use of laws, not their logical form.

I will first set out the overall structure of deductivism before examining its components, individually and collectively, in the light of open systems.

Underpinning deductivism is the actualist thesis that laws are relations between events or states of affairs. If the world consists only of atomistic events or states-of-affairs then for a general knowledge of it to be possible their relations must be constant. And so we have P_1 the principle of empirical-invariance

(1) laws are or depend upon constant conjunctions of events or states of affairs (which constitute the objects of actual or possible experiences). As such it generates the familiar Humean theory of causality. This theory is susceptible of different interpretations, viz.

(2)' as a theory of what we mean by saying 'X causes Y', viz. that the events as specified under their descriptions X and Y are regularly conjoined; and

(2)" as a theory of how we must be prepared to *justify* the claim that X causes Y, viz. by showing that the event as specified under these descriptions are regularly conjoined.

In the case of both theories it is possible to substitute the weaker requirement that the events be specified under these or some other set of descriptions. The weaker variants could be indicated by the subscript₁.

We then have a theory of explanation, that

(3) events are explained by subsuming them under one or more universal laws; i.e. by deducing them from a set of one or

⁹ G. Ryle, *op. cit.*, p. 117.

more universal laws, together with a statement of their initial conditions. This has become known as the Popper-Hempel theory of explanation. Popper was the first to restate it in modern times¹⁰ and Hempel has been its most systemic advocate and defender¹¹ It is convenient to divide the theory into two requirements:

(3a) a deducibility requirement, viz. that the explanandum be deducible from the explanans; and

(3b) a covering-law requirement, viz. that the explanans contain at least one universal law.

Next, a theory of prediction to the effect that

(4) events are predicted by deducing them from a set of universal laws together with a statement of their initial conditions. (3) and (4) together give rise to the theory that

(5) explanation and prediction are symmetrical (in the sense of (iii) on page 63 above).

On this view their difference lies merely in the fact that in 'explanation' the explanandum event lies in the past and in 'prediction' in the future.

Then we have a theory of the explanation of laws, namely

(6) laws are explained by subsuming them under or deducing them from more general, abstract or inclusive statements. Such statements may be regarded as theoretical principles or hypotheses. They may be interpreted descriptively, instrumentally or as fictions. I leave aside consideration of the various theories of theories until Chapter 3.

Next a theory of the explanation of theories and even sciences, viz.

(7) theories and sciences are explained by deductively subsuming them under more basic or general ones. The explained theory or science is then said to have been 'reduced' to the explaining one.¹² Thus we have a theory of the development of science, viz.

(7)* science develops monistically or in a linear fashion so as to leave meaning and truth-value unchanged.

Theories (7) and (7)* are rejected by many philosophers, most notably Popper, committed to other components of the de-

¹⁰ K. R. Popper, *The Logic of Scientific Discovery*, p. 59.

¹¹ See esp. C. G. Hempel, *op. cit.*, Chap. 12.

¹² See E. Nagel, *op. cit.*, Chap. II.

ductivist structure. As Feyerabend has pointed out these theories generate their own restrictive methodology, embodied in the conditions that

(7)** theoretical innovations should be consistent and meaning-invariant with respect to established, i.e. currently accepted, theory.¹³

These conditions tend inevitably to have a conservative effect. And they may be regarded as rationalizing the practice of what Kuhn has called 'normal science'.¹⁴

I do not intend to discuss theories (7)* and (7)** in any detail here. That will be done in Chapter 3. But their connection through thesis (7) with the doctrine of actualism and their consequent dependence upon the presupposition of a closure should be clear.

According to theses (3), (6), (7) and (7)*, the explanation of events, laws, theories and sciences all partake of essentially the same ideal 'deductive-nomological' form. And they share this also (through theories (2), (4) and (5)) with the activities of prediction and the identification of causes. Scientific knowledge then must consist (in part or in whole) of a deductive structure. But which one? Which out of all possible deductive structures is the best (or in Popperian terms, 'least worst')?

Here P₂ gets to work. Thus we have the theories that:

(8) laws, theories and sciences are directly or indirectly confirmed or corroborated by their instances (which constitute the objects of actual or possible experiences); and

(9) laws, theories and sciences are directly or indirectly falsified by their counter-instances (which constitute the objects of actual or possible experiences).

Unless the meaning of theoretical terms is reduced to ostensibly defined instances, as in Machian descriptivism, such criteria cannot be sufficient, but they are normally posited as at least necessary. (8) and (9) have sometimes been interpreted in a conventionalist way. But as what is regarded as 'conventional' is only the decision to accept a report as being genuinely 'observational' (and so capable of furnishing a genuine instance or counter-instance of a putative law), a conventionalist interpretation does not affect the status of the principle itself.¹⁵

¹³ K. Feyerabend, *op. cit.*, p. 164.

¹⁴ T. S. Kuhn, *op. cit.*, Chap. II.

¹⁵ Poincaré is widely regarded as the founder of 'conventionalism' in the

Finally, following on from (9), we have a maxim of scientific practice to the effect that:

(10) scientists should, in formulating their theories, state quite unambiguously the empirical conditions under which they are prepared to reject them (cf (iv) on pp. 63–4 above).

This may also be taken as a criterion of what it is to be ‘scientific’ and ‘unscientific’, viz.

(10)* to be ‘unscientific’ is not to be prepared to state such conditions or having done so to revise them *ex post facto*.¹⁶

Most received philosophy of science is based on a core extracted from theories (1)–(10).^{*} These theories all share one great weakness: they all presuppose a closure. If, as I have argued, the world is in fact open then they must all be more or less drastically revised and in some cases completely rejected. I have already argued against (1) in Chapter 2 (especially §4) above so I will not discuss it separately here. It will be remembered that the criterion of open-ness is the non-invariance of empirical relationships. Now clearly if the law-like statements whose antecedents are instantiated in open systems are interpreted as invariant empirical regularities they must be regarded as false. But this means that there can be neither laws, because there are no invariant empirical regularities; nor theories, because they are continually being falsified; so that neither explanation nor prediction can be given a rational basis. I have examined two actualist responses to this predicament (viz. weak and strong actualism) and showed how neither can sustain the concept of laws applying transfactorially, viz. in open and closed systems alike, that we need to render intelligible both the experimental establishment and the practical application of our knowledge.

It is important to keep the deducibility and covering-law requirements, as expressed in (3a) and (3b) distinct. For either can be non-trivially satisfied without the other. I have already shown in 2.6 how one can have explanation in terms of a network of normic statements (which may be strictly universal in the

philosophy of science. For a conventionalist interpretation of Popper see I. Lakatos, ‘Criticism and the Methodology of Scientific Research Programmes’, *Criticism and the Growth of Knowledge*, eds. I. Lakatos and A. Musgrave, pp. 104ff.

¹⁶ See e.g. K. R. Popper, *Conjectures and Refutations*, Chap. I and *passim*.

sense of space-time-invariant) without the event being deducible. Of course the sense in which the covering-law ‘covers’ in this case is different. In the same way but from another aspect, the deducibility requirement may be violated in the development of science although all the statements involved are universal. An example of this is given by the way in which Newton’s theory both explained and corrected Kepler’s and Galileo’s laws.¹⁷ In these cases (3b) is satisfied but not (3a). On the other hand our atavistic causal explanation ‘Tania pushed the door open’ is deductive, though no laws are involved. It is also possible for the deducibility requirement to be satisfied by statements mentioning named individuals or specific space-time regions.¹⁸ It should be noted that one can have deductive ‘explanations’ of events under transient empirical regularities but not deductive explanations of their explanations: for the space-time restriction cannot itself be derived from a strictly universal law.

Thesis (9) implies that all law-like statements whose antecedents are instantiated in open systems are false. It is therefore as stated quite useless as a decision rule for choosing between different law-like statement or as an ‘organon of criticism’.¹⁹ One way of dealing with this would be to allow theory a role in grounding laws. But with an unchanged ontology this is bound to be ultimately unsatisfactory. Of course if we possess a good theory it is irrational to relinquish it in the face of recalcitrant facts – without a better one. But our justification for holding on to the theory must be that it might eventually be able to explain them (by suitable modifications, refinements or developments) or show that they are not after all facts (i.e. that the statements used to state them are untrue). If it could never explain and/or correct them this justification would collapse. Of course the sufficiency of Nicod’s criterion must be disputed: the grounds for a law or theory cannot be *exclusively* empirical. Theory must supply some idea of a ‘connection’, without which it would be impossible to tell necessary from accidental sequences. But in an open world Nicod’s criterion cannot

¹⁷ See e.g. P. Duhem, *The Aim and Structure of Physical Theory*, Chaps. 9–10.

¹⁸ Cf. A. Donagan, ‘The Popper-Hempel Theory Reconsidered’, *Philosophical Analysis and History*, ed. W. H. Dray, pp. 136–8.

¹⁹ K. R. Popper, *Objective Knowledge*, p. 21 and passim.

be necessary either: the grounds for a law or a theory cannot be *undifferentiatedly* empirical. For the conditions must normally be carefully controlled so that a hypothesis about the connecting mechanism can be put to a fair test.

If it is wrong to regard law-like statements and theories as being falsified by the non-occurrence of their consequents in open systems, it is equally wrong to regard them as being confirmed or corroborated by their occurrence in an undifferentiated way, i.e. independently of the context in which the putatively falsifying/confirming instance occurs. Theses (9) and (8) must therefore be restated so as to place a restriction on the system in which a genuinely falsifying/confirming instance occurs, viz. that it be closed.

Not all evidence is equal; or rather not all evidence is evidence for or against a law. In general it takes a closed system to furnish evidence capable of falsifying or confirming a law. And within the class of closed systems, experimentally closed ones are preferable.

For experimentally we can test and re-test a greater number and variety of subjunctive conditionals of the form 'if x were to take on a certain value, then y_j would occur' by instantiating their antecedents. Whereas outside the laboratory we are restricted to observing whatever sequential performances nature is obliging enough to put on.

Similar considerations apply to thesis (10). One cannot lay down hard and fast criteria spelling out beforehand which observable situations 'if actually observed mean the theory is refuted' (see (iv) on pp. 63–4 above). For one can never know beforehand whether the system will be actually closed. On the other hand the closure of the system is not a part of the observable situation; so that it cannot be incorporated into the criterion of scientificity without destroying it. The judgement that the system is closed can only be made *ex post* *after* we have observed (and theoretically assessed) the observable situation.

Theses (3) and (9) postulate a syntactical identity between explanation, prediction and falsification in that, taken together, they imply a correct prediction explains and an incorrect prediction falsifies. They depend upon the assumptions that it is possible to give a purely syntactical account of scientific

activities and that these activities always occur in the context of an antecedently given closure. Recent philosophy of science has clearly demonstrated the poverty of the former; it is with the incorrectness of the latter assumption that I am here concerned.

Now explanation in open systems, failing the attainment of an antecedent closure, normally requires, as I have pointed out in 2.6 above, retrodiction; that is the inference from present effects to prior (perhaps hidden, perhaps just unrecorded) causes, via the application of normic statements. Now the significance of this activity is that it presupposes a non-conventional division of the class of law-like statements into those which are and those which are not capable of functioning in this way, i.e. into those which are accepted (for transfactual application) and those which are still under test. Now once we allow this the postulated symmetries between explanation and prediction and explanation and falsification break down. And it becomes important to distinguish between two kinds of prediction conflated in deductivism's syntactical account of science: practical predictions of categorical form which are rarely made in science but which are important in some of its practical applications in open systems and about which the applied scientist-can never be deductively certain; and test predictions of hypothetical form made under effectively closed conditions in order to test a theoretical hypothesis or putative law.

It is easy to see why the explanation/falsification symmetry collapses once we allow the legitimacy of retrodiction. For the activity of retrodiction presupposes the truth and applicability of the law used; the possibility that it is false is ruled out a priori. Now the intelligibility of falsification depends upon the idea that the would-be falsifier has independent grounds for the occurrence of the initial conditions. If the legitimacy of retrodiction is denied a vicious regress back to sense-experience ensues. Thus suppose we have a law-like statement of the form S , 'whenever events of type E_0 occur events of type E_1 occur'. For S_1 to be used to explain the occurrence of E_1 in a way which is consistent with the idea of its being subject to falsification independent grounds for E_0 are required, say G_0 . But the Connection between E_0 and G_0 is itself a contingent causal one,

which may be represented by the hypothesis S_2 'whenever events of type G_0 occur events of type E_0 occur'. Hence we need independent grounds for the occurrence of G_0 say G'_0 if our use of S_2 is to be consistent with the idea of its falsifiability. But as G'_0 stands in a contingent causal relationship we need independent grounds for it too, and so on. . . . There is of course only one connection with E_0 which, being non-causal and non-contingent, does not require independent grounds, namely immediate sense-experience. Thus insistence on independent grounds for the initial conditions of an explanation, which is an inevitable consequence of the idea of its susceptibility to falsification, inevitably leads to the requirement that the initial conditions be apprehended in sense-experience; in which case the event *could* have been predicted. The root of the trouble here is that the causal relationship is taking too much strain: it is required both to be contingent (and as such to be subject to falsification) and to explain; functions that it cannot combine without vicious regress to sense-experience. Once we distinguish between open and closed systems, however, this regress can be avoided. For we may allow that events may be explained in open and closed systems alike, but that law-like statements may only be falsified under effectively closed conditions (where deductive test predictions are possible).

Now the point of the explanation/prediction symmetry thesis is vitiated in open systems. For we can give excellent explanations, in virtue of the transfactual applicability of our independently validated knowledge, where we are incapable of any predictions (save perhaps of the most immediate or the most tentative sort). Moreover the kinds of statements involved in the two activities are radically different: explanation proceeding by way of normic, and prediction by way of empirical, statements. An empirical generalization typically merely generalizes the problem to be explained, whereas a normic statement locates it in the context of an explanatory theory. On the other hand, normic statements may be inferior predictors to the most crude generalizations or rules of thumb. Further, the occurrence of the event itself may be a practically necessary condition of our knowledge of the former state of the system, as in the case of the collapse of a bridge or an aeroplane crash, or even of the kind of system with which one is concerned, as in the case of the

sudden onset of uncontrollable hysteria.²⁰ Again, the intelligibility of much practical science of an exploratory kind, such as prospecting for oil, depends upon the existence of a radical asymmetry between explanation and prediction. Because of such difficulties defenders of the symmetry thesis have been forced to modify it so that it requires only that *were* we to be in possession of all the information available at the time of the explanation then we could have predicted it.²¹ Now I have argued in 2.6 that it is possible to give a complete explanation of an event without thereby being in a position to deduce it, namely if the different generative mechanisms at work are of radically different kinds; so that the reformulated symmetry thesis is either false or, if deducibility is built into the definition of 'explanation', uninterestingly tautologous.²² One further point on prediction. We are only deductively justified in predicting an event if the system is dosed. But there is no way of knowing in advance (at the only time when a prediction is relevant) whether the conditions for a closure will in fact be satisfied. Hence the probability of an event's occurrence can never be 1.

²⁰ Cf. M. Scriven, 'Explanation, Prediction and Laws', *Minnesota Studies in the Philosophy of Science, Vol. III*, eds. H. Feigl and G. Maxwell, esp. pp. 176–90.

²¹ See e.g. A. Grunbaum, 'Temporally Asymmetric Principles, Parity between Explanation and Prediction and Mechanism versus Teleology', *Philosophy of Science Delaware Seminar, Vol. I*, ed. B. Baumrin, p. 73.

²² The interesting question is then of course shifted to that of whether complete explanations must be 'deductive'. Most of the early objections to the deductive model turned on the non-availability of generalisations connecting events like the cracking of radiators or missile failures. Although this was no doubt encouraged by the way in which its advocates presented it, it was somewhat disingenuous of its critics not to realise the possibility of sophisticated reformulations of it. Mandelbaum, for example, has argued correctly that such events must be regarded as complex and analysed into components (M. Mandelbaum, 'Historical Explanation: The Problem of Covering Laws', *History of Theory, Vol. I*, pp. 229–42). However he still sees explanation as depending upon a knowledge of laws (which he interprets in the Humean way) covering the component events (*ibid.*, p. 241); and given this, the complex event itself still remains deductively predictable. I have argued, by contrast, that the laws covering the components are normic and that they may involve reference to radically different kinds (so that they cannot be incorporated within a single theory). Hence the complex event, even though completely explained, may not be deducible.

The undifferentiated ontology of received philosophy of science results in the very damaging view expressed in (v) on page 64 above, viz. that the acid test of a theory is its predictive power. On this view the more accurate a theory's predictions – no matter of what or where – the more worthwhile it is retaining. Coupled with permissiveness over the use of the CP clause, such a position provides a powerful rationale for scientific conservatism of any school. It has been used as such by Heisenberg in physics and Skinner in psychology, by Friedman in economics and Osiander in astronomy. But armed with our concept of a complex and differentiated reality we can see what is wrong with it. For, on the one hand, there will always be more than one hypothesis capable of saving any given set of facts, so independent tests for them will always be necessary; and, on the other, it is only under closed conditions that such tests can be decisive. Consistency with the facts is neither necessary nor sufficient for a theory.

Popper does not seem to see the connection between the criteria of explanation and rationality he expounds and the historicist view of social science he condemns. That there is a connection is clear. For if we know that power corrupts and regard this as a true, if trivial, empirical generalization then given only a knowledge of the initial condition that N is powerful we can predict with deductive certainty that N will be corrupted. On the other hand if we cannot know when the initial condition is satisfied the law cannot be applied and so is quite useless for either explanatory or social engineering purposes. If Popper is committed to thesis (3) in social science he is committed to thesis (4) and so to the historicist view of science as the prediction of events (*savoir, pour prévoir*). Popper nowhere denies the applicability of thesis (8) to social science.²³ But he equivocates between a conception of historicism as the view that the aim of the social sciences is the prediction of future events²⁴ and the view that its aim is to make unconditional historical prophecies.²⁵

²³ On the contrary he repeatedly emphasises the essential similarity in the logical form of the natural and the social sciences. According to him what is peculiar to the latter is its subject matter and to history its interest in the particular (see K. R. Popper, *The Poverty of Historicism*, p. 143). But the pattern of explanation is the same. ²⁴ See e.g. *ibid.*, p. 3.

²⁵ See e.g. K. R. Popper, *Conjectures and Refutations*, p. 339.

But the authors he attacks did not make unconditional historical prophecies: Hegel in fact made no predictions, and Marx only conditional ones. Popper's real argument is not against predictability as such in social science, but against the predictability of certain kinds of social events, viz. large-scale social changes and their consequences. It is an argument against certain theories of social becoming which he interprets historically. Indeed one could almost say it is an argument for an a-historical form of historicism (in which the laws involved are regarded as strictly universal) against an historical one (in which they are regarded as spatio-temporally restricted). Historicism is in general invalid in all its forms. And it is invalid in both natural and social science. And for exactly the same reasons. Incidentally, this is true for the refutation of historicism that Popper derives from the logical impossibility of predicting the precise effects of future knowledge in as much as any such prediction depends upon a knowledge of that knowledge.²⁶ For its effects on nature are no less predictable than its effects on men.²⁷

I turn now to the central unifying theory of explanation, viz thesis (3) and the theories of causality, viz. (2)' and (2)", that underlie it. Consider once more the paradigm of the kind of context in which a causal claim is made. 'Why is the door opening?' 'Because Tania's pushing it'. Now it is certainly not the case that in saying 'Tania's pushing it caused the door to open' we mean that every time Tania pushes it the door opens. For there are times when it is locked and times when she must turn the door knob too. (Nor equally is it the case that every time the door opens is it because Tania pushes it.) But neither could we produce any universal law which would show why the door opened in *this* particular case. Generally, theories (2)' and (2)" are only plausible if in the case of (2)' we mean to imply and in the case of (2)" we have grounds for supposing that a closure has been obtained of the system in which the events occur. Without this we cannot possibly mean nor can we reasonably be

²⁶ K. R. Popper, *Poverty of Historicism*, pp. vi–vii.

²⁷ Indeed one could even go so far as to say that the latter depends upon the former; that is, that it is only in so far as the effects of future knowledge are unpredictable on nature than they are unpredictable on men, in as much as all human action has a physical aspect but the converse is not the case. A social closure presupposes a natural one but not vice versa; there could be a nature without men, but not men without a nature.

committed to showing that the events which we claim are causally connected are so in virtue of being constantly conjoined. In short the Humean theory cannot be a general theory of causality but at best a theory of what is involved in the making of causal claims in closed systems and where the events are separately identifiable atomistic instants.

Davidson has proposed a sophisticated reformulation of the Humean theory. 'It is an error', he says, 'to think that no explanation has been given until a law has been produced. Linked with [it] is the idea that singular causal statements necessarily indicate, by the concepts they employ, the concepts that will occur in the entailed law. Suppose a hurricane which is reported on page 5 of Tuesday's *Times* causes the event reported on page 13 of Wednesday's *Tribune*. Should we look for a law relating events of these *kinds*? It is only slightly less ridiculous to look for a law relating hurricanes and catastrophies. The laws needed to predict the catastrophe with precision would, of course, have no use for concepts like hurricanes and catastrophies',²⁸ Davidson's analysis of 'singular causal statements' is as follows: "'A caused B" is true if and only if there are descriptions of A and B such that the sentences obtained by putting these descriptions for "A" and "B" in "A caused B" follows from a true causal law'.²⁹ The objection to this suggestion is that it places a requirement on the verification of causal claims which is (a) impossible, (b) useless, and (c) unnecessary. For, on it, 'A' and 'B' stand in, in the original causal claim, for the complete atomistic state-descriptions that would form the antecedents and consequents of such a causal law. But it is presumably only because (at least from a Humean viewpoint) we have not got and perhaps cannot get such state-descriptions that we make the original causal claim in the first case. Davidson holds that such claims are defended by 'producing a relevant law or giving reasons for believing such exists'.³⁰ How then would one set about defending a claim about the causes of the French Revolution? By giving grounds for believing that a true

²⁸ D. Davidson, 'Actions, Reasons and Causes', *Journal of Philosophy* 1963, reprinted in ed. A. White, *Philosophy of Action*, p. 92.

²⁹ *ibid.*, p. 92. See also D. Davidson, 'Causal Relations', *Journal of Philosophy* 1969, p. 701.

³⁰ D. Davidson, 'Actions, Reasons and Causes', p. 92.

‘neurological, chemical or physical law’³¹ exists? But such laws, were they to be known, would cover any set of historical causes of the French Revolution; in short, they would not enable us to discriminate between true and false causal claims at the level we are concerned with; that is, at the level at which the initial causal claim is made. Neurological laws are consistent with any social event and so cannot be possibly used to defend *specific* causal claims involving people. Worse still, in the end the only way of defending the belief that a Humean causal law exists covering some particular case will be by appealing to the truth of regularity determinism. Hence the defence of the most specific causal claim becomes an expression of faith in the philosophical dogma that the world is so constituted that the simple formula ‘same cause, same effect’ everywhere applies. On the other hand, once we deny the premise that a causal claim entails a Humean causal law, and hence a closed system, these absurdities can be avoided; and we can then allow that causal claims are defended, where they require justification in terms of general statements at all, by an appeal to the normic statements of the level at which the original causal claim was made.

Hempel makes a similar mistake in arguing that singular causal statements of the ‘q because p’ (man bites dog) type ‘claim by implication’, ‘tacitly presuppose’, or ‘assert by implication the existence of covering laws.’³² Now it is not clear from his account whether he regards this as part of the meaning of the original causal claim (cf. thesis (2)’ above) or merely as indicating the way in which it is to be justified (cf. thesis (2)'' above). If the former we have the absurdity that ‘man’ and ‘dog’ change their meaning when they are used in the making of a causal claim. If the latter a confusion between what it is necessary to know to ascribe causes and what is necessary for our knowledge of the ascription of causes to be possible, viz. that certain underlying laws hold. The latter is neither necessary nor sufficient for the former.

The non-availability of Humean causal laws is undoubtedly an embarrassment to the modern Humean. Inevitably he falls back on the idea that our explanations are sketches to be filled out in the fullness of time (cf. strong actualism) or that they are

³¹ D. Davidson, *ibid.*, p. 93

³² C. G. Hempel, *op. cit.*, pp. 360, 362 and 363.

subject to an implicit *ceteris paribus* clause (cf. weak actualism). In addition he may relax the stringencies of the deductive model by allowing other categories of explanation: such as statistical, elliptical and partial formulations.³³ Statistical explanations, however, spoil the point of the deductive model: for, as Scriven has put it, ‘they abandon the hold on the individual case’.³⁴ Moreover there is no a priori reason to suppose that the world is not statistically open.³⁵ As for the other categories: all explanations, contextually speaking, are elliptical; and the deductivist must show how partial explanations can be universally applicable.

At the methodological level one of the most unfortunate consequences of the spell of actualism is the blurring of the real differences that exist between the various sciences: both in their subject matters and the degrees to which they have achieved knowledge of them. The experimental sciences have been able, as a result of theoretical endeavour and technical ingenuity, to carve out a chunk of the uncontrolled world and use it as an object of inquiry. The non-experimental scientist has no such easy access. Now the view of the world as closed sets him off in the wrong direction – for it sets him looking for a complete description of a given field. A view of the world as open can, on the other hand, concentrate his endeavours enormously. For it means that all he has to do is to identify and describe (in ways to be considered later) some interesting and significant object of inquiry, without supposing that this will enable him to make deductively successful predictions.

³³ C. G. Hempel, *op. cit.*, pp. 376ff and pp. 415ff.

³⁴ M. Scriven, ‘Truisms as the Grounds for Historical Explanation’, p. 465.

³⁵ The question of the closure of systems is distinct from that of their statistical or non-statistical properties. The latter turns on the deducibility of single as distinct from mass events; whereas the former turns on the stability of empirical relationships. Clearly statistical systems can be open or closed.

3. The Logic of Scientific Discovery

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

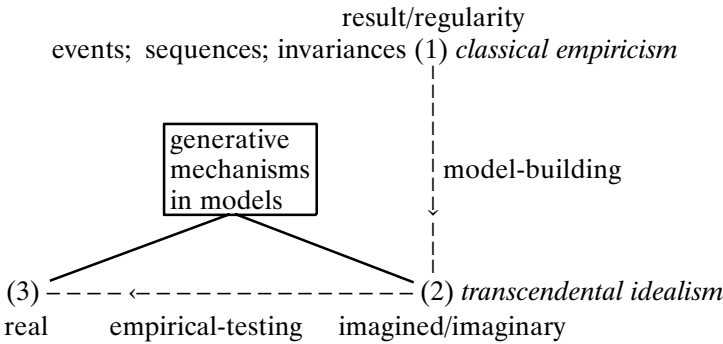
¹ This is the ontological form of Hume's doctrine that events 'seem conjoined, but never connected'. see D. Hume, *An Enquiry Concerning Human Understanding*, p. 74.

of an unailing 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 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 3.1 below. If the classical



transcendental realism

Diagram 3.1 The Logic of Scientific Discovery

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 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

² F. Bacon, *Novum Organum*.

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 I 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 I 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

³ To borrow Toulmin's useful concept. See S. Toulmin, *Human Understanding, Vol. I*, p. 158 and passim.

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

⁴ W. E. Johnson, *Logic, Vol. III*, Chap. 1.

⁵ R. Chisholm, 'The Contrary to Fact Conditional', *Mind* 55 (1946) reprinted in *Readings in Philosophical Analysis*, eds. H. Feigl and G. Maxwell, pp. 482-97

⁶ (a) and (b) correspond of course to Hume's two definitions of 'cause'. See D. Hume, *Treatise*, p. 172 and *Inquiry*, pp. 76-7.

⁷ I owe this term of G. Buchdahl, *op. cit.*, p. 27 and *passim*.

⁸ See e.g. R. B. Braithwaite, *op. cit.*, Chap. 8; C. G. Hempel, *op. cit.*, Chap. 12; and E. Nagel, *op. cit.*, Chap. 4.

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

⁹ The Jesuit mathematician Clavius demonstrated this fallacy in Oslander's apologetic preface to Copernicus' *De Revolutionibus*. Oslander had argued, as Galileo was later invited to before the Inquisition, that the helio-centric theory was merely a mathematically adequate representation of the facts of planetary motion that made no claim to be true. Clavius pointed out that it was never a good argument in favour of a theory that it 'saved the appearances', as a true result could be derived from any number of absurd or false premises. (Cf. J. Losee, *An Historical Introduction to the Philosophy of Science*, pp. 44 5.) Indeed even if we exclude all premises which we know to be false or which are not explicitly defined there will still be an infinite number of sets of premises from which the facts can be deduced, provided we allow for the introduction of artificial predicates such as Hesse's 'tove' (M. B. Hesse, *Models and Analogies in Science*, p. 30), of which place- and time-dependent predicates such as Goodman's 'grue' (N. Goodman, *Fact, Fiction and Forecast*, p. 74) merely form a special class. Hence deducibility cannot provide a sufficient criterion for choosing one set of premises rather than another (the source of Goodman's paradox) or for justifying one statement rather than another as law-like.

¹⁰ This is of course a very poor best. For (i) the simplest of any small number of explanations is not necessarily the best (cf. M. Bunge, *The Myth of Simplicity*, pp. 51–134); (ii) there will still be an in principle infinite number of equally simple explanations, if we restrict ourselves to formal or syntactical criteria alone (cf. J. J. Katz, *The Problem of Induction and its Solution*, Chaps. 4 and 5).

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

¹ J. S. Mill, *A System of Logic*, Vol. I, p. 378.

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 laws¹² or to predict novel kinds of facts¹³ 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 no 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 which its hypothetical subject matter may be imagined to be like in some, but not all, respects the real empirical subject matter of some field which is already known.¹⁶ On this view the surplus-

¹² E. Nagel, *op. cit.*, pp. 64–5. ¹³ I. Lakatos, *op. cit.*, p. 116.

¹⁴ N. Goodman, *op. cit.*, pp. 92–122.

¹⁵ For, as Craig's theorem shows, if it does not the theoretical component is then completely eliminable. See W. Craig, 'The Replacement of Auxiliary Expressions', *Philosophical Review* 65 (1956), pp. 35–55.

¹⁶ N. R. Campbell, *The Foundations of Science*, esp. Chap. 6.

element just is the model. Thus what distinguishes Boyle's law from a merely accidental generalization is, according to Campbell, the corpuscularian model informing the kinetic theory of gases. By means of this model gas molecules are imagined to be, in certain respects, like billiard balls bouncing off each other and exchanging their momentum by impact. And it is in our prior understanding of this that the necessity of the gas laws ultimately lies. Notice that for Campbell it is not the mere availability of a theory or even the organization that the theory makes possible (e.g. the fact that Boyle's law, Charles' law and Graham's law are all deductive consequences of the kinetic theory) but the interpretation of the theory in a model that accounts for the necessity of the law the theory explains.¹⁷

As a critique of the deductivist view of the structure of scientific theories, as typified by Mill, Duhem and Hempel, Campbell's case is a strong one. The deductivist, he says, merely exhibits 'the dry bones of science from which all the spirit has departed'.¹⁸ His project is to revitalize it. He sees the driving force of science as the exploitation of analogies in the conquests of new fields, without which neither theory nor the range of facts could grow or the language in which to state them develop.¹⁹ But is his case unanswerable? How does it fare when faced with the challenge of radical empiricism? Is it capable of providing an adequate account of the surplus-element in the analysis of law-like statements? To answer these questions we must look more closely at the terms of the modelling relationship which is intended to provide the basis for a non-Humean theory of natural necessity.

Now essential to Campbell's correction of the deductivist view of explanation is the idea that for the explanation of a range of phenomena say E_a to have occurred the relationship between the theory T_a which explains the phenomena and from which the latter is deducible must be supplemented and informed by another relationship. This is a relationship of analogy not deduction; and it is by means of it that we render T_a intelligible to ourselves. See Diagram 3.2 below. According to Campbell the entities and processes postulated at T_a , are unknowable; i.e.

¹⁷ *Ibid.*, pp. 126–40. ¹⁸ N. R. Campbell, *What is Science?*, p. 99.

¹⁹ N. R. Campbell, *Foundations*, pp. 132–7; and M. B. Hesse, *op. cit.*, pp. 35–43.

they do not constitute part of the phenomenal world described by science. Although we cannot know what produces E_a we can imagine it to be like something we do know. Such an act of imaginative daring need not be totally arbitrary. For it is possible to conceive of principles of analogical, just as there are principles of deductive or inductive reasoning. Only when we have constructed a model can we be said to have achieved

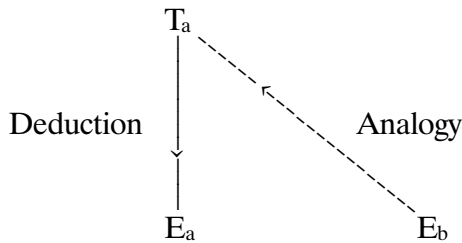


Diagram 3.2
Campbell's 'Theory'

scientific understanding. That is, not just saved the facts, preferably with elegance and economy, but explained them. Using the analogy provided by E_b a real or empirical phenomena can thus 'enliven' the abstract theoretical relationships from which E_a is deduced. And E_b does this by standing in for or representing (in the sense of the German 'darstellung') the unknown causes of E_a . Explanation thus involves, centrally, the substitution in our imagination of a real or empirical relationship for an unreal or theoretical one.²⁰ This is Campbell's debt to empiricist ontology: a debt that it summed up by his tacit acceptance of the concept of the empirical world. For on his theory T_a cannot be, or come to be known as, real; though it is at any moment of time, and perhaps forever, unperceivable to us. For him theoretical entities, such as molecules, can only be said to be 'real' by analogy with material objects.

Campbell does not deny that deducibility is necessary for explanation, merely that it is sufficient. His theory may thus be regarded as providing an alternative shave to Occam's razor. Tyndall formulated the criterion for the selection of explanations

²⁰ N. R. Campbell, *op. cit.*, pp. 243–56

implicit in Campbell's theory as follows: 'ask yourself whether your imagination will accept it'.²¹ Now such a criterion is clearly capable of selecting a theory within a given metaphysical schema, such as that provided by the classical mechanical worldview. But it is not capable of judging between different schemas when it is precisely the nature or the limits of the imagination that is in question. To take an obvious example: Aristotelian and Galilean dynamics are in conflict over whether when a stone falls to the earth, the earth should be conceived as fixed (Aristotle, Ptolemy and Tycho Brahe) or as moving (Copernicus, Giordano Bruno, Kepler and Galileo). Now, try as you may there is no neutral way of conceiving the falling of the stone.²² Our imagination, although not fixed, is either Aristotelian or Galilean. Tyndall's criterion cannot help us to decide between the competing frameworks, because what is in question is the nature of the concept in terms of which any motion has to be understood.

There is a similar break involved in the transition from Newtonian to Einsteinian dynamics. Part of the trouble with current micro-physics is that our imagination cannot accept it and in particular find an adequate pictorial representation for it, and yet we have every reason to believe it to be true. If Tyndall's criterion were acted upon it could have effects on scientific practice as conservative and dogmatic as the consistency and meaning-invariance conditions of classical empiricism. A new scientific ontology or a fundamental change in scientific concepts may transform our conception of what is plausible. At such times in the history of science it becomes necessary for the scientist to stand Tyndall's criterion on its head, and dizzily ask himself whether he can continue to accept his imagination.

Although its inadequacy to deal with fundamental scientific change is most evident, Tyndall's criterion is no less inadequate to deal with the continuing processes of conceptual micro-adjustment, in which our imagination is continually modified and extended, that are a part and parcel of the process of 'normal science'. More generally, it is always legitimate for

²¹ J. Tyndall, 'Scientific Uses of Imagination', *Fragments of Science for Unscientific People*, p. 131.

²² P. K. Feyerabend, 'Problems of Empiricism, Part 11', *The Nature and Function of Scientific Theory*, ed. R. G. Colodny, p. 317.

scientists to ask and sometimes possible for them to answer, questions about whether gases are really composed of molecules or whether the earth really moves. Such questions cannot be rephrased as questions about the plausibility of our conceptions. This would be, in terms of Diagram 3.1, to reduce phase (1) to phase (2). Rather the normal procedure in science is if we have a plausible conception to go on to ask whether it is true, which is to ask whether the entities and processes it postulates are real, or only fictional. Plausibility is a *prima facie* criterion for a theoretical explanation. But it is neither sufficient, nor in the last instance necessary.

How does Campbell's theory fare as a response to the challenge of radical empiricism? According to it, the surplus-element in the analysis of law-like statements is the model at the heart of the theory that explains it. But for Campbell the model cannot prompt questions about the reality of the abstract entities and processes postulated in the theory. For theoretical entities are by definition unperceivable and hence, given the fundamental equation of empiricist ontology, viz. real = empirical, cannot exist. Models function then not as knowledge-extending but as essentially pragmatic devices, servicing the needs of the understanding. Theory involves a journey from one set of experiences E_b to another E_a . Because of this it is always possible for the radical empiricist to ask whether the journey is really necessary. Moreover, even if a way could be found of showing that *some* model is necessary, there would seem to be no way of justifying the choice of any particular one (given that the idea that its necessity could be demonstrated a priori is rejected as being inconsistent with the fact of scientific change).

To this it may be contended that models are necessary not only as conceptual crutches for the tender-minded and as heuristic devices for the young (which the radical empiricist may graciously concede) but for a theory's growth and development, and in particular (so as not to beg the issue by positing non-empiricist criteria of development) for the generation of facts empirically relevant for the theory but which would not have been forthcoming without it.²³ But this only pushes the argument back a stage further. In a completed science models would

²³ See e.g. M. B. Hesse, *op. cit.*, pp. 35ff. Cf. also P. K. Feyerabend, 'Problems of Empiricism', *op. cit.*, pp. 173ff.

be dispensable. For, as Duhem has put it, 'to explain is to strip reality of the appearances in which it is wrapped as in veils in order to see this reality naked and face to face'.²⁴ When we have done this, what more can there be to do? The objection that 'explanations are practical context-bound affairs'²⁵ either is covered by the heuristic role allowed to models or depends upon the incompleteness of science, in which case their nemesis is merely (if perhaps indefinitely) postponed.

We are thus forced inexorably back to a particular conception of reality, the only 'world' that Campbell's account of science contains: the world of Mach and Hume. In such a world causality is bare and invariant conjunction; and scientific knowledge consists, for its part, in 'description, that is the mimetic reproduction of facts in thought, the object of which is to replace and save the trouble of new experience'.²⁶

Suppose now that arguments are advanced to show that no science can ever be complete in the requisite sense. Science still remains, on the Campbellian conception, a purely internal process, locked in a closed circle of thought. Science is still a creature of custom and habit, the only difference being that the habit is now one of the imagination, rather than sensation. In virtue of their shared ontology Campbell is closer to Mach and Tyndall to Occam than one might think.²⁷ In neither case can the possibility of major conceptual revisions be accommodated or the mechanism of scientific discovery be displayed.

Let us apply to Campbell's theory the litmus test for the adequacy of an account of science developed in Chapter 1. Can it sustain the idea of the applicability of the concept in question, viz. that of necessary connection, in a world without men? The answer is obvious. In the case of Campbell, as of Hume, there is still no difference, *independent of men*, between a necessary and an accidental sequence of events. The Campbellian can at best talk of a normically necessary statement; he cannot talk of a normically necessary sequence. The attempt to locate

²⁴ P. Duhem, *op. cit.*, p. 7. ²⁵ M. Scriven, 'Truisms', p. 450.

²⁶ 'This', says Mach, 'is all that natural laws are', *op. cit.*, p. 192.

²⁷ Indeed one might be tempted to see the difference as merely one of taste or temperament as when Duhem compared the 'rolling drums', 'pearl beads' and 'toothed wheels' of the mechanical models of English physicists such as Maxwell, Kelvin and Lodge with his own Cartesian conception of an axiomatic electricity. See *op. cit.*, pp. 70-1.

the surplus-element in the analysis of law-like statements in the imagination of men is a failure.

For transcendental realism the surplus-element distinguishing a law-like from a non law-like statement is the concept of the generative mechanism at work producing the effect in question. Such mechanisms exist and act independently of men; so that the necessity can be properly ascribed to the sequence. Moreover as the world is open not all events will be connected by a generative mechanism; so that the transcendental realist can sustain a concept of natural accident.

Only a real difference between necessary and accidental sequences can justify our distinguishing law-like from non-law-like statements. Hence one cannot deny Humean conclusion (b) (on page 149 above) without objecting to Humean conclusion (a), and thus to the ontology that implies it.

Nowhere is the anthropocentricity of post-Humean philosophy more evident than in the notion that natural necessity must be sought in the behaviour or nature of men. And nowhere is the displacement of rational intuitions more obvious than the attempt to locate structure in the imagination of men. 'Connection' is, as Chisholm has remarked, an 'ontological category and a source of embarrassment to empiricism'.²⁸ But it is not an irreducible one. For its basis lies in the generative mechanisms of nature which connect events as cause and effect and which exist as the powers of things. Thus to assert a counterfactual is not to make a meta-statement²⁹ (which would be to make a statement about its grounds), but to make a statement about the way some thing would have behaved (exercised its tendencies, liabilities or powers) had the conditions in fact been different. Theory is not an elliptical way of referring to experience,³⁰ but a way of referring to hypothesized inner structures of the world, which experience can (in ways to be explored in §3 below) confirm or falsify. We are not locked in a closed circle of thought; because there are activities, viz. perception and experimentation, by means of which under conditions which are deliberately generated and carefully controlled, relatively independent cross-bearings on the intransitive objects of thought

²⁸ R. Chisholm, *op. cit.*, p. 496.

²⁹ See e.g. E. Nagel, *op. cit.*, p. 75; or S. Toulmin, *op. cit.*, p. 185.

³⁰ *Ibid.*, p. 185.

can be obtained. Such activities are not independent of thought, but their results are not implied by them either.

Campbell's achievement is to have seen that scientific theory cannot be identified with a deductive system erected on the basis of a single set of experiences. But he made two mistakes. He too, like the empiricists, missed the essential point that science is essentially developing; so that the hypothetical mechanisms of yesterday may become today's candidates for reality and tomorrow's phenomena. But behind this failure also lay an inadequate intransitive dimension, and in particular the absence of the concept of objects apart from our changing knowledge and possibilities of perception of them. Campbell's theory has been extended in two ways. Some have rectified his first mistake but not his second, and viewed science as a sequence of models, an unfolding process of shifts in intellectual fashion. Others have developed his theory in a realist way. Harré, for example, has drawn attention to the role of the existential questions prompted by the creative use of analogies in the development of science.³¹ By way of concluding my discussion of Campbell's theory I want to sketch out such a dynamized realist version of it.

In Diagram 3.3 below the dotted lines now stand for relationships of deduction and the continuous lines for relationships of

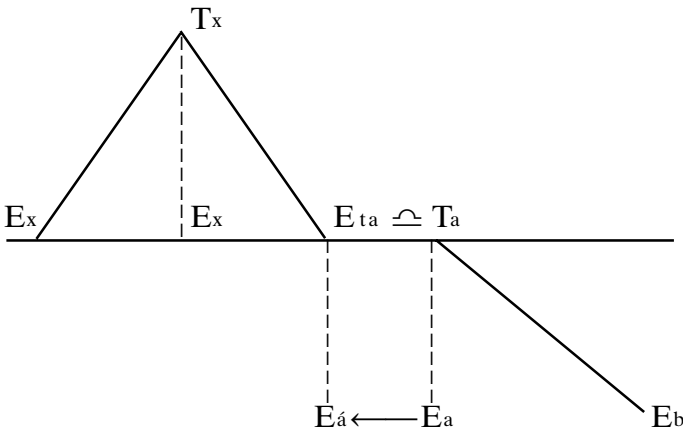


Diagram 3.3
A Dynamic Realist Development of Campbell's Theory

³¹ R. Harré, *op. cit.*, esp. Chaps. 2-3.

analogy (to indicate their reversed relative importance). T_a has come to be established as real, and in this case also is perceivable. In the course of this process facts E_a have been corrected and now become facts E'_a . T_a now provides one of the sources for a new model designed to explain phenomena E_a . And the process of checking its reality (which will almost certainly modify our conception of it) has begun. Needless to say there will in general be more than one model for E_a . The state of chemistry c. 1930 provides an illustration of the model. T_a is Prout's hypothesis and T_a , the theory of sub-atomic structure. $E_a \rightarrow E'_a$ consists in the elimination of the impurities that dogged the verification of Prout's hypothesis for over a century. And the new model might be the Bohr-Rutherford model of atomic structure; which conceived as a hypothesis about the internal structure of atoms is, we now know, false. The source of such models may lie either in some general conceptual scheme (such as atomism in chemistry) or some other science or proto-science (such as the wave models of light and sound in particle physics). The subject of such models is the unknown but knowable intransitive structure of the world. It is by means of the experimental testing of the hypotheses suggested by already existing knowledge that new knowledge comes to be produced.

The problem of the surplus-element, and Hume's challenge, has another aspect. This turns on the question of what warrant we have for distinguishing between cases of genuine and pseudo-falsification, and hence for invoking the CP clause in defence of generalizations in the former case. This calls into question the necessity of deducibility, not just its sufficiency in the explanation of laws.

Science needs a concept of pseudo-falsification for three reasons, two of which are epistemic and one of which is ontological. Firstly, because a theory may not be at present sufficiently refined or developed to cope with anomalous counter-instances; that is to say, every theory needs a 'protective-belt' for its development.³² Secondly, because the 'facts' may be wrong: either in the simple sense that they are misrepresentations of the phenomena or more profoundly because they depend upon

³² I. Lakatos, *op. cit.*, pp. 134-8.

false or inadequate observational theories.³³ As is well known, every new theory is faced with innumerable anomalies and counter-instances of these kinds. They form in a sense the staple diet of normal science. A successful theory is one which, like Newton's, though it never resolves them all and generates new ones in the process of their resolution, 'turns each new difficulty into a victory for its programme'.³⁴ Thirdly, science needs a concept of pseudo-falsification because a countervailing cause or interfering agent may be at work generating the 'counter-instance'. It is only under closed conditions, as we have seen, that a theory can be given a fair test or that a crucial experiment – Bacon's 'instance of the fingerpost'³⁵ – becomes possible.

The problems of the necessity and universality of law are indeed inextricably linked, but not in the way Hume thought. For if the surplus element in the analysis of law-like statements is the concept of a generative mechanism at work and this concept is irreducible to that of a sequence of events then it is quite rational to uphold an ontological distinction between cases of genuine and pseudo-falsification (in which, as exemplified by the case of Prout's hypothesis referred to above, our epistemic distinctions too may be grounded). For we may readily allow that the generative mechanism in virtue of which natural necessity is ascribed is not undermined by the instability of the conditions under which it operates. So that if a law has been confirmed under closed conditions and there is no reason to suppose that the generative mechanism at work in those instances has ceased to operate, the law that the concept of the mechanism grounds may be supposed to continue to apply outside the conditions under which it was confirmed, whether or not the consequent of the statement happens to be realized.

By now it would, I think, be generally agreed that models play some cognitive role in science and that there is a feature about such models which renders them irreducible to the experiences that they are in some way intended to embroider or explain. (This feature is, I have argued, typically an idea of a

³³ As Feyerabend has put it: a theory may be in trouble only because of 'the backwardness of the observational ideology'. See 'Problems of Empiricism, Part II', pp. 292ff.

³⁴ P. S. de Laplace, *The System of the World*, Bk. III, Chap. II.

³⁵ F. Bacon, *op. cit.*, Bk. II, Aphorism XXXVI.

mechanism which would, if it were real, generate the phenomena in question.) But the representatives of the three traditions in the philosophy of science differ radically in their interpretations of the status and role of such models, and of the irreducible concept that constitutes its essential core.

The classical positivist view is that it is merely a heuristic device (Duhem, Hempel and Brodbeck). This is liable to encourage the view that the rationale for distinguishing necessary from accidental sequences is solely pragmatic; that it is, as it were, a question of our greater attachment to the former (Quine),³⁶ or of the deeper entrenchment of their predicates in our conceptual system (Goodman). Similarly it encourages the idea of the CP clause as a device that can be relaxed or invoked, switched off or on, according to whether or not we are prepared to forego the falsified law-like statement. This view carries the implication of course that the use of the CP clause is bound to be more or less arbitrary or dogmatic.³⁷ And this in turn creates the Kuhn-Popper problem of the functions of dogma.

The concept of the generative mechanism may be given a firmer status, and the distinctions it grounds a better rationale, by seeing its function as concerned essentially with the development of science. Protection from pseudo-falsification then becomes protection from too easy or too early falsification; that is, before the full potentialities of the theory have been developed (Lakatos and Feyerabend). This view allows that our knowledge is structured – that it contains, as it were, layers of different age. The conditions of knowing are here explicitly distinguished from the conditions of being. But positivism still provides the underlying account of the world. And because of this the rationale of the concept of the generative mechanism, which forms the heart or essential core of the theory, is still more or less pragmatic, still science- or knowledge- or man-dependent.

The third position consists in coming to see not just our

³⁶ 'Any statement may be held true come what may, if we make drastic enough changes elsewhere in the system', W. V. O. Quine, *From a Logical Point of View*, p. 43.

³⁷ See e.g. K. R. Popper, *Logic of Scientific Discovery*, p. 42 and pp. 80–2; and T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory*, pp. 40–6. I have of course argued (in 2.4 above) on quite distinct realist grounds that once the irrationality of pseudo-falsification is granted the CP clause becomes superfluous.

knowledge but the world itself as structured and differentiated. According to this conception, which is that of transcendental realism, science is concerned neither with the incessant accumulation of confirming facts (or the incessant search for falsifying ones), nor even with its own growth and development, but rather with the understanding of the different mechanisms of the production of phenomena in nature. Thus it allows that under certain conditions the concept of the generative mechanism at work may be given a realist interpretation as a representation in thought of the transfactually active causal structures of the world. The possibility of such an interpretation supplements internal consistency and contextual plausibility as a constraint on the possible forms of theoretical advance; and it constitutes the ultimate goal of all theory construction.

Now empirical realism generates the following dilemma: Either theoretical entities refer ultimately to experience, in which case they can be eliminated. Or theoretical entities constitute experience (in whole or in part), in which case they cannot be eliminated, but must, given the equation of empirical realism, constitute the world (in whole or in part). Now as long as an ontology based on the category of experience is retained there can be no grounds independent of man for ascribing necessity to some but not other statements. On the first horn this generates the problem of what justifies our belief that the future will resemble the past, or the unobserved the observed, i.e. the problem of the induction. But on the second horn it generates the problem of what justifies the assumption of intellectual conformity. And, on this horn, scientific change, or even dissent, actually constitutes (in whole or in part) a breakdown in the uniformity of nature!

3. NATURAL NECESSITY AND NATURAL KINDS : THE STRATIFICATION OF NATURE AND THE STRATIFICATION OF SCIENCE

In the process of the establishment of a law of nature three questions may be asked:

(i) is there an empirical regularity which constitutes a prima facie candidate for a law?

(ii) is there some reason, other than the regularity, why the

predicates instantiated in the law-like statement should be conjoined?

(iii) is this reason located in the enduring powers of thing and the transfactually active mechanisms of nature? If the answer to (i) is yes we have what might be called 'protolaw'.³⁸ If the answer to (ii) is yes we have strong grounds for a law. If the answer to (iii) is yes we have a law. Typically of course the reason in question in (ii) will be provided by a model of the connection between antecedent and consequent, putative cause and putative effect. The transition from (ii) to (iii) typically occurs when a realist interpretation of the mechanism posited in the model becomes acceptable.

The answers to (i)–(iii) correspond of course to three levels of criteria for law, viz. those specified by the classical empiricist, transcendental idealist and transcendental realist philosophies of science. At the Humean level laws just are empirical regularities. At the Kantian level both (i) and (ii) must be satisfied. Here we have what might be called the dual criterion theory of law.³⁹ I have already noted its vulnerability to Humean counter-attack. At the level of transcendental realism, a distinction is drawn between the empirical identifiability and the universal (transfactual) applicability of laws; and the latter is seen to be a condition of the possibility of the former. As the application of laws in open systems is justified, and presupposed by the intelligibility of experimental activity, the existence of an empirical regularity or a constant conjunction of events is now not even necessary for the ascription of a law (see Table 3.1 below). I have argued that it is only at this level that a distinction

Table 3.1
Status of Constant Conjunction of Events

	Necessary	Sufficient	for Law
classical empiricism	√	√	
transcendental idealism	√	x	
transcendental realism	x	x	

³⁸ R. Harré, *op. cit.*, p. 132.

³⁹ This theory is most clearly stated in R. Harré, *op. cit.*, Chap. 4. Although Harré is I think logically committed to, and may be prepared to accept, transcendental realism in the form in which it is developed here, he

between necessary and accidental sequences can be sustained. A sequence $E_a.E_b$ is necessary if there is a generative mechanism M such that whenever E_a , E_b tends to be produced; a sequence is accidental if this is not the case. Their difference is represented in Diagram 3.4 below. Most events occur in open systems and must be conceived, as argued in 2.6 above, as ‘conjunctures’.

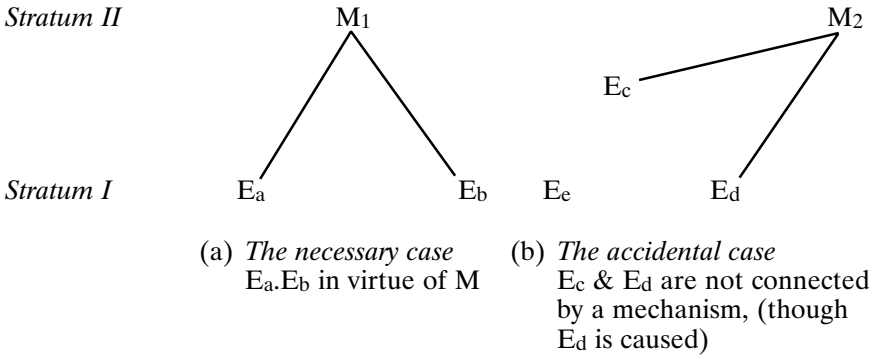


Table 3.4

This is illustrated in Diagram 3.5 below. Necessity as such, like universality, is thus ascribed essentially to the activity of the mechanism; and only derivatively to some particular event sequence. For the result of the activity of the mechanism will

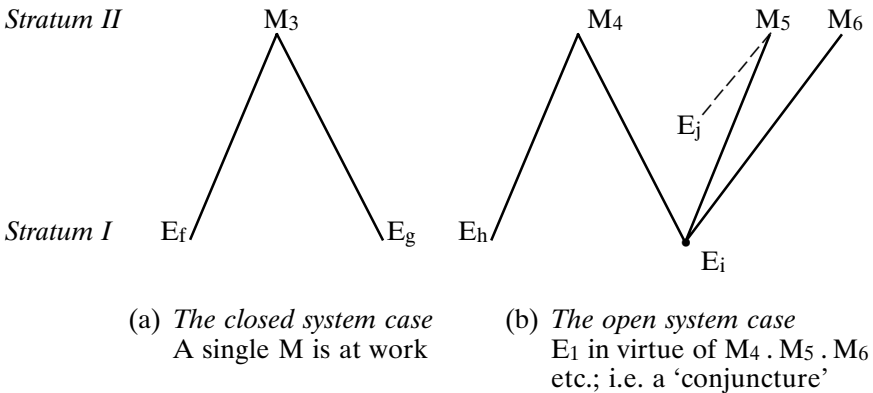


Table 3.5

does not say how laws ‘explain away counter-instances’ and ‘so achieve universality’, *ibid.*, p. 92.

in general be co-determined by the activity of other mechanism too.⁴⁰

Now these three levels of criteria generate and are generated by different views of science. Thus whereas the classical empiricist will ask merely:

(i)* is there a regularity such that whenever C then E? The transcendental idealist will ask in addition:

(ii)* given a regularity, is there an explanation such that we can render it intelligible to ourselves that whenever C then E?. The transcendental realist will however, after making an essential correction, go one step further and ask:

(iii) out of the plausible explanations for this regularity, is there one which correctly describes the mechanism by means of which, upon the occurrence or obtaining of C, E tends to be produced?

That is to say, the transcendental realist will demand that models be tested not just for plausibility but for truth; i.e. for their adequacy in correctly describing the real generative mechanism at work (if the connection between C and E is necessary) such that when C occurs, E tends to be produced (is produced in the absence of interfering causes or the transformation of M). That real things and generative mechanisms must exist can be established by philosophical argument. It is the job of the scientist to discover which ones actually do. Given the identification of some prima facie non-random pattern in nature or protolaw the scientist thus builds up ideas of various plausible hypothetical mechanisms by the creative employment of his imagination (cf. Diagram 3.6) and subjects these ideas to rigorous theoretical criticism and empirical test. These three phases of science are of course those represented in Diagram 3.1 on page 114 above.

For the transcendental realist then a model has a relationship with its subject as well as with its source. And it is within the

⁴⁰ It should be remembered that transcendental realism not only warrants subjunctive and counterfactual statements (where antecedents are un-instantiated) but normic and transfactual ones (where consequents may be unrealised). This is another nail in the coffin of deductivism. For at level (iii) a law may be upheld even when P is true and Q is false; which is of course the only case, according to the principle of material implication, when a conditional is false. The moral is that falsification always depends upon the non-formal requirement that the system in which the putative counter-instance occurs be closed.

nexus formed by this double articulation that new knowledge is produced. For new knowledge is doubly articulated, articulated in two dimensions (transitive and intransitive): it is a socially produced knowledge of a natural (man-independent) thing. It is this bipolarity that a model expresses in standing in two sorts of relationship: a relationship of *analogy* with its source; and a relationship of *adequacy* (when it is) with its subject matter.

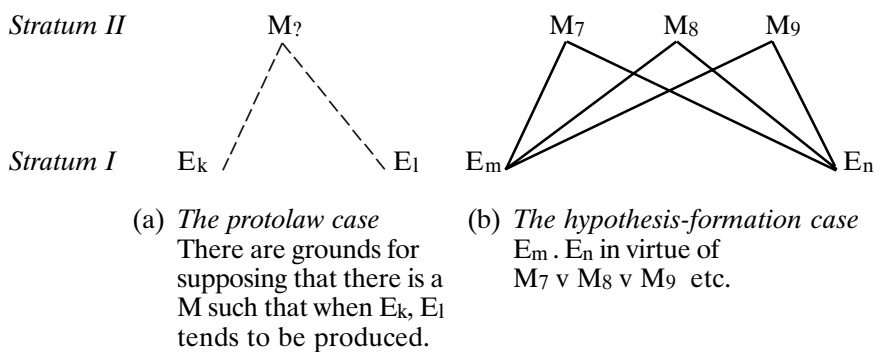


Diagram 3.6

Many philosophical problems arise from a misunderstanding of the second relationship. It is not a relationship of correspondence; the terms of the relationship are not necessarily like each other, though pictures and iconic models may play an important role in scientific thought.⁴¹ Moreover there are no general philosophical criteria for such judgements of adequacy; they are necessarily intrinsic to the particular science concerned. Analogy is one of the possible relationships that models may have with respect to their source. The existence of the first type of relationship (in the transitive dimension) is important in establishing both a constraint on the number of possible explanations⁴² and an indispensable means of their production.

⁴¹ See e.g. N. R. Hanson, 'A Picture Theory of Theory Meaning', *The Nature and Function of Scientific Theories*, ed. R. G. Colodny, pp. 233–73.

⁴² I have argued in §2 above that without such a constraint on the *content* of possible explanations, sorting them with respect to their plausibility, there will be an infinite number of possible explanations, even of equal simplicity. The plausibility of a possible explanation cannot be identified by purely syntactical or formal criteria alone but depends upon a complex relationship between what is so far known about the process generating the behaviour in question and established explanation patterns drawn from

It cannot be described at all adequately as one of coherence; and here again no general philosophical criteria can be laid down for it. Science is work that requires creative intelligence, and there can be no mechanical surrogate for that. The idea of an automatic science is a will-o'-the-wisp that the philosophy of science has pursued, with damaging consequences, since Bacon's search for a 'sure and certain method' that would eliminate the need for human thought, which of course inevitably entails the possibility of human error.

Most science proceeds by way of a two-tiered method designed to identify invariances in nature, normally under conditions which are experimentally produced and controlled, and to explain them by reference to enduring mechanisms.⁴³ It is in the movement from the identification of an invariance to the mechanisms and structures that account for it that the logic of scientific discovery must be found. Thus the observable reactions of chemistry, which are represented in textbooks by formula such as $2\text{Na} + 2\text{HCl} = 2\text{NaCl} + \text{H}_2$, are explained by reference to the atomic hypothesis and the theory of valency and chemical bonding. The patterns which constitute the explananda of the theory of valency are needless to say by no means superficially obvious or readily available. Both the concepts and the substances and conditions had and have to be worked for, produced in the social activity of science. The theory itself sets out to describe the causal mechanisms responsible for the overt behaviour of the substances. Once its reality has been established (which justifies our assuming that chemical bonding occurs and the laws of chemistry hold outside the laboratory) and the consequences of the theory have been fully explored, the next task consists in the discovery of the mechanisms responsible for chemical bonding and valency. This has been explained in terms of the electronic theory of atomic structure. Once the reality of this explanation has been established, science moves on to the discovery of the mechanisms responsible for what happens

analogous fields. It is thus in part a function of the existing knowledge in which the predicates occurring in the possible explanations are already embedded, so that the paradoxes of confirmation, etc. that flow from the insertion of artificial predicates into already-functioning and well-connected scientific contexts cannot (at least at that point of application) arise. Cf. R. Harré, 'Surrogates for Necessity', *Mind* 1973, pp. 355–80.

⁴³ *Ibid.*, p. 366.

in the sub-atomic microcosm of electrons, protons, and neutrons; and we now have various theories of sub-atomic structure. The historical development of chemistry may thus be represented by the following schema:

<i>Stratum I</i>	$2\text{Na} + 2\text{HCl} = 2\text{NaCl} + \text{H}_2$	
	explained by	
<i>Stratum II</i>	theory of atomic number and valency	Mechanism 1
	explained by	
<i>Stratum III</i>	theory of electrons and atomic structure	Mechanism 2
	explained by	
<i>Stratum VI</i>	[competing theories of sub- atomic structure]	[Mechanism 3]

It should be noted that the historical order of the development of our knowledge of strata is opposite to the causal order of their dependence in being. No end to this process of the successive discovery and description of new and ever deeper, and explanatorily more basic, strata can be envisaged. Other sciences reveal a similar open-ended stratification. Geometrical optics is explained in terms of Young and Fresnel's wave optics; which is explained in terms of the electromagnetic theory of light; which can be explained in terms of the quantum theory of radiation.⁴⁴

A general pattern of scientific activity emerges from this. When a stratum of reality has been adequately described the next step consists in the discovery of the mechanisms responsible for behaviour at that level. The key move in this involves the postulation of hypothetical entities and mechanisms, whose reality can then be ascertained. Such entities need not be smaller in size,⁴⁵ though in physics and chemistry this has normally proved to be the case. The species of explanation here identified itself falls-under a wider genus: in which the behaviour of individuals is explained by reference to their natures and the

⁴⁴ Cf. M. Bunge, *op. cit.*, p. 38.

⁴⁵ Cf. G. Schlesinger, 'The Prejudice of Micro-Reduction', *B.J.P.S.*, Vol. 12, pp. 215-24.

conditions under which they act and are acted upon (see 2.3 above).

Now for the transcendental realist the stratification this form of explanation imposes upon our knowledge reflects a real stratification in the world. Without the concept of real strata apart from our knowledge of strata we could not make sense of what the scientist, striving to move from knowledge of one stratum to knowledge of the next, is trying to do: viz. to discover the reasons why the individuals which he has identified (at a particular level of reality) and whose behaviour he has described tend to behave the way they do. Without this concept the stratification of science must appear as a kind of historical accident, lacking any internal rationale in the practice of science (if indeed it is not denied altogether in a reductionist and ultimately phenomenalist account of science).

As it is clear that the hypothetical entities and generative mechanisms imagined for the purposes of theory-construction must initially derive at least part of their meaning from some other source (if they are to be capable of functioning as possible explanations at all) theories must be already understood before correspondence rules for them are laid down.⁴⁶ Equally this means that the descriptive terms must have initially possessed a meaning independent of them. This enables us to see how meaning-change is possible, and indeed if the independence of predicates is denied, inevitable in the transitive process of science. Similarly we can see how knowledge of newly discovered strata may correct knowledge of less fundamental strata, as concepts and measurement techniques are refined. Now if changing knowledge of strata is to be possible the strata must not change with our knowledge of them. Thus the concept of real strata apart from our knowledge of them is necessary if both the ideas of scientific structure and scientific change, which are central to recent critical philosophy of science, are to be intelligibly sustained. More generally, acknowledgement of the real stratification of the world allows us to reconcile scientific discovery (of new strata) with scientific change (of knowledge of strata).

Now the stratification of the world must be assumed by the

⁴⁶ Cf. K. Schaffner, 'Correspondence Rules', *Philosophy of Science*, Vol. 36 (1969), pp. 280–90.

scientist, working in any field, to be in principle unbounded. For it will always be possible for him that there are reasons, located at a deeper level, for the phenomena he has hitherto identified and described. But his knowledge may be in practice bounded by semi-permanent technical or conceptual problems or by the domain assumptions of his particular science; or by the fact that reality is itself bounded at the level knowledge of which he has attained. However, if the stratification of the world has an end, i.e. if there are 'entities' which are truly ultimate – and I can see no reason for supposing this must be so – and the scientist has achieved knowledge at that level, he can never know that the level is ultimate. For it will still remain possible for him that there are reasons, located at a still deeper level, for the causes of the phenomena he has succeeded in identifying and describing. I will return to this point below.

Now the only kind of necessity that holds between events is connection by a generative mechanism. But there are two other concepts of necessity applicable to the objective world order: there is the necessity implicit in the concept of a law, i.e. in the activity of a generative mechanism as such or the exercise of a thing's tendencies irrespective of their realization; and the necessity implicit in the concept of a thing's real essence, i.e. those properties or powers, which are most basic in an explanatory sense, without which it would not be the kind of thing it is, i.e. which constitute its identity or fix it in its kind. The first concept of 'natural necessity' is clearly derivative from the second, dependent upon the contingent feature of the system in which the thing's behaviour occurs, viz. that it be closed (see 2.4 above). I am therefore going to refer to the second as the concept of natural necessity, and the third as the concept of natural kinds. Knowledge of natural necessity is expressed in statements of causal laws; knowledge of natural kinds in real definitions. But natural kinds exist and naturally necessary behaviour occurs independently of our definitions and statements of causal laws.

Now in the transition from knowledge of any one stratum to knowledge of the next, knowledge of three levels of the objective world order is progressively obtained: of relations between events, of causal laws and of natural kinds. I am going to refer to these three levels as the Humean, Lockean and Leibnizian

levels respectively. The transcendental idealist, as well as the classical empiricist, is, in virtue of his ontological commitment, restricted to the first level of knowledge of the objective world order. I shall argue that even at the Leibnizian level science remains empirical, so that the transcendental argument of Chapter 1 remains valid; and that even at that level the deductively justified prediction of events is impossible, so that the critique of philosophy of science contained in Chapter 2 continues to apply with undiminished force. Moreover I shall argue that the concepts, such as that of natural powers, that we need to render intelligible the transition to the Leibnizian level remain categorically valid even at that level.

At the *Humean* level a pattern is identified or an invariance is produced. (This, we know, empirical realism cannot sustain.) We thus have a protolaw (at let us say Stratum I). This is to be explained by reference to the circumstances and nature of the thing whose behaviour is described. The scientist never doubts for a moment that something is generating the effect in question. His problem is: what is ? That is, why does x behave the way it does, viz. B , in conditions $C_1 \dots C_n$?

The first step in the scientific explanation of B is to ascribe a power (or liability) of x to B , i.e. to do (or suffer) ϕ . This is to say, very roughly, that x does ϕ in virtue of its nature N .⁴⁷ The next step is thus to investigate N (defining Stratum II). This involves inter alia creative model-building and rigorous empirical-testing (cf. Diagram 3.1). As a result of this investigation we may say x comes to do ϕ in virtue of its having a certain constitution or intrinsic structure, e.g. genetic constitution, atomic structure or electric charge. Now it is contingent that x has the nature (e.g. constitution or structure) that it has. But given that it has, it is necessary that it behaves the way it does. One criterion of this is our capacity to deduce the tendency to

⁴⁷ Given that B is law-like and allowing for open systems we must say: x tends to do ϕ in virtue of its nature N . A discussion of the rather complex relationship between tendencies and powers must be postponed to the appendix to this chapter. For the moment they may be regarded as a class of powers whose exercise is normically qualified. But this is not a complete analysis. For a power may be exercised when the behaviour is not law-like, so that it would be wrong to attribute a tendency. The logic of power ascriptions, their role in science and the ontological status of powers will be discussed below.

B from N. This is the Lockean level of knowledge. Note that at this level it is still contingent that the thing has the structure that it has.

Now at the third *Leibnizian* level possession of that structure or constitution comes to be regarded as defining the kind of thing that x is. Now it is necessary that x has the structure it has if it is to be the kind of thing it is. It is no longer contingent that hydrogen is a gas with a particular atomic structure; rather anything possessing that structure is hydrogen. That is, the criterion for the application of the concept 'hydrogen' ceases to be the lightest gas and become instead possession of that structure. At this level the only contingent questions are whether and where things of a given kind exist.⁴⁸ But note contingency still lies in the flux of the circumstances in which things act, so that events are still not deductively predictable. That is, the 'contingency' of events deriving from open systems applies even at the Leibnizian level, so that laws must still be formulated as tendencies (whatever their scope of application). Moreover it is important to see that knowledge at the Leibnizian level is, or may be, attained empirically. We may discover, quite empirically, that the most important explanatory property or real essence of hydrogen, identified as the lightest gas, is its atomic structure; and then attempt to express this discovery in a real definition of hydrogen. Once more the importance of viewing science as a process in motion is clear. For if we stay at any one level, phase or moment of science the idea that a definition may be arrived at empirically will appear absurd. If it is accepted, however, the reason why the laws of nature cannot be deduced a priori from self-evident axioms becomes clear. For the axiomatic base of a science at any moment of time, at any stratum of reality, is something that has had to be worked for, produced, as part of the irreducibly empirical process of science.

Scientists attempt to discover what kinds of things there are, as well as how the things there are behave; to capture the real

48 The second question is both distinct from the first and important. Because it raises the question of the range or scope of application of the statements expressing the tendencies of the individuals concerned. It cannot be assumed that all tendencies will be spatio-temporally universal; for individuals and kinds may be transformed in time and bounded in space. A law may of course be universal (transfactually applicable) within its range and restricted in this way.

essences of things in real definitions and to describe the ways they act in statements of causal laws. The real essences of things are their intrinsic structures, atomic constitutions and so on which constitute the real basis of their natural tendencies and causal powers. Thus there is no conflict between explanatory and taxonomic knowledge. Rather, at the limit, they meet in the notion of the real essences of the natural kinds, whose tendencies are described in statements of causal laws.

At the Leibnizian level statements of law are substitution instances of necessary truths about the individuals to which they refer. For any individual which did not behave in that way would not be an individual of that kind. They may thus be regarded as analytic truths. But they are arrived at in the transitive process of science a posteriori, by empirical means. Thus a fully dynamic philosophy of science must take seriously the question ‘how is analytic knowledge arrived at a posteriori possible?’ To this question I will return in §5 below.

The situation at the Humean level is rather like that faced by the citizens of Königsberg who knew, from experience, that there was no way of crossing each of the town’s seven bridges just once.⁴⁹ See Figure 3.1 below. At the Lockean level this fact is deducible from the topology of Königsberg, given Euler’s theorem. At the Leibnizian level, there is a necessary truth about a certain physical set-up, whether or not there is a town called ‘Königsberg’ or any town at all to which it applies. At the Leibnizian level, Mendeleev was able to deduce from his Periodic Table, interpreted as dealing with atomic number and valency, the properties of several new elements. But it remained contingent whether, and if so where, there were elements in the world to which his predictions applied. Certain chess games have

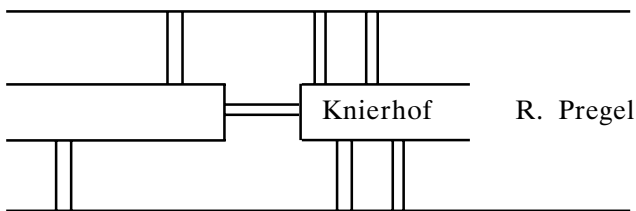


Figure 3.1. *The Seven Bridges of Königsberg*

⁴⁹ Cf. M. Hollis and E. J. Nell, *Rational Economic Man*, Chap. 7.

only one possible solution. But it remains contingent whether they are ever played.

The concept of powers has played a key role in our analysis of science's transition from knowledge of one stratum to knowledge of the next. To ascribe a power is to say that a thing will do (or suffer) something, under the appropriate conditions, in virtue of its nature. This is not, as is so often claimed, a pseudo-explanation⁵⁰ or a purely verbal formula.⁵¹ Rather it is an indication of work to be done. Molière's doctors in *Le Malade Imaginaire* have often been ridiculed for speaking of opium as possessing a 'dormitive virtue'. But in doing so they left open the possibility of an investigation, at some future date, into the nature of opium without committing themselves to what would doubtless have been, for them, a rash conjecture at the time. Moreover it is far preferable to the Humean alternative, viz. that whenever men smoke opium they fall asleep. For in the first place, the latter is untrue. Secondly, it is less informative. It might be a complete accident that everyone in the den is asleep: the powers formula rules this out. It says that there is something about opium in virtue of which when men smoke it they tend to fall asleep. The connection is necessary. But it is only a tendency. Thirdly, the Humean formula is regulatively useless. The powers statement is by contrast quite suggestive. For it indicates the need for an investigation into the chemical properties of opium and the way they induce sleep in men.⁵² In context, it constitutes an open admission of ignorance. The Humean, on the other hand, must pretend that once he has his generalization there is nothing more to be known. And if he should stumble upon a higher-order generalization this can be, for him, only accidentally related to the original one: there is no inner logic connecting the two, or rationale by which science moves from the first to the second.

To this it might be objected that the concept of powers does not figure in the discourse of science. This is true. And the reason for it is of course that the scientist, unlike Molière's doctors, is never just content to ascribe a power but moves

⁵⁰ See e.g. A. Flew, *An Introduction to Western Philosophy*, p. 49; or E. Nagel, *op. cit.*, p. 37.

⁵¹ See e.g. L. Kolakowski, *Positivist Philosophy*, p. 34.

⁵² Cf. R. Harré, *Principles of Scientific Thinking*, pp. 274–5.

immediately to the construction of possible explanations for it with the paradigms and other instruments of thought at his disposal. That is his job. (Sometimes, however, when we are completely at a loss we do just ascribe a power.) The concept of powers is introduced precisely to describe this normally instantaneous (or simultaneous) and unselfconscious response of the scientist to the identification of protolaws; it represents, if you like, an attempt to reconstruct the internal rationality of the inter-strata move. The concept of powers is not intended to figure in the discourse of science, but in the discourse of the philosophy of science (which is the former's rational reconstruction).

It should perhaps be stressed here that the stages of my rational reconstruction of the process of scientific discovery represent phases of scientific activity; they cannot be identified with moments of chronological time. Thus most scientific work must occur, for reasons I will bring out in §4, in the context of a research programme designed to show that on the supposition of the mechanism M the field of phenomena can be rendered intelligible. Thus the identification of a protolaw normally depends upon the prior existence of a conjecture or a hypothesis of a mechanism intended to function as a possible explanation for the presumed protolaw.⁵³

To ascribe a power is to say that there is something about the thing, which may be unknown, in virtue of which it behaves the way it does. The grounds for the ascription of a power must thus be stronger than the mere occurrence of a regularity. For we must possess some reason to suppose the connection necessary (though in the limiting case this may just be the invariance of an experimentally produced result). It is because it indicates the power-ascriber's belief in the existence of a reason, located at the next highest level of inquiry (in the nature of the thing), whether

⁵³ Cf. K. R. Popper, *Conjectures and Refutations*, Chap. 5. However the protolaw itself when it finally emerges, *pari passu* with its explanation, after the imitations and modifications necessitated by the experimental process, may be in a form far more complex and refined than that in which it was originally conceived (cf. S. Körner, *Experience and Theory*, *passim*). The normal response to a (genuine) counter-instance is modification within a continuous research programme, rather than (as is implied by naïve falsificationism) the complete abandonment of the original conjecture and its replacement by a totally different one (cf. I. Lakatos, *op. cit.*).

or not the reason is currently known, that the concept of powers, in pinpointing an essential moment in the transition from knowledge of one stratum to knowledge of the next, plays such a key developmental or strata-bridging role. In this way, a powers statement is a promissory note cashed in the development of science, a schematic explanation filled out in the growth of our knowledge.⁵⁴

It is worth noting that the structure of a powers ascription is well adjusted to accommodate both falsification (obviously, as the hypothesized reasons may be subjected to independent tests) and meaning-change (less obviously). If meaning change is to be possible, some elements of meaning must remain constant through the change. Now if 'x does B' is analysed as 'x is of such a nature N [defining Stratum II] that it will do ϕ in conditions $C_1 \dots C_n$ [defining Stratum I]' we may allow that the meaning of ' ϕ ' remains constant while the meaning of the N component changes between theories (and vice-versa). This applies even in the case of simple descriptive observational predicates such as 'blue'. For 'x is blue' may be analysed as 'x looks blue [defining Stratum I] in virtue of its reflecting light of a certain wavelength [Stratum II]'. The simple theory that things look blue because they are blue may then be replaced by the scientific theory that they tend to look blue in normal circumstances because they reflect light of wavelength 4400\AA . Subsequently we may allow the latter to define the scientific use of 'blue'; in which case of course it is no longer contingent that blue surfaces reflect light of that wavelength.

Now although the concept of powers serves this essential developmental function, it cannot be reduced to it. For when we have climbed up to Stratum II, we cannot throw away the ladder, so to speak. To pursue the analogy, the ladder is a rope, not a wooden one. For to make a powers statement is to make a categorical statement about the nature of the thing situated at the level to which we have climbed. It is to make a statement about possibilities which are possessed by the thing quite categorically, whether they are known (or actualized) or not. Dogs do not lose their power to bark when we understand how they do so, just as glass does not cease to be brittle when we know its molecular structure.

⁵⁴ Cf. R. Harré, *op. cit.*, p. 275.

The ontological bases of powers just are the properties that account for them; i.e. the natures in virtue of which they are ascribed. Now in the transitive process of science such natures may come to be qualitatively described. When this happens it will of course initiate a search for the higher-order entities and mechanisms that account for them. But how does it happen?

In general, at any one level, individuals must be identified and their normic behaviour described. Now for a qualitative description of a thing or a dispositional account of its behaviour it must be present to the scientist's senses and he must be able to describe it correctly, i.e. as being of the kind or type that it is. This will normally depend upon two kinds of work: practical (experimental and technical) work, in which the scientist's causal and perceptual powers are augmented (the latter with the aid of the construction of sense-extending equipment, such as microscopes); and theoretical work, in which the scientist's conceptual and descriptive powers are augmented. It is the aim of the former to produce the object, i.e. to render the thing or behaviour directly accessible to the scientist's senses (so that it becomes the possible object of an act of immediate demonstrative reference). And it is the aim of the latter to produce the concept of the object, so that the scientist is capable of an adequate description of it. Both are necessary for a qualitative description.

It should be noted that the two kinds of criteria, viz. demonstrative and recognitive, are distinct. For my incapacity to identify the chromosome structure by peering down an electron-microscope does not mean that it is not a possible object of an act of demonstrative reference. It is present to my senses, whether I recognize it or not. Conversely to render it accessible to my senses is an independent labour (itself only possible if some concept of it is possessed), requiring great ingenuity, just as experimental production and control does, when we are concerned with the description of the law-like behaviour of some thing already identified. The production of the object and the production of its concept are thus independent tasks, each essential to a qualitative description of a thing or account of its behaviour. The thing must be there and I must know what kind of thing it is, i.e. how to describe it; in general this will involve a theoretical redescription of it.

Now it is important to realize that though the production of

the object and the production of its concept are distinct, the judgement that the object has been produced itself depends upon a tacit theory of vision and the instruments according to which its range is extended. The case of the electron microscope illustrates this very well. In general it is the function of such background or auxiliary theories to specify the conditions under which an object of the appropriate type may be said to be present to the senses. In this sense they constitute, as it were, the criteriology of empirical science.

It is clearly essential to the theory of scientific development proposed here that imagined entities may come to be established as real. Now an entity may be 'theoretical' either in the sense that its existence is open to doubt (theoretical₁) or in the sense that it cannot be directly perceived, either unaided or with the help of sense-extending equipment (theoretical₂). The same distinction applies in the case of behaviour. Now an entity (or mode of behaviour) may be theoretical₁ at t_1 and perceived and adequately described at t_2 , so that it then ipso facto ceases to be theoretical₁. The existence of bacteria, initially conceived as minute hostile micro-organisms, and molecules, initially modelled on material objects, came to be established in this way. This is typical of science and shows once more the importance of viewing it as a process in motion.

But if an entity cannot be perceived, i.e. is theoretical₂, does this mean that it cannot be known to exist, so that it must be theoretical₁? If this were the case all theoretical₂ entities would indeed be hypothetical, and our knowledge would be necessarily confined to the domain of observable things, even if this were now regarded as an expanding class. Fortunately this conclusion does not follow. For theoretical₂ entities may be known to exist indirectly, viz. through the perception of their effects. The paradigm here is the case of the detection of radio-active materials by a geiger counter, of electricity by an electroscope, of a magnetic field by a compass needle. That there is a difference between the cases of detection and perception is clear. In the case of detection the thing can be individuated only indirectly, i.e. via the spatio-temporal framework or through its effects on particular things; it cannot be the object of an act of immediate demonstrative reference. Whatever the mental imagery we use to think of a magnetic field it can be present to us only through

its effects. On the other hand my incapacity to identify a bacterium under a microscope as being of a particular type, or even as being a bacterium at all, does not mean that it is not present to my senses; and so capable of functioning as the object of a possible act of immediate demonstrative reference, although ex hypothesi I am incapable of intentionally performing it.

It should be stressed that in the detection case that something does exist producing the effect is not in question. Nor is the fact that it exists and acts independently of its detection. To say 'electricity is what electricity does'⁵⁵ is to collapse powers to their exercise. Electricity is not what electricity does; but what it can do. The mode of reasoning employed in inferring the existence of causal agents through the ostension of their effects is thus perfectly proper. Hence though it is correct to say that when we cannot qualitatively describe the cause we know less about it than when we can (given that in the latter case we know the thing's causal powers as well) it is not true to say that there is a cause is less certain. It is just that in the detection case what we can know about a thing is limited to its causal powers.

Now there are two possibilities here. One is that there is a nature, susceptible in principle to a qualitative description, as yet unknown, which is the bearer of its causal powers. The other is that the nature of the thing just is its causal powers, as in the case of physical field theories. At any moment of time a science may have to put down its ultimate entities just as powers to produce effects, e.g. to affect observers and equipment, possible observers and possible equipment, material things, in certain ways. About such entities all the scientist knows is their powers. It always remains possible that he will be able to achieve a qualitative description of them, and he must strive to do so. On the other hand, it is also possible that such entities are their powers. The scientist can never dogmatically eliminate one of these alternatives in advance. If there is a frontier to possible knowledge of the world the scientist can never know when he has reached it. But whatever is responsible for the world as manifest must possess causal powers which are continually being exercised; it must be co-extensive with space and continuous with time. It must be structured and complex; it cannot be atomistic or event-like. The concept of a field of potential seems

⁵⁵ See A. J. Ayer, *The Fundamental Questions of Philosophy*.

closest to meeting these requirements.⁵⁶ However it seems to me there is no reason in principle why there should not be strata of fields (of perhaps radically different kinds), forever unknown to us. It should be noted that only the identification, not the existence, of fields depends upon the existence of material things in general. Here again the order of dependence in being is opposite to the order of dependence of our knowledge of being. The ontological order is distinct from the epistemic one.

The general thrust of my argument in Chapter 2 was against reductionism. How does this square with my emphasis on strata of knowledge? It will be remembered that I did not deny the possibility of an explanatory reduction but stressed (a) the need for a well-defined reductans (so that a reduction could not in general be a means of acquiring knowledge of a higher-order or less fundamental stratum); and (b) that a reduction left the reality of the higher-order entities intact, at least in as much as they were causal agents capable of acting back on the materials out of which they are formed (see 2.5 above). It is clear that I was there taking possession of causal powers, and hence existence in time, as the most general criterion of reality. There is an asymmetry between space and time here. For powers must be possessed and exercised in time, but they need not be localized at any point in space. Relations, for example, such as that of spin (in physics) and marriage endure through time and have causal effects. But they have no position in space. Now in general a reduction is possible because the entities in terms of which the behaviour of the thing is explained occupy a different volume of space, either larger or (more usually) smaller. Thus the possibility of a reduction implies in general that the individuals of the different kinds cannot be said to occupy the same place at the same time and one not be part of the other. This gives us a general criterion which imposes limits on regresses of strata, i.e. upon the possibility of a sequence of (explanatory) reductions. For one could define a branch of science as a series of theories within which this criterion is satisfied. On it, quantum mechanics and chemistry would belong to the same branch. But electromagnetism and mechanics, neurophysiology and

⁵⁶ See R. Harré and E. H. Madden, 'Natural Powers and Powerful Natures', *Philosophy*, Vol. 45 (1973), esp. pp. 223–30.

psychology and (it will be argued) psychology and sociology would belong to different branches.

Changes of things are explained in terms of unchanging things. If there are ultimate entities they must be unchanging. Atoms have already been disqualified as possible ultimate entities (see 2.3 above). So ultimate entities must be powers; that is, individuals characterized solely by what they can do. For if one could describe the changing states or conditions in virtue of which their powers were exercised they could not be ultimate (unchanging). In the last instance to be is just to be able to do. But this does not rule out the possibility of a science of cosmology (which would be concerned with the distribution in space and redistribution in time of the ultimate entities) or of irreducibly historical branches of science in which the ultimate entities were Aristotelian or even Strawsonian individuals. The transformation of the principles governing such things would in general have to be conceived as conjuncturally determined open systemic events (see 2.6 above). In this way a complex thing such as a person (or a society) could come to be the cause of its own transformation.

Now it is because we are ourselves material things that our criteria for establishing the reality of things turn on the capacity of the thing whose existence is in doubt to bring about (or suffer) changes in its material constitution or the constitution of some material thing. Space, for example, might be regarded quite abstractly just as any system of relations in which objects stand to one another. And we can conceive the possession and exercise of causal powers in time in ways, and at levels, forever unknowable to men. We can never know where we stand absolutely in the chain of being. Despite this cosmic incapacity, science has succeeded in identifying strata of reality. Now a scientist never doubts for a moment that there are reasons for the behaviour he has identified and described. It is in the search for such reasons, at a deeper level of reality, at present known to him only through its effects, that the essence of scientific discovery lies. This search necessitates the construction of both new concepts and new tools. But, as what is produced must possess a material cause, the scientist stands for his essential task, in two systems of social relationships, depending necessarily on the work of others.

I have argued that the concept of natural necessity is the concept of a real generative mechanism at work, a concept which is applicable to the world quite independently of men. And it is in virtue of their connection by such a mechanism, of which knowledge may be attained in the social activity of science, that necessity is properly ascribed to some but not other sequences. In §5 I will analyse and criticize some objections to this concept of natural necessity and the related concept of natural kinds. But I want to deal here with the following basic objection to the account I have proposed: If, as I have contended, at each stratum or level of reality an entity is identified and its behaviour is described what positive advantages does this account have over the traditional empirical realist ones?

I think it has at least four substantial advantages. First, it reveals the essential movement of science. Second, it allows room for the location of a surplus-element, reflecting a difference independent of men, in the analysis of law-like statements at any one level. Third, it alone is capable of sustaining the ideas of the necessity and universality of laws, which are necessary for the rationality of theory-construction and the intelligibility of experimental activity. Finally, it alone is capable of accommodating the possibility of the existence of entities and the necessary phase of the knowledge of entities which cannot be analysed as substances with qualities, but must be conceived as powers to produce effects, powers which are possessed and may be exercised quite independently of their detection. (Needless to say, these advantages are not independent of each other.)

Science never stops still for a moment. At whatever level we look, it always involves something more than the empirical realist concedes. For example, if we consider the phase of the identification of a protolaw (which seems *prima facie* most susceptible to empirical realist analysis), we find the categorical clause implicit in a powers ascription, representing the scientist's instantaneous response to this situation, indicating his belief in the existence of a reason, located at the next highest level of inquiry, for the predicates being conjoined. Only the powers conceptual system is capable of giving an account of the internal rationality of science, by which it moves from knowledge of one stratum to knowledge of the next, so displaying the actual

historical development of the sciences as something other than a sequence of accidents.

Now it is our knowledge of the reasons at Stratum II for the behaviour at Stratum I that warrants our designating the behaviour as necessary. But the reasons for the behaviour at Stratum II cannot be collapsed into the behaviour at Stratum I or an interpretation or model of that behaviour consistently with the intelligibility and rationality (respectively) of theory-construction or the possibility of empirical test. Nor can such reasons be glossed simply as more fundamental regularities,⁵⁷ if they are to be subject to experimental confirmation (or corroboration).

I have already shown in detail that the empirical realist account of laws, and hence the ontology that underpins it, is defective. Laws, I have argued, cannot be interpreted as conjunctions of events, but must be analysed as tendencies of things. If science is to be rendered intelligible the world must be seen as one of persisting things, of differing degrees of structure and complexity, to which powers and tendencies are ascribed; it cannot be reconstructed as a world of atomistic events apprehended in sense-experience. Briefly, to summarize my account of laws: To invoke a law I must have grounds for supposing a generative mechanism at work. These comprise: (a) independent grounds, preferably under experimentally closed conditions, for the mode of operation of the mechanism; (b) grounds for the satisfaction of the antecedent (or stimulus) conditions for the operation of the mechanism on the particular occasion in question; and (c) the absence of specific grounds for supposing a breakdown or transformation of the mechanism in that case. Generative mechanisms, I have argued, must be analysed as the ways of acting of things; and their operations must be understood in terms of the exercise of tendencies and causal powers. Tendencies may be possessed unexercised, exercised unrealized, and realized unperceived (or undetected) by men.

Finally, the empirical realist cannot deal with the case of entities which just are their powers or about which all we know are their powers. He thus rules out dogmatically, tout court the possibility of a certain kind of entity and a necessary phase of

⁵⁷ See e.g. P. Achinstein, *Law and Explanation*, pp. 13ff.

knowledge. In virtue of this he is no more able to make sense of the frontiers of knowledge, than show the mechanism by which science, if it can and when it does, will advance.

4. THE SOCIAL PRODUCTION OF KNOWLEDGE BY MEANS OF KNOWLEDGE

The basic conception of scientific activity that I have been concerned to advance here is that is (consists in or involves) work. Science, I have argued, must be conceived as an ongoing process of transformation, continually or essentially in motion, in an attempt to capture (i.e. penetrate and describe) the stratification of the world. The logical structure of work is Aristotelian . It depends, in particular, upon the co-presence in any given productive episode of both a material and an efficient cause. Science operates on given materials, including pre-existing theory and antecedently established facts, with given materials, i.e. by means of an ensemble of intellectual and technical tools (including among the former paradigms, models, metaphors and analogies), producing new theories and facts.

Science is produced by the imaginative and disciplined work of men on what is given to them. But the instruments of the imagination are themselves provided by knowledge. Thus knowledge is produced by means of knowledge. The objects from, and by, which knowledge is generated are thus always themselves social products (as is the knowledge generated). Thus science as a process is always entirely intrinsic to 'thought'. However, by perception and experiment access to objects, viz. things and causal structures, existing independently of thought may be obtained.⁵⁸ And of such objects knowledge may be achieved. Science is not an epiphenomenon of nature, for knowledge possesses a material cause of its own kind. But neither is nature a product of man, for the intelligibility of the scientific activities of perception and experiment presupposes the intransitive and structured character of the objects of knowledge, viz. that they exist and act independently of the operations of men and the patterns of events alike.

⁵⁸ As the immediate objects of perception are normally assumed to be short events or momentary states, perceptual access to things presupposes a resolution of the problem of induction (to be discussed in §6 below).

Thus science, I have argued, presupposes the ontological independence and the possible disjuncture of the domains of the real, the actual and the empirical at every stratum or level of reality. At each stratum scientists attempt to identify the entities responsible for what happens at the less fundamental stratum (their point of departure) and describe their normic behaviour. But knowledge of existence, I have argued, cannot be identified with demonstration of it. Causal powers, for example, can only be known, not shown to exist. Hence if, as I have suggested there are grounds for supposing, the ultimate entities in any one branch of science are bare powers, they must necessarily be undemonstrable. However, under certain conditions, some states of things may be perceived, unaided or with the help of sense-extending equipment; and some causes may be demonstrated indirectly, i.e. through the ostension of their effects. But for an existential or a dispositional claim to be confirmed or corroborated the states (or effects) and behaviour must be recognised or identified as being of the asserted type. Hence in general two kinds of criteria, viz. demonstrative and recognitive, must be satisfied for such a claim to be granted. Because the theoretical and technical conditions under which such claims are made (and criteria elaborated) are themselves developing, our knowledge may be extended; and because they may be falsified if the criteria are not met (or revised) our knowledge may be corrected.

The paradoxical air of talking of the correction of knowledge vanishes once the demand for extra-theoretical truth and inter-theoretical synonymy is rejected. Progress, I shall contend, can be shown to have occurred but only from some particular position, some specific vantage point, as it were, in theoretical time.

Science is explanatory, not simply descriptive. Explanation is achieved by reference to enduring mechanisms. Such mechanisms exist as the powers of things and act independently of the conditions that enable us to identify them. Thus there is a direct link between the dynamic realist thesis that the things and causal structures of nature not only exist but act independently of men and the conception of science as a social activity *sui generis* in which both the facts and the conjunctions that, when attainable, provide the empirical grounds for causal laws are

seen as social products. In classical empiricism, in a subtle interchange, these ideas are crossed: so that facts and their conjunctions appear as naturally given and things and causal structures as experiences of men. (In transcendental idealism, the former is seen as in part imposed by, and the latter as unknowable to, men.) Now the identification of the conditions of (knowledge of) being with the conditions of experience in empirical realism leaves 'theory' with a very uncertain status. For it must be either reduced to, or grounded a priori in some necessary condition of, experience; so that it is either reducible or immutable. For transcendental realism theory is both irreducible and mutable. It is always there and liable to change, as part of our socially innate intellectual endowment. It is this endowment that we must draw upon as we attempt to deepen our knowledge of the way things are and act in the world; and in so doing we can continually add to and modify it. The existence of this stock, as a layered structure, is a necessary feature of any human cognitive situation; so knowledge can never be seen as a function of individual sense-experience.

The necessity for a scientific training shows that knowledge is a social product and cannot be conceived as a purely individual acquisition. For it always stands to the individual as something that must be acquired to be used (for scientific work). That science is ongoing implies that some individuals do so. Knowledge shares a feature common to many social products then: namely that though it exists only in virtue of human activity, it is irreducible to the acts of men. For any cognitive act to be possible there must be a material cause; some knowledge established, given to us, already produced. No sum of individual cognitive acts can yield knowledge, for the first member of the series would already presuppose it. Experience is, on the other hand, susceptible to a purely individualistic analysis; 'mass experience' is clearly derivative and analysable as 'the experiences of masses of individuals'. It can thus be seen that underpinning empirical realism is an epistemological individualism. That knowledge is not analysable in terms of individual experiences does not imply that it is not analysable in terms of experience. But that the latter is the case can be seen by reflecting upon the consideration that the antecedent cognitive situation of the individual would have at the very least to contain one theoretical

conjecture, viz. that there were experiences of others. Assuming that the category of experience was allowed to apply inter-subjectively, if all terms were explicitly (ostensively or operationally) defined all truths would be analytic and all falsehoods contradictory, as a claim about the facts would be implicit in the meaning of every theoretical conjecture, so that the point of appealing to experience would be lost; and theory would be incapable of growing and developing. In short, if antecedently established knowledge is to be capable of functioning as a material cause, the layered stock must contain some terms not completely definable in terms of experience; i.e. it must consist of a web of empirical and theoretical ideas.

Recent work establishing that science has a transitive (or sociological) dimension and some facts about its nature has been widely regarded as shocking. That science is a social activity which shares many of the characteristics of, and does not exist in isolation from others; that it depends upon a whole complex of institutions, some of which have little interest in knowledge 'for its own sake'; and that in particular circumstances its fortunes can depend upon the accidents of particular men raises serious moral and political questions. Moreover there are some particularly disturbing features about current science. One need only invoke, from the recent literature, the epithets of entrepreneurial science and shoddy science, reckless science and dirty science,⁵⁹ government science⁶⁰ and mob science,⁶¹ repressive science, Stalinist science and their anodyne anarchist science⁶² to appreciate this. However these problems do not flow from the social character of science per se, i.e. the mere existence of a transitive dimension, but from the present character of its social character. The realization that science has social problems could only be shocking if one had been tacitly viewing it, in the style of Hume, as a kind of behavioural

⁵⁹ J. R. Ravetz, *op. cit.*, esp. pp. 47–59.

⁶⁰ N. Chomsky, 'Objectivity and Liberal Scholarship', *American Power and the New Mandarins*, pp. 23–129.

⁶¹ K. R. Popper, 'Normal Science and Its Dangers', *Criticism and the Growth of Knowledge*, eds. I. Lakatos and A. Musgrave, pp. 51–8.

⁶² P. K. Feyerabend, 'Against Method', *Minnesota Studies in the Philosophy of Science, Vol. IV*, eds. M. Radner and S. Winokur, pp. 17–130.

response to the stimulus of given facts and their conjunctions. This is the positivist concept of behaviourist or automatic science. It is a concept which can itself be used to disguise embarrassing facets and rationalize the practice of a science.

Recognition of the transitive dimension implies that scientific beliefs can no longer be distinguished by their content. For experiences and the facts they generate must now be viewed as socially produced and what is socially produced is socially changeable. There are no absolutely privileged statements. The application of the category 'empirical' becomes relative and theory-dependent. Hence it cannot be used, without a degree of circularity, to establish the scientificity of one class of statements with respect to another. Knowledge, viewed as a transitive process, has no foundation – only a structure in time. The sciences have histories, which like all histories are characterized by both continuity and change; and in which, as in all histories, certain events stand out retrospectively as especially significant, e.g. the discovery of oxygen, the publication of *The Origin of the Species*, the Michelson-Morley experiment. (Later we shall have to inquire into the grounds for their significance.)

Now the fact that scientists do not possess a special attitude or a superior morality does not mean that science does not have a rationality of its own. Nor does the fact that scientific beliefs cannot be distinguished by their content imply that scientific activities cannot be distinguished by their structure or their aim. There are two errors here: the first is to suppose that science is not a social activity in the fullest sense (exactly what this entails we have yet to see). The second is to suppose that it is not equally a social activity, quite unlike any other, *sui generis*: namely a social activity whose aim is the production of the knowledge, with the cognitive tools at its disposal, of the enduring and transfactually active mechanisms of the production of phenomena in nature.

In §3 I described science as the systematic attempt to capture the stratification of the world. Only the concept of a real stratification allows us to sustain the idea of scientific progress, in a way which is both non-inductivist and consistent with the possibility of scientific change. Knowledge of new strata does not dissolve, though it may occasion a correction of, knowledge of old strata. Nor does it render the less fundamental strata

illusory. We do not need the metaphor to which so many writers have found it necessary to resort in order to reconcile progress with change: viz. that of an asymptotic approach to the truth.

Two fallacies must be most assiduously avoided. The first is to suppose that science grows but does not change. The second is to suppose that science changes but does not grow. It is the fact that science grows, i.e. that in the transitive process of science new strata and dimensions (or branches) of reality are discovered, that means that scientific change can be accommodated as a fact of history without sacrificing the idea of scientific progress. But progress can only be shown to have occurred from some substantive theoretical standpoint or position. There is no Archimedean point outside theoretical time. But knowledge changes as it grows. For knowledge at a new level may lead to a revision, correction or modification of knowledge at the previous level. For what is explained is never the 'pure' phenomena, but always the phenomena read in a certain way: i.e. facts. The scientist seeks to describe the mechanisms generating the phenomena; but the results of his activity belong to the social world of science, not the intransitive world of things. Does this mean that it is wrong to talk of the scientist explaining events, describing mechanisms, etc.? No: provided we remember that what is explained in any concrete scientific episode is always the event known under a particular description. This does not mean that the event is, or that we must think of it as if it were, its description. On the contrary, the ontological independence of the event is a condition of the intelligibility of its description. But here, as elsewhere, it is the task of philosophy to analyse concepts, such as that of an event, which can only be used syncategorematically in science.

Scientific activity is continuous. This has meant that 'refutations' have normally taken the form of 'replacements'. Now it has been pointed out that cases of both 'inconsistency' and 'meaning-change' can be drawn from the history of the sciences. For example, Newtonian physics corrected Kepler's and Galileo's laws;⁶³ and the concepts of 'mass' employed in classical dynamics and the theory of relativity are radically

⁶³ See e.g. P. Duhem, *op. cit.*, Chaps. 9 and 10; or K. R. Popper, "The Aim of Science", *Objective Knowledge*, Chap. 5.

different.⁶⁴ Now given that these are the most obvious ways in which scientific changes occur, great care must be taken about the way in which they, and their relationship, are formulated. Kuhn, Feyerabend, and others, have claimed that theories may be so radically different in meaning as to be literally 'incommensurable'. To this there is the obvious objection that if they were literally incommensurable, i.e. shared no elements of meaning in common, it is difficult to see how scientists could have had grounds for preferring one to another. It is clear that at the moment of 'falsification', when one theory is replaced by another, some elements of meaning must be shared in common.⁶⁵ But that the subsequent divergent development of the theories may result in their eventually becoming 'incommensurable'. So that 'inconsistency' and 'incommensurability' refer to distinct moments of the scientific process.⁶⁶ (Something similar must be true of the normal process of education.)

Theory without experiment is empty. Experiment without theory is blind. But in the historical development of the sciences experiment and theory are often out of step. Michelson and Morley did not see their experiment as a refutation of the æther, and Michelson never in fact relinquished his belief in it.⁶⁷ On the other hand Prout's hypothesis could not be vindicated until the invention of physical techniques of chemical separation, a

⁶⁴ See P. K. Feyerabend, 'Problems of Empiricism', *op. cit.*, pp. 168–71, and T. S. Kuhn, *op. cit.*, pp. 100–71.

⁶⁵ I have already suggested in §3 above the role that the concept of powers might play here.

⁶⁶ It is his failure to see this that I think leads Feyerabend into error. For he wants to say (a) that there is bound to come a time when the 'alternatives' do not share a single statement (including observation statement) in common, yet (b) we could still 'choose' between the theories, viz. in terms of the uninterpreted sentences that the scientists testing them would be motivated to produce in observational contexts, *op. cit.*, pp. 214–715. Now what is objectionable about this suggestion is not only that such uninterpreted sentences could never provide *grounds* for a choice cf. e.g. D. Shapere, 'Meaning and Scientific Change', *Mind and Cosmos*, ed. R. G. Colodny, p. 61) but also the idea that we could ever be in a position to make such a choice. For this involves the hypostatization of a whole historical process of meaning-change and its encapsulation in a single notional moment of judgement. In this way it involves a relapse back to the pre-relativistic notion that we can make judgements outside some particular theory and some particular position in theoretical time.

⁶⁷ Cf. I. Lakatos, *op. cit.*, pp. 159–65.

century after its formulation. It is not just the experimental results but what is done with them that counts. On the other hand, the nonfulfilment of experimental expectations can always be explained in terms of the deficiency of experimental techniques.

Scientific activity is itself differentiated into periods or better 'phases' (so as not to identify them with chronological time): viz. into (a) phases of discovery and/or change and (b) phases of application. Both are necessary. I use these characterizations in preference to the emotive and somewhat misleading terms 'revolutionary' and 'normal'. (a) consists in the production of the knowledge of a new stratum or level and/or the radical revision of knowledge at the current one. This is often preceded by the hint or glimpse of a new level or by a crisis induced by the proliferation of anomalous facts of a particularly disturbing kind. The discovery of X-rays illustrates both these facets. Indeed Lord Kelvin initially thought that Roentgen had devised an elaborate hoax.⁶⁸ (b) consists in the application of the discovery and/or change to account for (and perhaps correct) currently established facts and generate new ones.

Needless confusion has been engendered by the failure to distinguish models, theories, paradigms, etc. Very roughly, a theory is a model with existential commitment; that is, a model conceived, and meant to be taken, as true; i.e. a model in which the entities posited and mechanisms described are conceived as real. It is relatively easy for the scientist to invent models, but much more difficult for him to construct theories. There were several models of the æther, but never a satisfactory theory of it. Diagram 3.7 illustrates a schema for the development of science. A general conceptual scheme (abbreviated here to G.C.S.? or metaphysical framework, such as that provided by atomism, 'begets' (logically, not temporally) a research programme, such as that associated with the attempt to explain phenomena by reference to the primary qualities of matter. The research programme in turn generates a theory and/or a sequence of theories either intended for different fields (or different strata) and/or in competition with and replacing each other. At the centre of theory-construction is the process of model-building and technical innovation required for the empirical testing of

⁶⁸ Cf. T. S. Kuhn, *op. cit.*, p. 59.

the various models. These levels are never in complete harmony. Some hypotheses, seemingly necessitated by the facts, are always out of line with the theory generating the facts. Gravity, for example, could never be assimilated to the corpuscularian metaphysical paradigm.

Puzzles or problems are the concrete working data of the scientist. His immediate task is their clarification and resolution. For this he must use the tools he has at his disposal: established results, facts, and theories, promising hypotheses and half-tried

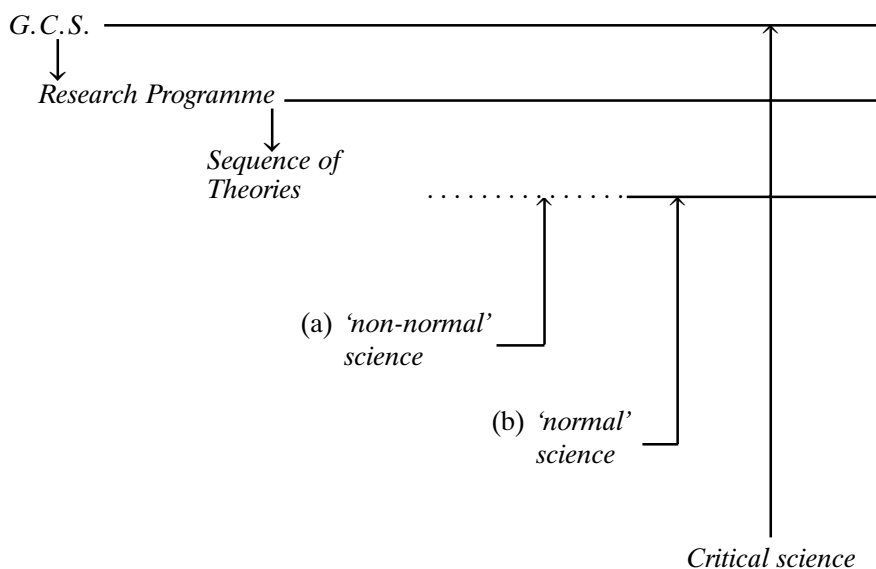


Diagram 3.7. The Internal Structure of Science

Nb. so-called 'non-normal' science corresponds to phase (a) viz. of discovery and change: in it models are invented and subjected to empirical tests with the aim of theory construction.

so called 'normal' science corresponds to phase (b) viz. of application: in it puzzles generated by phase (a) are resolved.

the structure of theory-construction: models + facts + theories + techniques [within G.C.S. + Res. Prog.] → new theories.

the structure of puzzle-solving: facts + theories + techniques + methodological (and heuristic) paradigms → resolution of puzzles.

critical science has no formal structure but may call into question any level, including established theories, successful research programmes and even the G.C.S. or metaphysical framework, leading to the replacement or development of theory, research programme or G.C.S. (e.g. Mach, Einstein).

(or forgotten) ideas, the available formal and technical equipment and usually some methodological (or heuristic) paradigm, concretely embodied perhaps in some exemplary piece of work. It is because such paradigms are shared that there are inter-subjective criteria for assessing the scientific adequacy of his work. The scientist's work normally takes place within a definite institution, a disciplinary matrix and is governed by what some continental writers have called the problematic of his science (that is, roughly the structured field within which alone meaningful questions can be asked or problems posed, expressing the dominant theoretical concerns of the time). The research programme may have its own methodological paradigm, such as Newton's *Principia* or Durkheim's *Suicide*. Now if the problems generated by work at phase (a) cannot be resolved by the material available to the scientist in his own field he must necessarily draw on another. This provides the rationale for paramorphic model-building, a role for analogies and metaphors (the models of discursive thought), leading to existential novelty (e.g. 'what are the punctuation marks in a genetic code?'). Incidentally, this also helps to explain why theoretical innovations are often made by individuals originally working in fields adjacent to the field they innovate, as in the case of Dalton (a meteorologist); or with a strong 'prejudice' taken from an adjacent field, as in the case of Pasteur who was convinced, in opposition to orthodox opinion at the time, that fermentation could not be caused by chemical agents alone but had to be explained by dissymmetrical forces associated with the activity of living organisms."⁶⁹

Conceiving science as work readily lends itself to Aristotelian schematization.⁷⁰ The material cause is antecedently established knowledge, facts and theories; the efficient cause is the methodological paradigm or generative theory at work in the theoretical and experimental activity of men; the formal cause new knowledge, facts and theories; and the final cause knowledge of the enduring and transfactually active mechanisms of nature. Now from the perspective advanced here an event, such as the discovery of oxygen, is significant not just because it refuted the

⁶⁹ See e.g. M. J. Mulkey, *The Social Process of Innovation*, p. 12.

⁷⁰ This has been noted by J. R. Ravetz, *op. cit.*, pp. 116–18. But his use of Aristotle's schema differs substantially from mine.

phlogiston theory of combustion, but because it constituted a decisive moment in the transformation from one way of doing chemistry, viz. that associated with the theory of elective affinity, to another, viz. that represented by post-Daltonian atomic chemistry; that is, because it constituted a transformation in the ongoing activity of chemistry.

Knowledge does not exist in a third world.⁷¹ Rather, it exists in our world, embedded in the scientific community. Without men there would be no knowledge, only its traces. In this sense it depends upon men. But though it exists only in virtue of the thoughts and actions and products of men, it is irreducible to them. For though it would not exist without the activity of some men, its pre-existence is a necessary condition of the activity of anyone. It is a public mix that always antedates the individual. Now it is not necessary that science should continue, i.e. be ongoing. It is contingent that it is. But given this men must reproduce (or more or less transform) the knowledge that is given to them. Men do not construct their knowledge: they reproduce or transform it. This is another way of saying that any knowledge that there is must possess a material cause. Now in general for scientific activity to be continuous all levels depicted in Diagram 3.7 must be represented. The trouble with social science, for example, is not that it has no (or too many) paradigms or research programmes; but rather that it lacks an adequate general conceptual scheme.

I said in 1.6 that an adequate philosophy of science would depend not only upon the development of an adequate philosophical ontology, but upon the development of an adequate philosophical sociology too. This must consist in an answer to the question: what must society be like if science (as a specific kind of social activity) is to be possible? It must satisfy the desiderata of being a structure irreducible to but present only in its effects. Society can only be known, not shown, to exist. It exists only in virtue of the intentional activity of men but it is not the result (or the cause) of their intentional activity. Sociology and psychology thus constitute distinct branches of science, in the sense of the criterion developed in §3 above. Sociology is not concerned with masses of individuals or mass behaviour; but with the persistent relationships between in-

⁷¹ See K. R. Popper. *Objective Knowledge*, esp. Chaps. 3 and 4.

dividuals. Such relationships would not exist without their relata but they do not depend for their efficacy upon any particular relata, any particular named individuals.

Now the autonomy of sociology and psychology accords well with our intuitions. Thus we do not suppose that the reason why the war is fought is the soldier's reason for fighting it, just as the reason why the bar of chocolate is wrapped need not be the chocolate wrapper's reason for wrapping it (though it depends upon the latter). I do not have to know the laws of supply and demand to buy a mackintosh or to know the deep structure of language in order to use it. The deep structure of language may indeed impose limits (like natural structures) upon the kinds of speech acts I can perform but it does not determine what I say. This conception of social science thus preserves the status of human agency, but does away with the myth of creation (logical or historical), i.e. the possibility of a methodologically individualist reduction. It is not necessary that that society should continue. But if it is to do so then men must reproduce (or more or less transform) the structures (languages, forms of economic and political organization, systems of belief, cultural and ethical norms, etc.) that are given to them. The Newtonian revolution in sociology consists in coming to see that it is not necessary to explain society as such; but only the various structures responsible for different societies and their changes. The problem of how men reproduce any particular society belongs to a linking science of social psychology. As so conceived, society may be regarded as an ensemble of powers which exist, unlike other powers, only as long as they are exercised; and are continually exercised via (i.e. in the last instance through) the intentional action of men.⁷²

Established facts are social products. Understanding their logic may help us to clarify the relationships between men and society and men and the world. Here the metaphor of a reading may be used. Its adequacy depends upon the existence of both a given language and an independent text. (But the metaphor is misleading in one way: in that the text of nature exists independently of any language.) It is this that makes possible talk of a correct, rather than just commutatively successful,

⁷² For the concept of powers continually exercised we have of course groomed the concept of tendency.

reading. Men never create this language. For it always pre-exists them. But it exists as an actual, i.e. 'living', language only in virtue of, and changes with, their uses of it. Thus if society is represented by the model of a language it may be regarded as a structure which is always there; which men must reproduce or partially transform; but which would not exist without its 'functionaries'. It is methodologically incorrect to search for an efficient cause of society, though society depends necessarily upon the efficient activity of men. But a reading depends upon antecedent social activity; the acquisition of a language by the reader. It is in this sense that the facts always depend upon social activity. In experience the skilled scientist reads the world as if it were a text in an attempt to understand the mechanisms of the production of phenomena in nature. But his own reading depends upon the mechanisms of the reproduction and transformation of language, of knowledge and of society.

To return now to science: it is contingent that science is continuous but given that it is men must reproduce (or more or less transform) the knowledge that is given to them. The condition that science be continuous is equivalent to the condition that all knowledge possesses a material cause. Hence the criterion of adequacy in the transitive dimension of the philosophy of science is that the account of science should be capable of sustaining the concept of it as an ongoing social activity. It is here that dyadic theories of falsification, that is, theories that conceive falsification as consisting in a confrontation between a single theory and a set of facts fail. If science is to be continuous, refutations must be replacements; which means that always more than one theory must be involved. But, related to this, is an even more basic objection to fallibilism as such. For the refutation of any theory presupposes the *acceptance* of the refuting observation statement. If everything is conditional nothing can be. If all knowledge is (equally) conjectural, no statement can be refuted. Of course it is always possible that the scientist is mistaken in any particular belief (and a good scientist is continually alive to this possibility). But in order to demonstrate a mistake some proposition must be asserted (some theory accepted and framework worked within). In order to learn from our mistakes we must *know* that (and when) we are mistaken. Lacking from fallibilism, as from classical empiricism,

is the key concept of knowledge necessarily possessing a material cause: antecedently established knowledges; science's means of production. It is not necessary that a scientist works within any particular framework or accept any particular theory; but it is necessary that he works within (accepts) one.

Underpinning empirical realism is a model of man in which men are seen as sensors of given facts and recorders of their constant conjunctions: passive spectators of a given world, rather than active agents in a complex one. This model plays a role at least as important as that played by the classical paradigm of action and the celestial closure discussed in Chapter 2. Together they form a complementary triangle (see Diagram 3.8).

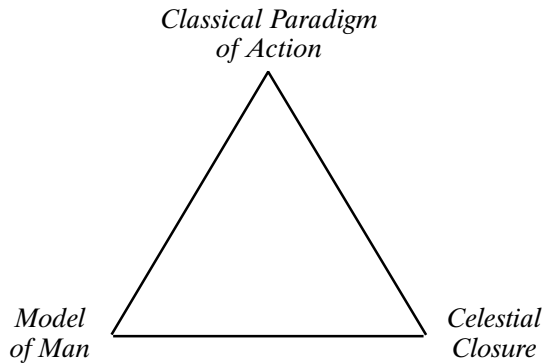


Diagram 3.8. Three Sources of Empirical Realism

Implicit in empirical realism is a conflation between a ground of knowledge, viz. experience, and the world. If experience is to be capable of playing the role traditionally assigned to it of grounding our knowledge (in whole or in part) then the items of which it is composed must be perfectly simple and atomistic; i.e. insusceptible to further analysis or justification. But if it is to define the world then the world must be similarly composed: of atomistic and discrete events (or momentary states) independent of each other. If knowledge is to have its foundations in experience and experience is to define the world then both the ultimate items and objects of knowledge must be atomistic and independent of each other. This creates the problem of what grounds we can have for moving from the observed to the unobserved, or from the actual to the possible (and thence to

the counter-factual). Fallibilism, which shares this model, can no less escape this problem. For the refutation of theory T at time t_1 by an observation statement is consistent with its corroboration by that statement after t_1 unless we are justified in moving from the observed to the unobserved, and from the actual to the possible. (Unless induction is justified or nature is uniform we can never know that a 'mistake' is a mistake; so we can never put our mistakes behind us.)

5. OBJECTIONS TO THE ACCOUNT OF NATURAL NECESSITY PROPOSED

Having outlined the principle advantages of my account of natural necessity and natural kinds (on pp. 183–5 above), I now want to consider some objections to it. In Chapter 4 I will consider the conditions of the plausibility of these objections.

The chief Humean counter-arguments may be put in the form of three theses:–

- (i) there can be no, or at least no knowledge of, necessary connections between matters of fact;
- (ii) if there were necessary connections between matters of fact they would have to be known a priori; so science could not be empirical;
- (iii) men are never directly aware of any causal power or agency or necessary connections between matters of fact, so these concepts cannot be justified by experience (though they may be explained by it; or are, for the neo-Kantian, imposed upon it).

The argument for thesis (i) is typically constructed as follows: there is nothing inconsistent about the supposition that the cause of a phenomenon, say putting a kettle of water on the stove and heating it, should not be accompanied by the effect in question. It is conceivable that water might freeze instead of boil when it is heated. Now thesis (i) is, as stated, highly ambiguous. It is not clear whether it is an ontological or an epistemological thesis (this ambiguity is of course explicit in the way I have formulated it); whether the 'necessity' is logical or non-logical; and whether the 'matters of fact' are events and states of affairs or the statements describing them. Before returning to the argument, we must see exactly what is at stake in it.

Now, it will be remembered, that for the transcendental realist to say that a sequence $E_a . E_b$ is necessary is to say that there is a generative mechanism at work such that when E_a occurs E_b tends to be produced (is produced in the absence of interfering causes). If there is such a mechanism the sequence is necessary; and its necessity is quite independent of any knowledge of it. To analyse the necessity of the connection in terms of our knowledge of the necessity of the connection would be to commit the epistemic fallacy (see 1.4 above). There is a real difference, quite independent of men, between the fact that when I heat the kettle of water it boils and the fact that it boils when the time is half-past two or the colour of my socks is blue. The necessary connections that bind some but not other events together (which are the enduring mechanisms of nature) are quite independent of our knowledge of them.

Statements clearly belong to the epistemic not the ontological order; and logical connections hold only between statements, not between events and states of affairs. Hence the *prima facie* absurdity of those who, in attempting to refute Hume, try to establish that nomic necessity is, or may be, a species of logical necessity.⁷³ Natural necessity is not logical necessity. Natural connections hold between things, events, states of affairs and the like; logical connections between propositions. Moreover there could be a world without propositions, in which the concept 'logical connection' had no application. The laws of logic are not features of the world, nor are they imposed upon it. Rather, we must say: the world is such that changes in it can be consistently described.

Neither natural necessity nor knowledge of natural necessity can be identified with logical necessity. But our capacity to deduce the Wiedmann-Franz law from Drude's theory of electrical conductivity may serve as a *criterion* of our knowledge of the necessity the theory describes. I suggested in §3 above that three levels of knowledge of the objective world order can be

⁷³ See e.g. A. C. Ewing, *The Fundamental Questions of Philosophy*, pp. 159-81 and B. Blanshard, *Reason and Analysis*, Chaps. 11-12. Cf. also. N. Maxwell, 'Can there be necessary connections between successive events?', *B.J.P.S. Vol. 19* (1967), pp. 1-25; and M. Fisk, 'Are there Necessary Connections in Nature?' *Philosophy of Science, Vol. 37* (1969), pp. 385-404.

distinguished in the development of science; so that statements can be classified as definitions, deductive consequences of true theories and simple protolaws according to the position they occupy (at any moment of time) in the development of our knowledge. Hence the deducibility of a tendency from a nature may serve as a criterion at the Lockean level for our knowledge of natural necessity, just as a correct definition may serve as a criterion at the Leibnizian level for our knowledge of natural kinds. But whether or not a sequence of events is necessary is quite independent of the logical status of the proposition used to express it; which is a function of the way it is described in the context of our knowledge; which in turn may be shown to have a certain rationale in the development of science.

Some causal statements expressing necessary connections are logically necessary and some are logically contingent.⁷⁴ For the Humean, however, logical and natural necessity are easily confused. For given the isomorphic relationship between knowledge and the world assumed in empirical realism and restricting our knowledge of nature to the protolegal phase of science (see page 172 above) he naturally comes to regard relationships between events as characterizable in the same kind of way as the statements expressing their relationships are at that phase typically, though not invariably, characterized; namely as contingent. But it is into this very same trap that defenders of the entailment view of natural necessity fall.

I shall construe thesis (i) as an epistemological claim to the effect that knowledge of necessary connections between events is impossible. And I will attempt to refute it by arguing that unless there were necessary connections between some (but not other) events, science would be impossible; and that in science the most stringent criteria for knowledge of natural necessity may be satisfied.

Unless there were necessary connections between matters of fact neither confirmation nor falsification would be possible. For without them no confirmation instance adds any probability whatever to any inductive instance.⁷⁵ On the other hand for it to

⁷⁴ Cf. 'Tania pushed the door open' logically implies 'the door opened'. As Davidson has put it: 'the truth of a causal statement depends upon *what* events are described; its status as analytic or synthetic depends upon *how* they are described', *op. cit.*, p. 90. ⁷⁵ Cf. M. Fisk, *op. cit.*, p. 390.

be rational to reject what is falsified it must be assumed that a hypothesis which has been false in the past will not suddenly become true in the future.⁷⁶ Whether the conclusions of inductive arguments are weakened to probability judgements or it is denied that science is inductive in nature there must be necessary connections between matters of fact. Such necessary connections are provided by enduring mechanisms. Moreover, if experimental science is to be possible, there must be necessary connections between some but not other events. This implies a dynamic principle of indifference: to the effect that mechanisms not only endure but are transfactually active. Neither their enduring nor their transfactual activity is in need of explanation.

Unless there were necessary connections between matters of fact we could have no knowledge, even particular knowledge (in as much as this depends upon inferences beyond what is immediately observed), of the world. For science to be possible then the world must consist of enduring and transfactually active mechanisms; and there must be necessary connections between some but not other matters of fact.

Natural mechanisms are of course nothing other than the powers or ways of acting of things. Thus, if science is to be possible, there must be a relationship of natural necessity between what a thing is and what a thing can do; and hence between what a thing is and what it tends to do, in appropriate conditions. The deducibility of a tendency from a nature thus constitutes a criterion for our knowledge of natural necessity. Events are necessarily connected when natural tendencies are realized.

With this in mind, let us return to a detailed examination of the argument for thesis (i). Is it conceivable that water should not boil when it is heated? Now it might be said straightaway that it is inconceivable to suppose that water might not boil when it is heated. Since anything that did not boil when it was heated could not properly be said to be 'water' at all. That is, that, in Lockean terminology, 'boiling when heated' specifies part of the nominal essence of water; or we could say with Putnam that 'water' functions as a 'law-cluster concept'.⁷⁷ Now

⁷⁶ Cf. R. Harré, 'Surrogates for Necessity', p. 380.

⁷⁷ H. Putnam, 'The Analytic and the Synthetic', *Minnesota Studies in the Philosophy of Science*, Vol. III, eds. H. Feigl and G. Maxwell, p. 376.

the strength of this reply should not be under-rated. I have no doubt that we should ordinarily say something on these lines. Indeed, unless we have some criteria for the correct application of the term 'water' there is no reason why we should use it to refer to substances which as a matter of fact boil when heated rather than to say desk lamps or Saturday afternoons (which do not boil when heated). And such criteria would be at least in part dispositional; appearances, notoriously, can be misleading. Litmus paper that does not turn red when dipped into acid, a metal that does not conduct electricity, or petrol that does not explode when ignited could not be said to be 'litmus paper', 'a metal' or 'petrol' respectively; since the point of referring to the particulars concerned in those ways would be gone.⁷⁸ A magnet that could not magnetize, a fire that cannot burn or a pen that can never write would not be 'magnets', 'fires' and 'pens' at all. Things must satisfy certain criteria for them to be (correctly identified as) the kinds of things they are. By far the most important of such criteria are those that depend upon their powers to affect other bodies (a class which may be extended, analytically, to include their powers to affect observers under specified conditions in certain standard ways).

Such a reply will not however satisfy the Humean (particularly if he believes that definitions are merely matters of convention and cannot express empirically ascertained truths about kinds of things). More to the point it will not satisfy the scientist: for, accepting that 'boiling when heated' specifies part of the nominal essence of water, i.e. the criteria for the identification of a substance as 'water', he will want to know what it is about water in virtue of which it boils when it is heated. That is, he will set out to construct an explanation, in terms of the molecular and atomic structure of water, from which he can deduce its tendency to boil when it is heated. Now it is clearly inconsistent with this explanation to suppose that water might freeze, blush shyly or do anything else rather than boil when it is heated. That is, if the explanation is correct water must boil when it is heated.

Suppose however we came across a stuff which in all other respects looked and behaved like water but which did not boil

⁷⁸ Cf. E. H. Madden, 'Hume and the Fiery Furnace', *Philosophy of Science* 1971, p. 66.

when it was heated. Assuming standard conditions and a closed system (so as to eliminate the possibility of intervening causes) it would seem that we have the following alternatives:–

- (a) our explanation was false;
- (b) the fact that it was intended to explain, viz. that water boils when heated, was false;
- (c) the particular concerned had been wrongly identified: it was not a sample of water after all;
- (d) the particular concerned had changed; so that it had ceased to be water by the time it was heated.

Now the Humean asks us to imagine, and inductive scepticism requires that it be possible, that the cause event occurs and the effect event fails to materialize. Let me call this the critical situation. Now I want to argue that, given only that possibility (a) is ruled out, so that we have a correct explanation, the critical situation is impossible; that is, it is not possible that the cause event occurs and the effect event fails to materialize – in our example, that water is heated and does not boil.

Let me show this. If the explanation is correct water must boil rather than freeze when it is heated (though of course the converse is not the case); so possibility (b) is ruled out. Consider (c), the misidentification of the particular concerned. Now in this case it is not true to say that water did not boil when it was heated. For what did not boil was not water but only something which looked, and perhaps otherwise behaved, like it, say ‘nwater’. Finally consider (d), a change in the particular concerned: what was water when it was put into the kettle at time t_1 ceased to be water by the time it froze at t_2 . Here again it is not true to say that water did not boil when it was heated. For by the time it froze it had become something else, say ‘retaw’. Hence given only the possibility of a realist interpretation of the entities postulated in the explanation, the conditions for inductive scepticism cannot be satisfied. If there is a real reason, located in the nature of the stuff, independent of the disposition concerned, water must tend to boil when it is heated (though in an open world any particular prediction may be defeated). The stratification of nature thus provides each science with its own internal inductive warrant.

Now it might be objected that I have omitted from my list of

alternatives the possibility of the explanation, though correct up to time t_1 , subsequently breaking down. But this possibility equally does not satisfy the requirements of the critical case. For, now at Stratum II (defining the Leibnizian level of the particular movement of science with which we are here concerned), nothing which did not possess the molecular and atomic structure that water has been discovered to possess could be said to be 'water'. So, here again, it would not be water that was freezing. A stuff remains water only so long as its nature (or real essence) remains unchanged. (Of course scientists could make a taxonomic change, but this does bear upon the argument of thesis (i).)

It is of course possible that the nature of some particular will be transformed: in which event, scientists will search both for an underlying substance or quasi-substance which preserves material continuity through change (e.g. a gene pool through species change, an atom in chemical reactions, energy in micro-physics) and for the agent or mechanism which brought about the change. The principles of substance and causality are inter-dependent and complementary. Things persist (and continue to act in their normal way) unless acted upon; and their changes are explained in terms of the action of persisting (and transfactually active) things. If science is to be possible changes must be transformations, not replacements; and transformations must be effected by the actions of causes (causal agents). Things cannot pass clean out of existence or events happen for no reason at all. These are ideals of reason. But if science is to be possible our world must be such that they hold. This entails that it must be a world of enduring and continually acting things. It is of course true that it is impossible to prove that cases of *ex nihilo* production and miracles cannot ever happen. All we can say is that they cannot be known to happen. For it always remains possible for the scientist that what appears to be a case of an *ex nihilo* production or a miracle at time t_1 can come eventually at t_2 to be explained in terms of the transformation of real things and the action of real causes upon them.

I have argued that provided we have a correct explanation the critical situation cannot occur; that, for example, as long as the particular stuff remains water it must tend to boil when heated. But it might be urged if, as I have acknowledged, the nature of

some particular may be changed does this not open the flood-gates of inductive scepticism once more? The answer is no: for there is a big difference between wondering whether some particular will be so acted upon by real causes in its environment that its nature (in this case, molecular structure) will be transformed, so that it ceases to be an individual of that kind; and wondering whether, while remaining an individual of that kind, it will cease to behave in the way that it has tended to behave in the past. The point is even clearer if we generalise it, so raising the questions of the boundaries of kinds and of the scope of application of laws. The difference is between wondering whether water will cease to exist; and wondering whether, while continuing to exist, it will stop boiling (in exactly the same circumstances) when it is heated.

It might be objected that while what I have said clearly covers case (d), viz. that of a particular changing, I have not taken the possibility of case (c), viz. that of a particular being misidentified, of nwater being mistaken for water, seriously enough. What is to prevent us continuously misidentifying particulars in just this way? Now just as particulars may be transformed, so they may be misidentified. But the situation the inductive sceptic asks us to imagine only gets of the ground if we assume that the relevant particulars have been correctly identified. The problem of induction is the problem of what guarantee we have that the unobserved will resemble the observed, or the future the past; it is not the problem of what guarantee we have that we have correctly observed the observed or correctly described the past. The suggestion that what I have here may in fact be a piece of lead piping is irrelevant to the question of what warrant I have for assuming that water will continue to boil when heated or for supposing that there is a necessary connection between water boiling and its being heated.

Nevertheless despite this irrelevance to our present concern, scepticism about particular knowledge can and should be met. It might be met in the following way: Any argument in which the case for the general misidentification of particulars is stated itself presupposes the capacity to identify certain particulars, namely words as tokens of a type and hence possessing a certain standard meaning in a given context. Hence no argument for the general misidentification of particulars can be consis-

tently stated. If this argument does not carry conviction try to imagine a world in which we (a) systematically (b) at random misidentified (α) some particulars (β) all particulars ($\bar{\alpha}$) all the time ($\bar{\tau}$) some of the time. A world in which we systematically misidentified some given class of particulars (such as books as saucers and vice versa) would just be a world in which objects had different names. But a world in which our misidentifications were haphazard or universal is not coherently conceivable. It makes no sense to say that a particular has been misidentified unless one is prepared to say in what respect it has been misidentified. This itself presupposes the capacity to identify the particular as of a certain type. Of course our capacity to identify particulars presupposes the extended or dynamic principle of substance enunciated above, namely that things persist and continue to act unless acted upon, and hence in this way it presupposes the existence of necessary connections between matters of fact. It is up to the criteriology of empirical science to determine whether a particular has been misidentified or a perceptual report is nonveridical. The point is, however, that if science it to be an ongoing concern it cannot persistently demand and persistently return negative verdicts.

It might be objected to my refutation of thesis (i) that I have not considered the possibility that the explanation, which gives each science at any moment of time its own inductive warrant, is incorrect. Now it is of course always possible that we are mistaken in our explanation of why water must boil when heated; that our description of the mechanism in virtue of which it does so is wrong. But this is a general condition of all knowledge; it does not bear on the argument of thesis (i), which concerns the special difficulty of knowledge of necessary connections between matters of fact. I have already argued against the idea that all knowledge is conjectural on the grounds that refutations presuppose acceptances (progress requires a material cause). But whether or not my account of the transitive dimension of the philosophy of science is accepted, refutations presuppose necessary connections between matters of fact.

I have argued that scepticism about change, about our capacity to identify particulars and about the possibility of non-conjectural knowledge as such are all distinct from the special kind of scepticism involved in thesis (i), which is

scepticism about the possibility of knowledge of necessary connections between matters of fact. I have shown how the second and third forms of scepticism, though irrelevant to thesis (i), may be averted. But how can Heraclitean scepticism be countered? Changes in things, I have argued, are explained in terms of unchanging things. The world is stratified. We need only worry about whether atoms will cease to exist when tables and chairs do; we need only worry about whether electrons will cease to exist when atoms do. It is contingent that the world is such that science is possible. But given that it is the dynamic principles of substance and causality that I have formulated must be true of it.

Three further forms of Heraclitean scepticism are possible in which we could be invited to imagine that our world is replaced (a) by a totally different one; (b) by one in which the principles of substance and causality no longer held; and (c) by one in which science ceased to be possible. I shall argue that the replacements envisaged in (a) and (b) are impossible, but that I am precluded by my own premises from saying anything about (c).

In (a) it is supposed that our world could be replaced by a totally different one; but to which, once it had come into being, inductive techniques could be reapplied. Now this is not an intelligible supposition, not only because scientific continuity would be lost during the replacement (so it would make little sense to talk of reapplying inductive techniques), but because there is no possible way in which such a replacement could be affected save by the action of real causes.⁷⁹ In (b) it is supposed that our world might be replaced by one to which the principles of substance and causality do not apply. Now although the existence of our world is contingent, given that it exists the supposition that it might be replaced in this way is not an intelligible one. Transcendental realism demands that we reason from the effect, science, to the condition of its possibility, viz. a world of enduring and transfactually active mechanisms. So we can rest assured that long after mankind has perished things will persist and continue to interact in the world that we once lived in. This leaves us with (c), about which I have said

⁷⁹ It is of course inconceivable that a fundamental entity or entities should act inconsistently with its (their) nature. Hence in the last (non-Laplacean) instance everything is as it must be.

my premises preclude me from speaking. But a moment's reflection shows that (c) is devoid of interest for us. It is an empty counterfactual. For we know as a matter of fact that our world is one in which science is possible. Hence to assert the possibility of a world without science is merely to reassert the contingency of the circumstance that makes a study of the conditions of the possibility of science possible.

I have established that we can have (and that science actually possesses) knowledge of necessary connections between matters of fact. And I have shown how inductive scepticism proper, namely that arising from the assumption of the possibility of what I have called the critical situation, viz. the occurrence of the cause event and the non-occurrence of the effect, can be allayed, viz. by the provision of an adequate explanation; and how the other forms of scepticism often confused with inductive scepticism can be countered. I now turn to theses (ii) and (iii) which the Humean uses to bolster his central contention.

Thesis (ii) alleges that if there were necessary connections between matters of fact they would have to be known a priori, so that science could not be empirical. It is clear that this argument trades on a tacit conflation of logical and natural necessity and the identification of the resultant concept with that of the a priori. To refute it, I will have to show how knowledge of the natures or real essences of things, which I have argued ground our ascriptions of natural necessity, can come to be attained empirically; that is, how a posteriori knowledge of natural necessity is possible.

As there is some misunderstanding about the role of the concept of essence (and, as we shall see, the nature of definition) in science, some preliminary terminological clarification is necessary. The nominal essence of a thing or substance consists of those properties the manifestation of which are necessary for the thing to be correctly identified as one of a certain type. The real essences of things and substances are those structures or constitutions in virtue of which the thing or substance tends to behave the way it does, including manifest the properties that constitute its nominal essence. Science, I have argued, seeks to explain the properties of things identified at any one level of reality by reference to their intrinsic structures, or the structures of which they are an intrinsic part (defining the next level of

inquiry). Thus the dispositional properties of say nickel, e.g. that it is magnetic, malleable, resistant to rust, melts at 1445°C and boils at 2900°C are explained, in the context of post-Daltonian atomic theory, by reference to such facts about its intrinsic structure as that its atomic number is 28, its atomic weight is 58–71 and its density is 8–90. The atomic constitution of nickel is its real essence. But it was discovered a posteriori, in the transitive process of science. And it itself constituted an explanandum of the next phase of scientific inquiry.

In general to classify a group of things together in science, to call them by the same name, presupposes that they possess a real essence or nature in common, though it does not presuppose that the real essence or nature is known. Thus we are justified in classifying alsations, terriers and spaniels together as different varieties of the same species dog because we believe that they possess a common genetic constitution which, despite their manifest sensible differences, serves to differentiate them from the members of the species cat. A chemist will classify diamonds, graphite and black carbon together because he believes that they possess a real essence in common, which may be identified as the atomic (or electronic) structure of carbon, of which these are allotropic forms. To classify a thing in a particular way in science is to commit oneself to a certain line of inquiry. *Ex ante* there will be as many possible lines of inquiry as manifest properties of a thing, but not all will be equally promising. Thus if one's concern is to account for the manifest properties of cucumbers it is clearly preferable to classify a 12 in. long green cucumber under the sortal universal 'cucumber' rather than under the universals 'green' or '12 in. long'. Not all general terms stand for natural kinds or taxa; because not all general features of the world have a common explanation. Carbon and dogs constitute natural kinds; but tables and chairs, red things and blue, chunks of graphite and fuzzy dogs do not. The justification of our systems of taxonomy, of the ways we classify things, of the nominal essences of things in science thus lies in our belief in their fruitfulness in leading us to explanations in terms of the generative mechanisms contained in their real essences. Not all ways of classifying things are equally promising; because not all sets of properties individuate just one and only one kind of thing.

The distinction between real and nominal essences should not be confused with that between real and nominal definitions. Real definitions are definitions of things, substances and concepts; nominal definitions are definitions of words. (Nominal essences are the properties that serve to identify things). Real definitions, in science, are fallible attempts to capture in words the real essences of things which have already been identified (and are known under their nominal essence) at any one stratum of reality. As so conceived, they may be true or false (not just – or even – more or less useful). The atomic weight of copper is 63.5. It would be wrong to claim that it was 53.4 or alternatively that 63.5 was the atomic weight of tin. Of course this fact was discovered a posteriori; but it may now be said to constitute part of the real definition of copper. If the real essence of copper consists in its atomic (or electronic) structure, its nominal essence might consist in its being a red sonorous metal, malleable and a good conductor of electricity etc. Something that did not satisfy these properties could not properly be said to be ‘copper’. But conversely just because the word ‘copper’ in science has a history, and at any moment of time a use, the nominal essence of copper cannot suddenly be designated by the use of ‘reppoc’ or ‘tin’. Nominal definitions in science cannot therefore be conceived as stipulative, arbitrary or matters of convention. Although there is a sense in which any other symbol could have been used to refer to copper; given this usage and that history ‘copper’ cannot be replaced by ‘bronze’ or ‘♀’ for no reason at all.⁸⁰ Changes in the definitions of words in ongoing social activities require justification.

On the view advanced here science consists in a continuing dialectic between taxonomic and explanatory knowledge; between knowledge of what kinds of things there are and knowledge of how the things there are behave. It aims at real definitions of the things and structures of the world as well as statements of their normic behaviour. The source of the failure to see this is the ontology of empirical realism which reduces things to qualities, taxa to classes, enduring and active mechanisms to constant conjunctions of independent and atomistic

⁸⁰ For a discussion of the history of ‘copper’ see M. Crosland, *Historical Studies in the Language of Chemistry* and R. Harré and E. H. Madden, *Causal Powers*, Chap. 1.

qualia.⁸¹ Now if the world consists only of qualia and qualia are independent of one another then the particular names that we give to qualia cannot matter and all qualia will appear on a par. On this conception, predicates must be independent of one another and classification is ultimately arbitrary.

Now just as it is a mistake to assume that science is concerned with any and all behaviour it is a mistake to assume that it is concerned with any and all things. Scientists do not seek to describe the behaviour of or to classify common objects like tables and chairs, though the laws of physics and the principles of scientific taxonomy (e.g. the identification of a table as an oak one) may be brought to bear on them. Now from the fact that tables have no real essence it does not follow that carbon has none. Electrons are not related in the same way as games. A resemblance theory of universals works best for the complex Strawsonian individuals of ordinary life. But the universals of interest to science are real: they are the generative mechanisms of nature which account, in their complex determination, for the phenomena of the world, including (upon analysis) the genesis and behaviour of ordinary things. The dialectic of explanatory and taxonomic knowledge must thus be formulated as follows: science is concerned with the behaviour of things only in as much as it casts light upon their reasons for acting, and hence upon what kinds of things there are; and science is only concerned with things of a particular kind, in as much as they constitute the reason for some pattern of normic behaviour and thus themselves become an appropriate object of inquiry.

The importance of taxa in science may be expressed by saying that what is non-accidentally true of a thing is true of a thing in virtue of its essential nature. A thing acts, or at least tends to act, the way it is. It should be stressed that the difference between a thing which has the power or tends to behave in a certain way and one which does not is not a difference between what they will do, since it is contingent upon the flux of conditions whether the power is ever manifested or tendency exercised. Rather, it is a difference in what they themselves are; i.e. in their intrinsic natures. A copper vase remains malleable even if it is never

⁸¹ To use Goodman's very useful term. See N. Goodman, *The Structure of Appearance*, p. 130 and passim. Goodman himself attributes the term to C. I. Lewis, *Mind and the World Order*.

pressed out of shape. It is contingent whether an electric current is ever passed through a copper wire. But it is necessary, given its electronic structure, that it be a good conductor of electricity. We know how things will behave, if certain conditions materialize, if we know what the things are. But we can only know what things are a posteriori, via the empirical process of science.

This view may be contrasted with the idea that scientists are not concerned with questions such as ‘what is energy?’ or ‘what is an atom?’ but only with questions of the kind ‘how can the energy of the sun be made useful?’ or ‘under what conditions does an atom radiate light?’⁸² Popper’s ‘methodological nominalism’ seems to be based on the idea that to suppose that things have essences is to suppose that it is possible to give explanations which are ‘ultimate’ in the sense that they are unsusceptible in principle of further explanation (which is what he calls ‘essentialism’).⁸³ Although Locke may have held this view, is it certainly no more a necessary feature of the concept of real essence than it is a necessary feature of the concept of behaviour to suppose that because a thing can be described as behaving in a certain way the behaviour itself cannot be subject to further explanation. It is clear that to suppose that things have real essences is not to suppose that the real essences of those things cannot be explained in terms of more fundamental structures and things.

Two other arguments sometimes invoked against the concept of real essences should be mentioned. The first depends upon the assumption that differences in nature are continuous, not discrete; that ‘God makes the spectrum, man makes the pigeon-holes’;⁸⁴ so that ‘genera, species, essences, classes and so on are human creations’.⁸⁵ I can find no possible warrant for such an assumption. Taken literally, it would imply that a chromosome count is irrelevant in determining the biological sex of an individual, that the class of the living is only conventionally divided from the class of the dead, that the chemical elements reveal a continuous gradation in their properties, that tulips merge into rhododendron bushes and solid objects fade gaseously away into empty space. The second involves the belief

⁸² K. R. Popper, *The Open Society and Its Enemies*, Vol. I, p. 32.

⁸³ K. R. Popper, *Conjectures and Refutations*, Chap. 3, esp. p. 102.

⁸⁴ A Flew, *op. cit.*, p. 450. ⁸⁵ *Ibid.*, p. 449.

that to suppose that there are natural kinds is to suppose that these kinds are fixed, and is in particular to rule out the possibility of a mechanism of evolution.⁸⁶ Again, this is completely unwarranted. For natures may change; and whether, and if so the ways in which they do, are matters for substantive scientific investigation. No spectrum exists between men and apes but that does not preclude the possibility of a mechanism of evolution (involving a whole sequence of ‘missing links’). What happens in such cases is that biologists posit a novel entity, a gene pool, as the underlying continuant through the species’ change. The objection is only valid at the level of ultimate physical entities since necessarily if such entities exist they must be enduring.

Scientists attempt to discover the real essences of things a posteriori, and to express their discoveries in real definitions of the natural kinds. From a description of the nature of a thing its behavioural tendencies can be deduced. When such tendencies are realized the events describing the stimulus or releasing conditions for the exercise of the tendency and its realization may be said to be necessarily connected. Thus scientists can come to possess knowledge of necessary connections between events as a result of an a posteriori process of discovery. Scientists are not content to collect conjunctions of events. Rather they try to discover the natures of things. Given this, no problem of induction can arise. Since it is not possible for a thing to act inconsistently with its own nature and remain the kind of thing it is. That is, a thing must tend to act the way it does if it is to be the kind of thing it is. If a thing is a stick of gelignite it must explode if certain conditions materialize. Since anything that did not explode in those circumstances would not be a stick of gelignite but some other substance. Now given the satisfaction of the criteria for the identification of a substance, say water, and the recording, preferably under experimentally closed conditions, of its most significant and suggestive behavioural properties, scientists move immediately to the construction and testing of possible explanations for the protolaws identified. But *if* there is an explanation, located in the nature of the stuff or the system of which the stuff is a part, *whether or not it is known by men*, water must tend to boil when it is heated. It is the real strati-

⁸⁷ S. Toulmin, *op. cit.*, pp. 135–6.

fication of nature that justifies induction in science. It is not we that impose uniformities upon the world, but nature that makes induction (properly circumscribed) a rational activity for men.

The third Humean counter-argument is that we are never directly aware of any necessary connection between matters of fact or causal power or agency so that these concepts cannot be justified by experience. Thesis (iii) thus completes a triangle, whose other sides are theses (ii) and (i). It could be argued that we are sometimes directly aware of necessarily connected sequences (see 2.3 above), and that we are sometimes directly aware of the exercise of causal powers (though the powers themselves can only be known, not shown, to exist; i.e. we are never directly aware of causal powers as such).⁸⁷ It seems clear that we are aware of ourselves as causal agents *in* a world of other causal agents; and that unless we were so aware we could not act intentionally, or come to know ourselves as causal agents at all. (Projective explanations of our idea of necessary connection are clearly anthropocentric.) However for the transcendental realist this is incidental. For, for him, the status of the concept of necessary connection is clear: it has been established, by philosophical argument, as applicable to some but not other sequences of events as a necessary condition of the social activity of science. (It should be stressed that this does not mean that any particular science has correctly identified, let alone adequately described, the necessary sequences: it is a condition of the *possibility* of science.) Thus the concept of natural necessity does not have to be justified in terms of or traced back to its source in sense-experience; though there must be a scientific explanation of how we come to possess the concept.

That science has a posteriori knowledge of necessary connections between matters of fact is a proposition that can be given no further justification.

6. THE PROBLEM OF INDUCTION

In the concluding section of this chapter I intend to argue that traditional approaches to the problem of induction fail; to

⁸⁷ E. H. Madden and P. Harré do not clearly distinguish powers from their exercise in their criticism of this Humean argument in 'The Powers That Be', *Dialogue* 1971, pp. 12–31.

reveal a crucial ambiguity in the formulation of the problem; and to show how transcendental realism can resolve it. In doing so I will be bringing together my critiques of the ideas of the actuality and contingency of law; and I will relate my resolution of the problem of induction to the problem of universals. I shall argue that the condition of the intelligibility of the traditional problem of induction is an ontology of atomistic events and closed systems; but that in our world inductive reasoning may be shown to have a rational place.

The traditional problem of induction is the problem of what warrant we have for reasoning from particular instances to general statements (induction proper) or from observed to unobserved or past to future instances (eduction). Now it is clear that unless we (sometimes) have some such warrant nothing can be justified, shown to be mistaken or called into doubt: memory cannot be relied upon, a mistake demonstrated or grounds for a sceptical conclusion given. (Why, for instance, should the fact that my senses have deceived me in the past be a ground for believing that they will do so in the future?) Indeed complete scepticism about induction seems literally unthinkable.⁸⁸ So pervasive a feature of our social life is inductive-type reasoning that it seems patently unsatisfactory to be told that it is just a contingent fact about the world that induction is successful.⁸⁹ If inductive-type reasoning is necessary, then it seems incumbent upon us to ask what the world must be like for it to be possible; and what must have been assumed (inter alia about the world) for the problem to have remained intractable. The answer to the first question will constitute a set of synthetic a priori truths about the world; the answer to the second a set of synthetic a priori truths about received philosophy of science.

The standard responses to the traditional problem of induction are of course: (i) to deny that science is inductive in nature (e.g. Popper); (ii) to justify induction inductively (e.g. Black, Braithwaite); (iii) to strengthen the premises of inductive arguments, so that they became in effect enthymematic deductive

⁸⁹ Cf. K. Campbell, 'One Form of Scepticism about Induction', *Analysis*, Vol. 23, pp. 80-3, reprinted in ed. R. Swinbourne, *The Justification of Induction*, pp. 144-8.

⁹⁰ P., Strawson, *An Introduction to Logical Theory*, Chap. 9, Pt. II, esp. p. 261.

arguments (e.g. Mill); (iv) to weaken the conclusions of inductive arguments to probability judgements (e.g. Carnap); (v) to justify induction pragmatically or vindicate it (e.g. Reichenbach, Salmon); (vi) to dissolve the problem, i.e. to claim that it is a pseudo-one (e.g. Strawson, Edwards). The objections to (ii)–(v) are well known; they all in one way or other beg the point at issue which is:

(A) the problem of what warrant we have for supposing that the course of nature will not change.

(A) must of course be distinguished from:

(B) the problem of what warrant we have for believing some proposition, statement or theory true.

For to say that the special theory of relativity refuted Newtonian mechanics is not to say that the course of nature changed: it is to say that Newtonian mechanics was (at least in one respect) wrong all along. I will leave aside considerations pertaining to question (B) and the rationale for distinguishing it from question (A) until Chapter 4.

Popper claims to have solved the problem of induction, by accepting Hume's conclusion that induction cannot be justified but denying that science is inductive in nature. According to Popper science proceeds by the refutation of bold conjectures (general statements) by their deductive consequences (singular relatively observational statements). However, for a relatively observational statement to refute a general law-like statement (or theory) it must be presupposed that the course of nature will not change so that the experimental and observational context in which the refuting observation statement is true ceases to be true. Popper would reply to this objection as follows: '... there is a logical asymmetry: one singular statement – say about the perihelion of Mercury – can formally falsify Kepler's laws; but these cannot be formally verified by any number of singular statements. The attempt to minimize this asymmetry can only lead to confusion'.⁹⁰ But the decision to accept the singular statement about the perihelion of Mercury as falsifying Kepler's laws presupposes that in exactly the same circumstances Mercury's perihelion would behave in exactly the same way. The asymmetry is there alright. But what warrant is there in

⁹⁰ K. R. Popper, *op. cit.*, p. 41, n. 8.

popper's system for supposing that nature is uniform so that its course will not change, in the way Hume and Goodman invite us to imagine, so that our best-falsified theories (astrology, Marxism, psycho-analysis, Newtonian mechanics) become true? Whatever the merits of Popper's philosophy of science, his claim to have solved the logical problem of induction is manifestly untenable and based on a confusion of problems (A) and (B).

(vi) also fails, for a number of reasons. First, it seems possible to imagine worlds in which induction would be unsuccessful – not just 'counter-inductive' worlds, in which the unexpected always happens, but *capricious* worlds, for which no kind of rule could be formulated.⁹¹ Secondly, even a straightforwardly counter-inductive world would be incapable of sustaining scientific or social life. For the unexpected is a potentially infinite class. No inductive rule could be operationalized in a counter-inductive world. 'Expect the unexpected' can only be applied *ex post*, after the unexpected has actually happened. The fact that induction is (sometimes) successful places a constraint on the world in which we live. For in some conceivable worlds induction would be unsuccessful or unoperationisable. Hence it would seem reasonable, and indeed necessary, to isolate the conditions that must obtain for induction to be successful. If there are necessary conditions for the success of induction, they must constitute the missing 'justification'. Thirdly, it is clear that not all inductive arguments are equally good. To appeal to induction as an institution does not help us to decide between good and bad inductive arguments (any more than appeal to the law helps us to decide between good and bad laws). Finally, induction is not always justified. If it is not always justified there must be conditions under which it is justified, about which the approach represented by (vi) has nothing to say.

Induction, I have said, is not always justified. In general induction is only justified if we have some reason other than positive instances for the generalization concerned. Two kinds of reasons are distinguishable:–

(α) a plausible model or hypothesis of a mechanism by means of

⁹¹ M Hollis, 'Reason and Reality'. *P.A.S.* 1967–8, esp. pp. 282ff.

which we can render it intelligible to ourselves that when E_a then E_b .⁹²
(β) knowledge of the mechanism which given E_a generates E_b .

Induction is only justified if the generalization concerned is a law of nature.⁹³ Of course *ex ante* when inductive reasoning occurs we do not know *whether* the sequence is necessary. But it is justified *if* it is necessary. And to justify it we need only have grounds for supposing *that* it is necessary. Now I have already argued that (α), though representing an important moment in the process of scientific discovery, carries too little ontological bite to justify the assumption of a law of nature (see §2 above). The generative mechanisms of nature are of course nothing other than the powers or ways of acting of things.

Now *eduction*, or inference from particular instances to other particular instances, is only justified if *in addition* the system in which the consequent event occurs is closed. The crucial ambiguity in the formulation of the problem of induction to which I referred earlier now becomes clear. It turns on the question of whether the generalization referred to (or in eduction assumed) is an empirical or a normic statement, a statement about the conjunctions or events or the tendencies of things, a statement about actualities or possibilities. Now a belief in the uniformity of nature is quite misplaced if it is a belief in the invariance of patterns of *events* (or experiences). For the non-invariance of their patterns is, I have shown in Chapter 2, a condition of the possibility of science. A belief in the uniformity of nature is only rational if it is a belief in the invariance of *structures*. The eventsequential past is an unreliable guide to the future. Instead what we require, and in small measure actually possess, is a knowledge of the invariant tendencies and natures of things (though this does not legitimate predictions).

Induction is only justified if the generalization is a law of nature and eduction is only justified if the system is closed (so that the tendency designated in the law statement must, given the occurrence of the antecedent, be realized). Induction is

⁹² See e.g. G. Harman, 'Enumerative Induction as inference to the Best Explanation', *Journal of Philosophy* 65 (1968), pp. 529–33

⁹³ The stringency of this requirement may be relaxed in the kind of way indicated in 2.6 above when we move from scientific contexts to the rough-and-ready generalizations of everyday life.

justified because nature is stratified. Now we do not need to know *what* the structures are to know *that* nature is stratified. (We do not need to know what the explanation is to know that there is an explanation.) We know that nature is stratified because its stratification is a condition of the possibility of science-in-general. And we know that science is possible in general because it in fact occurs. To know that induction is justified we do not need to know what any particular explanation is.

Now if we know what the correct explanation is we do not need to reason inductively. And when we need to reason inductively we do not know what the correct explanation is. Given then that our knowledge that nature is stratified is secured a priori how can we justify any particular piece of inductive reasoning? By giving grounds for supposing that there is an explanation, located in the nature of things (at the next highest rung of reality), for the generalization claimed. It is the possibility of an adequate explanation, in terms of real invariances, from which the behaviour concerned can, normically understood, be deduced that must justify any piece of inductive reasoning in science. Thus it is the possibility of the satisfaction of a deductive criterion that justifies induction in science. But we know that the world must be such that this is so. It is the structure of the world that makes induction, when it is, a rational activity for men.

I have of course already argued that the conditions for inductive scepticism only obtain if we deny the possibility of (β) above. If we allow (β), i.e. a realist interpretation of the entities postulated in scientific theory, then we do have a reason independent of the facts identified at any one level of reality as to why one but not another sequence of events must be forthcoming (if the system is closed). Now as the argument for inductive scepticism turns on the alleged impossibility of knowledge of necessary connections between matters of fact and I have demonstrated how we can (and do) come to have such knowledge a posteriori in science, it remains only for me to examine the conditions of the plausibility of the traditional problem of induction and to show how the galaxy of problems in its wake can be rationally resolved.

The problem of induction arises if we restrict the grounds for a

generalization to its instances, i.e. if we accept Nicod's criterion of the evidence for a law. It is resolved in two steps: (α)* by allowing a model of a generative mechanism or structure to supply the missing reason that the coherence of scientific practice demands, and in particular to provide a crucial part of the grounds for a law; and (β)* by allowing that under certain conditions, i.e. if certain criteria are satisfied, such models held out in the scientific imagination as plausible representations of the real mechanisms of nature may come to be established as real. Mechanisms are enduring; they are nothing but the powers of things. Things, unlike events (which are changes in them), persist. Their persistence does not need explaining. Space and time are causally inert: they possess neither liabilities nor powers. Now step (β)* involves experimentation. It is a condition of the intelligibility of experimental activity that causal structures not only persist but act independently of the patterns of events. Thus the world is open, and the laws of nature must be analysed as the tendencies of things. The dynamic realist principles of substance and causality to which I have been working may thus be stated as follows: the world consists of enduring and transfactually active things (substance) which endure and act in their normal way unless acted upon (causality). Effects presuppose both continuants and causes. They must occur in things and be brought about by things (other than position in space or moment in time). On the other hand only effects need explaining.

The condition of the intelligibility of the problem of induction is an ontology of atomistic events and closed systems. For without closed systems there is no reason for the past to resemble the future and without atomistic events there is reason why it should. The grounds for the atomistic ontology of empirical realism disappear when we realise that sense-experience is neither the only ground nor the only source of knowledge; and that it is analysable neither in purely atomistic terms⁹⁴ nor as a happening to passive men.⁹⁵ The grounds for the actualist ontology of closed systems disappear when we realize that in general, outside astronomical contexts, they need to be experimentally established. In place of the ontology of experience

⁹⁴ See e.g. M. Vernon, *The Psychology of Perception*.

⁹⁵ See esp. J. J. Gibson, *The Senses Considered as Perceptual Systems*.

and atomistic events constantly conjoined, transcendental realism establishes an ontology of complex and active structures and things. In place of the contrast (and unbridgeable gulf) between our particular and our general knowledge of the world, transcendental realism allows knowledge both of things and of their powers or ways of acting. In place of the analysis of laws as constant conjunctions of events, transcendental realism analyses laws in terms of the tendencies of things which may be exercised unrealized and realized unperceived by men. Science becomes a social activity, difficult and discriminating; not an automatic, individualistic affair. Science is explanatory non-predictive.

I now want to show how the replacement of the empirical realist ontology of atomistic events and closed systems by the transcendental realist ontology of persisting and transfactually active things allows us to resolve the problems and paradoxes associated with the problem of induction. This problem, I have argued, only arises if we deny the possibility of a reason, located in the enduring nature of some thing, for the behaviour concerned. In its sharpest form it may be expressed as follows: if all predicates refer ultimately to experience and experiences are independent of each other, as they must be if they are to ground (in part or in whole) our knowledge of the world, then predicates must be independent of one another. There can then be no reason for expecting one rather than another set of experiences so that for all we know predicates may become associated in entirely new ways. Thus there is no reason why cabinet ministers should not suddenly start bearing figs or Mancunians disintegrate when exposed to the sun (the problem of induction); no reason why emeralds examined after A.D. 2000 should not turn out to be blue or blue things become green next Christmas (Goodman's paradox);⁹⁶ no reason why the sighting of a black raven should confirm the proposition that all ravens are black better than the sighting of a red herring or a white shoe (Hempel's paradox);⁹⁷ no reason to suppose that if I had gone for a walk in the rain five minutes ago I would in fact have got wet (the problem of subjunctive conditionals). Now there are two ways of meeting these absurdities. The first is to

⁹⁶ See N. Goodman, *Fact, Fiction and Forecast*, pp. 73–80.

⁹⁷ See e.g. C. G. Hempel, *op. cit.*, Chap. 1.

hold that the paradoxes and problems stem from the insertion of artificial predicates and fanciful conjectures into already functioning and well-connected scientific contexts, for which no positive reason can be given. The trouble with this line of response is that it is still vulnerable to the objection that there is no ground, independent of custom or convention or past practice or mob psychology, for expecting one sequence of events rather than another. And besides the nature of the 'connection' predicates are supposed to enjoy is unclear. The second is the transcendental realist line. This line holds that there are objective connections in the nature of things, which may be identified as enduring mechanisms, which bind or link some but not other events and states of affairs. I will now sketch the transcendental realist resolution of these problems.

It is physically impossible for a cabinet minister to bear figs; that is, nothing which bore figs could properly be said to be a cabinet minister at all. Desk lamps cannot fly or walk about the room, just as Mancunians do not disintegrate when exposed to the sun. A particular must tend to behave in certain ways if it is to be of the kind that it is. On an ontology of things the general problem of induction cannot arise, though there may be specific problems of identification and special reasons for expecting change. Things persist. They are natural endurers and their changes are explained in terms of unchanging things. What is the rationale for this resolution? The scientific explanation of scientifically significant behaviour is in terms of invariant principles of structure. Thus the scientist assumes that there is something about metals (their possession of free electrons, perhaps) in virtue of which it is not possible for them not to conduct electricity. Their possession of free electrons is the invariant principle of structure. There is something about cabinet ministers (their genetic constitution, perhaps) in virtue of which it is not possible for them to bear figs; just as, if Socrates is a man he must die.

On an ontology of things Goodman's paradox cannot arise. Now either 'all emeralds are green' is law-like or it is not. If it is not the Goodmanesque alternative 'all emeralds are grue' is equally admissible. For it is then *ex hypothesi* purely accidental that all emeralds happen to be green. On the other hand, to suppose that 'all emeralds are green' is law-like is to suppose

that there is a reason, located in its crystalline structure of chemical composition, why it differentially reflects light the way it does. Now given that structure, emeralds must, to normal observers under standard conditions, look green. So anything which looked blue could not possess that structure, and hence would not be an emerald at all. Now of course occasionally we may have grounds for supposing that a particular and even a kind will cease to exist, i.e. be transformed into a different thing or kind (or even into an entirely different kind of thing or kind?). Thus a genuine Goodman-type problem could arise. However It would be a specific problem, itself presupposing the existence of both a continuant and a cause. Moreover no predicate such as 'grue' could ever be admissible to science. Since the mere passage of time cannot constitute a cause. It would have to be a coincidence that emeralds examined after A.D. 2000 looked blue. Dates can be at best only proxy causes.

Hempel's paradox may be resolved quite simply once the significance of his intuition, viz. that propositions about shoes and herrings are irrelevant to the truth of propositions about ravens, is grasped. If laws are statements about things and there must be some reason other than instances for accepting them, then Hempel's paradox may be resolved as follows: If 'all ravens are black' is law-like there must be a reason, located in the nature of ravens (not in the nature of black), why ravens are black. 'All ravens are black' is a truth about ravens, not about colour. Hence the contrapositive 'all non-black-things are non-ravens' has no bearing on it. The logical subject of a law of nature is a (natural kind of) thing. Hence there is a logical asymmetry built into its structure, reflecting the site of the mechanism designated, in virtue of which its terms are not equivalent and contraposition is prohibited. To put this another way: the mechanism that, to use Strawson's term, 'collects'⁹⁸ red under herring or white under shoe is either entirely different from the mechanism that collects black under ravens or else, where as in the shoe case the 'connection' is entirely accidental, there is no mechanism involved at all.

The problem of subjunctive conditionals is easily and rationally resolved on an ontology of things. To assert a law of nature is to ascribe a possibility to a thing – a possibility which

⁹⁸ P. Strawson, *Individuals*, pp. 167ff and passim.

is possessed by the thing, and has a real basis in the enduring nature of the thing, whether it is exercised or not. To assert a subjunctive conditional is just to say that the possibility possessed by the thing would have been exercised, had the conditions in fact been different. I would have got wet alright, rain being what it is.

The source of these problems lies in the reduction of things to qualities and laws, which are statements about things, to conjunctions of events. This is reflected most sharply in the failure to sustain the idea of the necessity of law. But side by side with these well-known problems is a less well-known set (due to the tacit assumption, by almost all philosophers of science, of closed systems), which turn on the failure of the actualist ontology of empirical realism to sustain the idea of the universality of law. Lacking from the former set is a criterion for distinguishing necessary from accidental sequences (depending upon a concept of the stratification of the world) lacking from the latter set is a criterion for distinguishing open from closed systems (depending upon a concept of the differentiation of the world). For empirical realism all sequences are accidental and the world is closed; for transcendental realism some sequences are necessary and the world is open.

Let us briefly note these homologues of the well-known problems on the universality axis. Corresponding to the problem of induction we have the problem of what justifies the assumption that laws will continue to hold outside the laboratory. This is resolved by allowing (or rather seeing that it is a condition of the intelligibility of experimental science) that things endure and continue to act in their normal way outside as well as inside the laboratory (as they will do in the future as in the past) unless, as may sometimes happen, they are themselves transformed. Corresponding to the problem of subjunctive (and counterfactual) conditionals we have the problem of normic (and transfactual) ones. Corresponding to the paradoxes of confirmation, paradoxes of falsification. (Laws and theories are straightaway falsified by any open-systemic instance, just as they are straightaway confirmed by any contrapositive instance, if we regard laws as empirical statements.) Corresponding to the problem of justifying the use of hypothetical entities in theory construction (which Hempel has called 'the theoretician's

dilemma')⁹⁹ we have the problem of justifying the use of the CP clause in theory application (which we could call 'the engineer's dilemma', or the problem of the applied scientist's excuse). All these problems can be rationally resolved by an account of science which sees it as an attempt to penetrate ever deeper into the nature of things and to describe more adequately the things of nature.

The Humean analysis of laws is a failure: it does too little and too much. The causal contingent is neither contingent nor actual, but necessary and real.

The intelligibility of perception presupposes that objects persist, in space and time, independently of our perception of them. The intelligibility of experimental activity presupposes that they act, in space and time, independently of the patterns of events they generate. Now the use of general terms in identifying these objects presupposes that they fall into natural kinds. But it is not possible to say anything in general about the number of kinds there are or about the numbers in any particular kind. Now the things posited by science in its investigations may be quite recondite and abstract with respect to our ordinary experience. It is wrong to think of them as necessarily like material objects – they may be powers, forces, fields or just complex structures or sets of relationships. Their metaphysical character, which justifies us labelling them as 'things to mark their insusceptibility to analysis as 'events' or 'experiences', lies in their persistence and transfactual activity. This entails that they persist even when they do not act, and act in their normal way in the flux of conditions that co-determine the actual outcome of their activity. Things, as so conceived, must be complex and structured; in virtue of which possibilities may be ascribed to them which may be unexercised or exercised unactualized or actualized unperceived by men. On this account of science the actual is seen as an instance of the possible; and a normic mood is added to the hierarchy of conditionals marking the space of possibilities exercised but unactualized.

On the account of laws advanced here they cannot be identified with constant conjunctions of atomistic events or regarded as reporting correlations between either independent or equivalent variables. On the contrary, they must always be grounded in

⁹⁹ C. G. Hempel, *op. cit.*, Chap. 8.

some conception of an explanatory mechanism and ascribed, as tendencies, to specific kinds of things. This is consistent with the view of ordinary things as subject to dual (and multiple) control, perhaps by principles of relatively different kinds. Laws do not describe the patterns of events. Rather, we could say, they describe the normic behaviour of novel kinds and impose constraints on familiar things. Ordinary things may be conceived, metaphysically, as *compounds*. This allows us to make sense of the *individuality* of historical particulars; just as the conception of ordinary events as 'conjunctures' (see 2.6 above) allows us to make sense of the uniqueness of historical events.

If the ordinary things of the world are compounds then it is natural that they should share nothing in common except resemblances. But just as only some events are significant in science, although all in principle may be explained by it, so with things. Ordinary things have a genesis and their changes may be rationally explained (in terms of continuants and causes) by reference to the exercise of the tendencies of things which share a common identity, i.e. which fall into a natural kind. Scientifically significant generality does not lie on the face of the world, but in the hidden essences of things.

How can this be shown? Either classification is arbitrary or it is not. If it is non-arbitrary it must be based on a relationship of resemblance (similarity) or identity. If it is only based on the assumption of a relationship of resemblance there is no rationale for the stratification of science. On the other hand if it is based on an assumed theoretical identity then we do have a rationale for the move from manifest behaviour to essential nature that we have seen lies at the heart of rational theory-construction in science. To stress, nothing can be said about the number or variety of real universals there are. But it is clear that 'table' and 'red' are not real universals; and 'gene' and 'molecule' are.

A similar trilemma may be applied to our explanatory knowledge of the world. Either explanation is arbitrary or it is not (arbitrariness is suggested by the problem of induction or any of the paradoxes discussed above). If it is non-arbitrary the ground for the explanation is either imposed by men or it exists in the world. If it is imposed by men we are left without any rationale for experimental activity, the process of testing human constructions against the world. Predicates are not

independent of each other and classifications are not arbitrary in science because there are necessary connections in the world and things fall into natural kinds.

I can now return to the question I asked at the beginning of this chapter. To what, in our ascription of laws, is necessity and universality properly ascribed? The answer is to the transfactual activity of things, i.e. to enduring mechanisms at work. For these ascriptions to be possible the world must be composed of enduring mechanisms which act independently of men; science must be an ongoing social activity; and men must be (in the sense indicated in 2.5 above) free.

Now it is because we are material things, possessed of the senses of sight and touch, that we accord priority in verifying existential claims to changes in material things. But scientists posit for these changes both continuants and causes, some of which are necessarily unperceivable. It is true that ‘that a flash or a bang occurs does not *entail* that anything flashes or bangs. “Let there be light” does not *mean* “let something shine”’.¹⁰⁰ But a scientist can never rest content with effects: he must search for causes; and causes reside in or constitute things. Charged clouds, magnetic fields and radio stars can only be detected through their effects. But this does not lead us to deny their existence, any more than we can rationally doubt the existence of society or of language as a structure irreducible to its effects. There could be a world of electrons without material objects; and there could be a world of material objects without men. It is contingent that we exist (and so know this). But given that we do, no other position is rationally defensible. It is the nature of the world that determines which aspects of reality can be possible objects of knowledge for us.¹⁰¹ But it is the historical development of the various sciences that determines in what manner and to what degree these possibilities are taken up by men.

¹⁰⁰ Cf. P. Strawson, *op. cit.*, p. 46 (my emphasis).

¹⁰¹ Cf: ‘In the Newtonian world and in Newtonian science . . . the conditions of knowledge do not determine the conditions of being; quite the contrary, it is the structure of reality that determines which of our facilities of knowledge can possibly (or cannot) make it assessable to us. Or, to use an old Platonic formula: in the Newtonian world and in Newtonian science, it is not man but God who is the measure of things’, A. Koyré, ‘The Influence of Philosophical Trends on the Formulation of Scientific Theories’, *The Validation of Scientific Theories*, ed. P. G. Frank, p. 199.

Appendix

NATURAL TENDENCIES AND CAUSAL POWERS

I have argued that there is a pair of ontological distinctions, presupposed by the intelligibility of experimental activity and scientific training, at any one stratum or level of reality, between structures and the events they generate (and things and the changes that occur in them) and the experiences men have of them. These distinctions may be conveniently expressed by the formula $D_r \geq D_a \geq D_e$,¹ where the special case $D_r = D_a = D_e$, assumed to be spontaneously satisfied by empirical realism, has in fact to be worked for in the social activity of science. The possibility of $D_r \neq D_a$ implies that not all events, and that of $D_a \neq D_e$ that not all experiences, are equally significant epistemically (see 1.6 above). Now the postulation of this novel non-Humean ontology of structures and generative mechanisms raises the question of how they ought to be analysed. Possibility $D_a \neq D_e$ implies that they cannot be reduced to qualities; $D_r \neq D_a$, that they cannot be analysed as dispositions. I have suggested that structures and generative mechanisms must be analysed as the tendencies and powers of enduring and transfactually acting things. In this appendix I want to clarify these concepts, and to defend the transcendental realist ontology from some possible misconceptions.

A 'tendency' as I have been using the term up till now is just a power which may be exercised unrealized, a power normically qualified. This qualification is necessitated of course by the fact of open systems. I will call this concept of tendency the primary concept. But there is another concept, in principle distinct from this, in which it functions so as to pinpoint the enduring orientations, rather than the possibility of the transfactual activity, of

¹ D_r = domain of the real; D_a = domain of the actual; and D_e = domain of the empirical in the sense specified in 1.6 above.

things. On this concept a tendency is something more than a power. It depends upon distinguishing from within the class of actions naturally possible for a thing (which constitute the totality of its powers), in virtue of its being the kind of thing that it is, those which are typical, usual or characteristic of that thing as distinct from others of its kind. It is the function of this second concept to individuate natures within kinds, species within genera, individuals within classes, etc. It is made possible by the fact that some complex structured objects reveal, in virtue of their pre-formed structure, what I am going to metaphorically characterize as an 'ontological preference' for some but not other of the natural possibilities open to them. A thing possesses powers in virtue of its falling into a natural kind, tendencies in virtue of its being one of a type within that kind. All men (living in certain kinds of societies) possess the power to steal; kleptomaniacs possess the tendency to do so.

The distinctiveness of the two concepts of tendency is clear. To say Tania pushed the door open completely explains why the door is open and implies that she can do it, i.e. has the power to do it. But to say that she *tends* to push the door open is to say something more; which cannot be analysed as when she exercises her power to push the door open, it tends to open (which is just to normically qualify the exercise of her power). There is a real difference in the kind of behaviour, and the state of the thing whose nature is referred to, in the two cases. Men, but not dolphins, can (i.e. possess the power to) smoke; but some men are non-smokers. To attribute a tendency (in the second sense) is not just to normically qualify the exercise of a power; but to say that some of the intrinsic enabling conditions of a relatively enduring kind for the power's exercise are satisfied; that the thing is predisposed or oriented towards doing it, that it is in something of a state or condition to do it.

In distinguishing in this way between tendencies and powers we are able to avoid the dilemma of supposing either that all behaviour is law-like or that some events are uncaused. For a power may be exercised and completely explain an event when the behaviour is not law-like (typical) and so cannot be seen as the exercise of a tendency in the second sense (which I will henceforth denote as tendency₂). Particular circumstances may account for the exercise of a power; whereas they must be,

invoked to account for the non-realization of a tendency₂, if the conditions for its realization are satisfied.

To say that a thing has the power to do \emptyset is to say that it will do \emptyset , in the appropriate circumstances, in virtue of its nature (e.g. its intrinsic structure or genetic constitution). I have suggested in §3 above that in indicating the existence of a reason, at the next highest level of reality, for the manifest behaviour a power statement constitutes a schematic explanation which is filled out in the growth of science. To ascribe a power is to make a statement about possibilities which may not be actualized and which are possessed by the thing whether or not they are known by men; so powers cannot be reduced to their exercise or our ignorance.

Now if powers are possessed by things which act in open systems their exercise must be normically qualified; and they must be seen as tendencies₁. This is the concept of tendency I have been using so far. In order to apply any tendency₁ or normic statement we must know when the antecedent or stimulus conditions for the mechanism it designates are satisfied. But this does not warrant the prediction of the tendency's₁ fulfilment, i.e. the consequent's realization; which depends upon the system being closed, and in particular upon the non-intervention of countervailing causes.

If things are complex and pre-formed they may be in a relatively enduring state or condition to exercise some but not other of their powers. This is the concept of tendency₂ – the concept of tendency normally employed in ordinary life. When such things act in open systems the exercise of tendencies₂ will of course also have to be normically qualified. To say that a thing, X, has a tendency₂ to do \emptyset is thus to say:–

- (i) X has the power (or liability) to do (or suffer) \emptyset ;
- (ii) X is in an enduring condition to do \emptyset , i.e. it is predisposed oriented towards doing \emptyset ;
- (iii) X will do \emptyset , given an appropriate set of circumstances, in virtue of its predisposition, in the absence of intervening (or countervailing) causes.

That is, it is to say that X can do it, that most (or the most important) of the intrinsic enabling conditions for it are satisfied

and that when the other conditions are satisfied it will do it unless it is prevented.

Notice that a tendency₂ statement presupposes a power statement but the converse is not the case. For a thing must not only possess the power but be in something of a condition to exercise it. Now it might be thought, in view of this, that though both tendencies₂ and powers designated possibilities, tendencies₂ designated less remote ones, possibilities closer to or in the process of realization. But this metaphor must be interpreted very carefully. For the greater 'proximity' of the tendency's₂ realization is still a purely non-epistemic one. For I may be absolutely certain that the stimulus or other conditions will be unsatisfied or that countervailing causes will intervene so that the tendency₂ will be unexercised or if exercised unrealized respectively. Knowledge of a tendency₂ is quite distinct from judgement about the tendency's₂ realization.

It is a mistake to think of offsetting causes as relatively short-run retarding barriers which are sooner or later overcome by the superior staying power of the tendency. This is to tacitly conflate epistemic and natural possibilities. Equilibria may last for ever. A related mistake is to identify the 'lawfulness' implicit in a tendency₂ statement with the regularity or repeatability of a syndrome of behaviour. For some tendencies₂ (to explode, to commit suicide) have of necessity only a single manifestation; and cannot be fulfilled twice.

A tendency₂ may be revealed in action, even when it is unexercised; in which case our grounds for attributing it must be indirect. If it is not shown at all, i.e. has no overt expression, then our grounds for attributing it must be based entirely on our theoretical knowledge of the thing. In general, the attribution of tendencies₂ requires more about things to be known than is the case in the attribution of powers. For we must know that the enabling conditions for the power's exercise are satisfied and hence what they are.

Tendency₂ ascriptions may be generic. There may be no unique set of conditions under which a tendency₂ is exercised; and the way in which it is realised, if it is, may depend upon the particular circumstances in which its exercise occurs. A kleptomaniac's tendency is not normally fixed on a particular shop and it is not normally knowingly exercised under the eye of the

law. (If it is, he ceases to be a kleptomaniac and becomes something else.) Similarly the way in which Fido expresses his generic tendency₂ to bark may depend upon the particular factor that excites it. He may bark viciously at an intruder but conventionally at the postman, fearfully at the moon but affectionately at another dog, arrogantly at a cat and playfully at an old shoe.

Both (ii) and (iii) play an important role in indicating a characteristic explanatory problem and the appropriate direction of research. Thus the problem indicated in (iii) is not: what accounts for the fulfilment of tendencies, given that the conditions for their fulfilment are realized? But rather, what accounts for their nonfulfilment in these circumstances? (The former needs no special explanation.) And it tells us what to look for: namely, the presence of interfering causes. It should be noted that a formulation which only included reference to the circumstances would, if the tendency₂ were unrealized, set us off in the wrong direction, on a search for some exceptionless generalization which covered that case too; so providing an empty and ever-expanding redescription rather than an explanation. Generality in nature lies in things not conditions. Explanation in terms of the powers/tendencies conceptual network avoids the circularity and triviality of 'explanations' conforming to the Popper-Hempel model, where an event cannot be explained other than by or without subsumption under generalizations. The former is always insufficient and the latter mostly impossible. So room must be found for the concept of a generative mechanism, activated under closed conditions, explaining regularities when they occur; and the concept of an agent bringing about and so explaining an event in a given situation without this being interpreted as involving a claim to a regularity.

Offsetting causes are often assumed to be always extrinsic. But the cause of a failure of a car to move when the gear is in neutral is not something distinct from and extraneous to the mechanism responsible for its normal motion. Science is never concerned with just listing causes but seeks always to relate them, even when seemingly opposed or contradictory, to common structures. Now intrinsic offsetting causes may or may not directly interfere with the operation of the mechanism

responsible for the satisfaction of the intrinsic enabling conditions. If they do then we must say that the tendency₂ is no longer possessed, as condition (ii) ceases to be satisfied. But not all intrinsic offsetting causes are like that. Thus a person may possess a reason for acting in a certain way and not act in that way under appropriate circumstances if, at the time, he possesses in addition a set of overriding or more compelling reasons for not acting in that way. The idea of a perfect balance of opposed causes is expressed in the theoretical concept of an equilibrium.

It is the specific role of (ii), I suggest, to indicate the existence of a level of activity within the thing which is ensuring or has ensured the satisfaction of the intrinsic enabling conditions for \emptyset . That is, it directs our attention to the mechanisms responsible for those relatively enduring states of a thing that distinguish the possession of a tendency₂ from that of a mere power. The tendency₂ statement is action-oriented by contrast with the simple power statement in two respects then: firstly, in indicating the past or present activity of generative mechanisms responsible for the persisting orientations of things; and secondly, in indicating the possibility of their transfactual activity, i.e. the exercise of these tendencies irrespective of their fulfilment. A simple power statement is, on the other hand, consistent with completely quiescent or dormant things or things which have a level of activity sufficient only for the retention of that power.

Now in an important sense it is not whether the tendency, is realized or not that constitutes the scientifically interesting thing about it (for this will always be to a greater or lesser extent circumstantially determined) but the reason for the relatively enduring orientation that differentiates the possession of a tendency₂ from that of a power. Thus in the case of the kleptomaniac, the scientist will be concerned with the reasons for his tendency₂, whether he suppresses it or not. Of course this need not be the chief concern of social policy. But this merely underscores the latter's distinctiveness from science. For science is concerned with the behaviour of things only in as much as it casts light on their reasons for acting and hence upon what they are.

The underlying action indicated in the tendency₂ statement

may be continuous or sporadic; and the onset of a tendency₂ may be gradual or sudden. Moreover some of the mechanisms responsible for the possession of tendencies₂, e.g. those posited in Freudian theory, may have ceased to operate long before the time of their manifestation. The grounds for our attribution of the tendency must then of course lie entirely in our evidence for the pre-formed intrinsic structure rather than in our evidence for the present higher-order activity of the thing. Such tendencies, are latent. It should be stressed that we cannot assume that the underlying action occurs in systems which are themselves anything other than open; just as we cannot assume that underlying the action of those mechanisms are anything other than more basic ones.

Some tendencies₂ are powers which are held in abeyance; and are straightaway exercised when the impediments to their exercise are removed. Other tendencies₂ are powers which require for their exercise the active stimulus of other things. One might in the former case talk of 'releasing' rather than 'stimulus' conditions. Releasing or stimulus conditions may be intrinsic or extrinsic to the thing possessing the tendency₂. Spontaneity, self-determination and acting for a reason all fall under the former paradigm. It is better to distinguish between powers and liabilities than between active and passive things. But within the category of extrinsic causes we must remember to include pulls as well as pushes, structural relationships as well as the momenta of other things.

Both components (ii) and (iii) of the analysis of tendencies₂ encourage a further investigation of powers: viz. into those responsible for the persisting orientations of things and into those whose exercise is responsible for the non-realization of known tendencies₂, when the conditions for their realization are satisfied. To sum up then a tendency₂ statement says there is a level of activity, perhaps unknown, intrinsic to the thing, such that it is predisposed to perform an action of a certain type. Its chief function is to indicate the existence of a level of activity within the thing such that it is oriented towards some rather than other of the natural possibilities open to it. In this way it leads us to a more precise specification of the natures of particular things (or groups) within kinds.

I now want to turn to some possible misconceptions arising

out of my use of the primary concept of tendency (which I shall not denote by a subscript) and the concept of power. The intelligibility of experimental activity and the possibility of the practical application of our knowledge depends, I have argued in Chapter 2, upon the assumption of the ontological independence of causal laws (which I have analysed as the tendencies of things) from the patterns of events. That science can job back and forth from open to closed systems depends entirely upon the consideration that the natures of things and the generative mechanisms of nature persist and act in open and closed systems alike. That is to say, the intelligibility of scientific practice requires that we view them theoretically as at any moment of time unaffected by the closure or otherwise of the systems in which they occur. Clearly this does not mean that natures and generative mechanisms must be viewed as eternal or as completely unaffected by the actions of things or their operations. Powers may wane and disappear. Mechanisms may be transformed. Such transformations must themselves be analysed as events in open systems; and are in principle explicable in terms of a network of normic statements. The changing is explained in terms of the unchanging, but the unchanging need only be relatively so. Some fields may be irreducibly historical. But the very move, which I have argued is essential to our understanding of science, from talking of the occurrence of events to the things and structures that account for them presupposes that they are relatively persistent. A non-enduring thing would be a mere event, a totally affected mechanism incapable of production.

Now it is vital to distinguish the initial conditions whose instantiation warrants the application of a normic or tendency statement, i.e. of a causal law, from the complete atomistic state-description that the empirical realist needs to sustain his analysis of laws. For on the transcendental realist view a law is applicable if the mechanism it designates is set in motion, i.e. if the conditions explicitly mentioned in the protasis of the law-like statement are satisfied, whether or not the system is closed, i.e. whether or not what are sometimes referred to as the 'boundary conditions' are satisfied, and hence whether or not the consequent is realized. The 'boundary conditions' for the transcendental realist are conditions for the experimental testing, not the applicability of a law. To say that laws are

applicable if and only if their 'boundary' as well as their initial conditions are satisfied is useless because it is men that ensure the satisfaction of the boundary conditions. And this activity is only intelligible if laws continue to apply in open systems, where the so-called 'boundary conditions' are not satisfied. We isolate systems to understand mechanisms; but the mechanisms endure and act quite independently of our activity.

The ordinary things or standard particulars of the world are metaphysically compounds, just as ordinary events are conjunctures. The philosophy of science has noted, quite correctly, that the objects of scientific thought are 'ideal' or 'abstract' with respect to such things and events. But the transcendental realist sees such objects as real. For him the world is composed of real things and generated by real mechanisms. It is the world itself, not our thought of it, that is abstract and ideal. The world consists of chemical elements, electrons, quarks; its phenomena are generated by actions, reactions, forces, fields. The received tradition in the philosophy of science has seen such things as inventions of men designed to gloss, and account for deviations in, our familiar experiences. But transcendental realism, starting from the premise of the contingency of our own experience, sees nature as real; and science as our persistent effort to understand it.

In §3 I argued that the concept of powers, in combining a specific attribution of behaviour with an unspecific attribution of structure, plays a key strata-bridging role. For in hinting at the perhaps to be discovered reason [at Stratum II] for behaviour [at Stratum I] it represents an important moment in the process of scientific discovery. Its structure reflects this, in incorporating both a characterization at the science's point of departure and a reference to its intended destination. To say that X has the power to do \emptyset is to say that it will do \emptyset in the appropriate circumstances in virtue of its nature (e.g. structure or constitution);² that is to say it will do it in virtue of its being the kind of thing that it is.

Now to ascribe a power is to suppose that there is a real

² Cf. R. Harré, 'Powers', *P.J.P.S. Vol. 21* (1970), p. 85 and *Principles of Scientific Thinking*, p. 270. See also D. Armstrong, 'Dispositional Properties', *Analysis* 22 (1961-2), pp. 44-46 and Belief, *Truth and Knowledge*, esp. pp. 11-16.

basis for the possession of that power independent of whether the power is exercised or not.³ But it is not possible to say whether that real basis can be qualitatively described or not. Some real bases, such as the molecular structure accounting for the fragility of glass, can be qualitatively described; others, such as the structure of a gravitational or magnetic field, can not. Hence it is contingent whether we can know about a thing anything other than its causal powers; and it is contingent, when we can not, whether a thing is anything other than its causal powers. Now Mackie has objected to the concept of powers on the ground that if it is to form the basis for a new metaphysics of science it must violate 'Hume's principle that there can be no logical connections between distinct existences'.⁴ In support of this he asks 'what would be the point of showing that opium contains morphine which (despite its name) is only contingently related to sleep, if we knew already that opium contained an intrinsic power whose presence entailed the production of sleep?'⁵ Now I have of course already argued that if science is to be possible there must be necessary connections between some but not other events; and that such necessary connections are provided by the enduring mechanisms that bind some but not other events together and that exist as the powers of things. Now the knowledge of the chemical nature of opium which allows us to deduce its tendency to produce soporific effects in men is not a priori; but discovered a posteriori, in the ongoing process of science. Once we know a thing's nature then we can deduce its tendencies, how it will behave CP. But typically in science when we ascribe powers we do not know a thing's nature but only believe that it is in virtue of the thing's nature, whatever it is, that it does what it does (i.e. that its behaviour is necessary, not accidental). A power-statement then acts as an imperative for scientists to find, and as a temporary place-holder for, that explanation which, by capturing the essence of the thing, will allow the most stringent possible criterion for our knowledge of natural necessity to be satisfied.

When we know what a thing is we know what it will tend to do, if appropriate circumstances materialize.

³ Cf. D. Armstrong, *A Materialist Theory of Mind*, p. 86.

⁴ J. L. Mackie, *Truth, Probability and Paradox*, p. 137.

⁵ *Ibid.*, pp. 143-4.

4. Metaphysics and the Philosophy of Science

If science is to be possible men must possess certain essential powers. Among these is the power of affecting the sequences of states and events in the world in the sense of bringing about effects which but for their action would not have been realized. In this way men contribute to the universal maelstrom of existence. More specific to men is their power to initiate and prevent change in a purposeful way. The possession of this power seems to stem from the fact that men are material things with a particular degree of neuro-physiological complexity which enables them to monitor and control their own actions. Foremost among the powers necessary for science and, as far as we know, distinctive of men is their power of intentional action, which enables them to act self-consciously on the world: that is not just to monitor and control their performance, but to monitor the monitoring of their performance: to plan, to act and so to make an anticipatory commentary come true.

Among the powers falling within this genus perhaps the most basic and certainly the most studied is the power to acquire and use language. The latter implies at the very least that we are material things with a point of view in space and an existence in time, so that we must be able to communicate with each other from different spatio-temporal locations. And the former implies at the very least that we must be able to communicate with each other on the basis of the possession of differing cognitive equipment, so that reindividuation and recharacterization must be possible and the definitions of terms must be at least partially open. However language is by no means the only vehicle of thought; nor is language-using our only intellectual skill. Pictures, diagrams and iconic models play, it has been argued, an indispensable role in scientific thought.

Now endowed with our ensemble of intellectual powers we

are able not just to describe the patterns of events but by imagining structures (which may come to be established as real) to grasp the mechanisms of their production.

Closely related to these powers is our capacity to design, manufacture and use tools. This *inter alia* enables us to act at a distance; and in the course of science it has enormously extended our powers of perception and detection. More generally, it has enormously increased our powers of intervention in and control over, as we say, 'the course of nature'.

In all these activities – involving perceiving, manipulating, discursive and pictorial thought – the role of the nature of men as mechanisms capable of second-order monitoring and feedback is vital. In virtue of it our diverse perceptual, manipulative and cognitive performances become possible objects of self-criticism; and *inter alia* through our capacity to make public our monitored commentaries upon these monitored performances open to the criticism of others. Simple rudimentary perception, such as sniffing, and basic actions, such as raising one's arm, which depend in general only upon a first-order monitoring, are in themselves relatively insignificant in science. The chemist who sneezes while conducting an experiment is aware of himself in a certain way, but not in the way that the physiologist understands all sneezes, including his own. For the latter knows the mechanism of sneezing, while the former merely experiences its effects. (The chemist, unlike the layman, may however be aware of the cause, in the sense of antecedent or stimulus conditions for the mechanism, in this case).

Scientific perception is non-simple then in the sense that it must normally be theoretically-informed. And most scientific actions are non-basic (in Danto's sense)¹ in that they consist in doing things to bring other things about (i.e. ϕ ing to ψ),² that is initiating sequences of events whose outcome is both distant and planned. It is through our acquired skills of perception that we come to be in a position to formulate propositions concerning the behaviour of things, to identify and describe the flux of events. But it is through our manipulative powers that, by interfering with the course of nature (this flow of events), we

¹ See A. C. Danto, 'Basic Actions', *A.P.Q. Vol. 2* (1965), reprinted in *The Philosophy of Action*, ed. A. White, pp. 43-58.

² Cf. G. H. von Wright, *op. cit.*, pp. 66ff.

are able to check the reality and study the operation of the hypothetical generative mechanisms that in the scientific imagination we picture as responsible for their behaviour.

In such a move we disrupt the sequences of events to identify the underlying causal laws. Its significance – the significance of experimentation – lies in the consideration that it gives us access to causal structures that exist and act independently of the experiment. In the same way the significance of perception lies in the consideration that it gives us access to things that exist independently of it. And in a sense scientifically-relevant perception depends upon an anterior disruption too – of the sequence of common-sense experiences in a scientific education or training.

Now the crux of my objection to the doctrine of empirical realism should be clear. It is that it cannot sustain the intelligibility of perception and experimental activity; and that in positing a correspondence (or even in its positivist form an identity) between:

- (a) sense-experiences, which are in general only made significant by the transformation of antecedent common sense, and their objects, viz. events and the states of things, as expressed in the concept of the empirical world; and
- (b) constant conjunctions, which are in general only made possible by human activity, and causal laws, as expressed in the idea of the actuality of causal laws

it makes impossible both scientific change (at least in our descriptions of possible objects of experience) and the scientific explanation of things existing and acting in open systems (that is, in systems where invariant conjunctions of events have not been found or made to prevail). In this way empirical realism comes to seriously understate the critical significance and scope of application of science.

(a) depends upon the possibility of identifying or characterizing the same thing or phenomenon (or explaining the same event) in a different way, as well as from different spatio-temporal locations. (b) depends upon the intelligibility of the idea of causal structures existing and acting where no empirical regularities prevail. Both depend upon the non-identity and possible disjuncture of the terms of the pairs; that is, upon the

possibility of one varying without the other. Thus we must have a concept of facts as social products and what is described and/or explained as independent of men. And we must have a concept of the mechanisms that generate phenomena irreducible to the phenomena they generate. And so two dimensions must be established in the philosophy of science: a transitive dimension, in which experiences and conjunctions of events are seen as socially produced; and an intransitive dimension, in which the objects of scientific thought are seen as generative mechanisms and structures which exist and act independently of men.

Now empirical realism is characterized by the absence of an intransitive dimension and of a transitive dimension with respect to experience. But I have already argued that this merely results in the generation of an implicit ontology, based on the category of experience and an implicit sociology, based on the category of men: that is, in the generation of an ontological atomism and an epistemological individualism (at least with respect to experience). I have further suggested in 3.4 above that underpinning empirical realism is a particular conception of men (at least with respect to experience): in which men are seen as passive sensors of given facts and recorders of their given conjunctions. This model of man constitutes, together with the celestial closure and the classical paradigm of action, the ‘analogical grammar’³ of empirical realism, the scientific substance that lends plausibility to its metaphysics, that gives credence to its philosophical form.

If there is a basic or fundamental level of knowledge, at which what is expressed is certain and given independently of any human activity and reflects (and ultimately constitutes) the world, then its constituents must be atomistic. For if they were not atomistic they would be themselves susceptible of analysis, and so require justification. Hence it is the characteristics of the concept of knowledge to which empirical realists have subscribed, that accounts for the disastrous ontology that we have examined. It is the desire for incorrigible foundations in a level which constitutes the world that generates the ontology of atomistic events and closed systems responsible for the problems and errors analysed in Chapters 2 and 3. Against this I

³ To borrow Buchdahl’s useful concept. See G. Buchdahl, *op. cit.*, p. 3 and *passim*.

have argued that knowledge depends upon knowledge-like antecedents and that the world is independent of men. But if it is the requirements of an incorrigible ground for knowledge of the world in empirical realism that generates the implicit ontology of empirical realism, it is the model of man necessary to sustain the incorrigibility of this ground that forms the lynchpin of the tradition; and hence explains in the last instance the doctrines and problems we have been criticizing and examining. This is of course as one would expect given the epistemological bias and anthropocentricity of our philosophical thought.

Empirical realism is faced with the following trilemma: either (i) our knowledge is determined (in part or in whole) by experience or (ii) it is fixed a priori as a necessary condition of experience or (iii) it is free (in the sense of unconstrained by experience).⁴ I said at the outset that part of my intention was a philosophy for science. What, then, are the implications of these three positions for scientific practice? It is clear that if knowledge is regarded as justified in terms of given experience we have the makings of what is in effect a conservative ideology, in which the current experiences of a science are rationalized in being thought of as natural, given or implied by the very nature of things themselves. If (ii) is seriously considered it would tend to have the same conservative effect. It is less obvious that (iii) would too – until we remember that science is an institutionalized activity in which if to be free is to be unconstrained by the possibility of critical experience a self-perpetuating dogma may ensue. Examples could be drawn from the history of physics and chemistry and economics to show how empirical realism has functioned in this way as a conservative ideology for science.

A philosophical system may serve to rationalize the practice of a science in another way, viz. through its own substantive scientific analogies and the correspondence or resonance they find in the science. Thus empirical realism readily finds an echo in mechanistic explanations in physics and psychology and reductionist programmes in sociology and biology. One would

⁴ Radical conventionalists adopt a mixture of the first and third horns in the sense of allowing that we are free to decide what constitutes experience.

expect such a link to be particularly strong in the case of the social sciences, if only because of the greater sensitivity on the part of social scientists to the philosophy of science - a sensitivity partly explained by the apprehension of their own underdevelopment and partly by the consideration that science in general and social science in particular is also part of their own field of inquiry. The resonance between Skinner and Hume or Marshall and Mill is too clear to need further remarking.

In addition to the effect of its concept of knowledge and its own substantive scientific analogies a philosophy of science may of course also, and most obviously, rationalize the practice of a science through its own conception of the methods of science and the appropriate objects of scientific inquiry. If I am right in my argument the methodology of empirical realism is not that of science but if it were to be acted upon it would have the most deleterious effects on its practice.

Now if empirical realism depends heavily for its plausibility upon its analogical grammar, it could be asked upon what substantive scientific analogy does transcendental realism depend? The answer is, I think, none. All philosophical argument that is not explicitly transcendental depends, I think, upon more or less strained scientific analogies or tacitly presupposed substantive theories. But a transcendental argument whose premise is explicitly stated need not depend upon any particular theory, other than those bound up in the activity that is its object and which is its task to explicate.

The central argument of this study, establishing an ontological distinction between causal laws and patterns of events (the independence of the domains of the real and the actual, the irreducibility of structures to events), has turned on the possibility of experimental activity. Now as it is clear that experimental activity is impossible in the social sciences and at the very least devoid of the same significance in psychology as it possesses in physics and chemistry, I want to round off my argument by considering whether something analogous to the controlled investigation of nature, making possible the experimental confirmation and falsification of theories, might be possible in the social sciences and psychology and other fields where experimental activity is impossible or more or less seriously circumscribed.

Three points are clear. First, that there is a general problem of confirmation (or corroboration) and falsification in the non-experimental sciences. Second, that though we can assume that there are explanations for social and psychological phenomena (and under social and psychological descriptions), we cannot assume that the social or psychological sciences have got anywhere near them. Thirdly, that any adequate solution to the methodological problems (and in particular problems of confirmation and falsification) of the non-experimental sciences must depend upon a more adequate conception of natural science than that which has so far informed discussion of them.

An awareness of the general problem of confirmation and falsification in the non-experimental sciences is shown by the following quotation:

The linguist . . . is studying one fundamental factor that is involved in performance, but not the only one. This idealization must be kept in mind when considering the problem of confirmation of grammars on the basis of empirical evidence. There is no reason why one should not also study the interaction of several factors that are involved in complex mental acts and that underlie actual performance, but such a study is not likely to proceed vary far unless the separate factors are themselves reasonably well understood.⁵

Now if I am right in arguing that the significance of experimental activity in natural science is that it gives us access to enduring and transfactually active structures and that it is only under closed conditions that confirmation and falsification of theory is possible, then we are in a better position to see that the central problem of the psychological and social (and other non-experimental) sciences is that of devising (or reconstructing) an analogous procedure of inquiry and selectively empirical confirmation (and falsification) and to appreciate the great gulf that must separate them, in the absence of such a procedure, from the sciences of nature.

In the case of psychology the agent's capacity to give a commentary on intentional action might provide an experiment-analogue, though one cannot rule out the possibility that new

⁵ N. Chomsky, 'Problems of Explanation in Linguistics', *Explanation in the Behavioural Sciences*, eds. R. Borger and F. Cioffi, pp. 427–8. I have of course already argued against the use of the concept of 'idealisation' to refer to generative structures in 2.4 above.

concepts will have to be given by the investigator to the agent (even e.g. in the case of the identification of emotions). Much social science can be seen to depend upon attempted real definition of forms of social life which have already been identified under certain descriptions and are known by the agents who participate in the social activities concerned under those or other descriptions. Social structures, unlike natural structures, cannot exist independently of their effects. Thus real definitions of concepts such as capitalism, democracy, power, love can only be justified by their capacity to render intelligible a certain domain of phenomena. I suggest that they are falsified by their incapacity to explain in a non-ad-hoc way a range of phenomena that takes on a special significance for the agents that participate in the forms of social life they define. Thus it was the mass unemployment of the 1930's that demonstrated the inadequacy of the neo-classical system and provided the motor for the Keynesian innovation which showed how an unemployment equilibrium was possible. Clearly I have no space to defend or elaborate these suggestions here. It is sufficient for our purposes merely to note the problem: what are the enduring and transfactually active 'mechanisms' of the sciences of society and man? Transcendental realism conceives the various sciences as unified in their method but specific to (or differentiated with respect to) their particular objects.

Now once generative mechanisms are seen to be the objects of scientific thought, it can be seen that four questions can be asked of any generative mechanism G:-

- (i) is the mechanism enduring?
- (ii) is the mechanism operating?
- (iii) are the results of the activity of the mechanism unaffected by the operations of others (of either the same or different types)?
- (iv) are the results of the activity of the mechanism perceived or otherwise detected by men?

Now if one assumes, as the actualist does, that laws are empirical invariances (universal empirical generalizations) one cannot sensibly ask these questions. Two consequences should be noted. First, a neglect of the conscious human activity that is necessary for the generation of event-invariances under signifi-

cant descriptions, that is of the activity involved in (ii)–(iv). Thus the stimulus and enabling conditions for the operation of the mechanism must be satisfied; the mechanism must be isolated and the flux of conditions held constant or otherwise controlled; and skilled observers must be present to perceive or detect it. And secondly, neglect of the possibility of non-enduring mechanisms, of laws which though universal and normic in form (i.e. transfactually applicable) are themselves bounded in space and restricted in time. On the transcendental realist ontology, the description of what the world must be like if science is to be possible, the classical principle of indifference (or invariance) applies only to structures, not to events. But structures may themselves be transformed; and so concepts of diversity and change, like that of structure, squeezed out by the implicit undifferentiated ontology of empirical realism, may come to occupy as significant a place in ontology as the concept of indifference.

I have stressed throughout this study the diversity of phenomena and the autonomy of the various sciences (both from one another and with respect to common sense), the opacity of their concepts and the strangeness of the objects with which they have to deal. Natural processes are independent of man but man is himself in, and in continual interaction with, nature. Nature itself is diversified and complex. But I doubt whether our concept of nature can be understood in isolation from our concept of man; and I doubt whether we understand the latter. The scope of this enquiry has however been strictly philosophical; and its conclusions apodeictic. I have shown the structured and intransitive character of the objects of scientific enquiry to be a condition of the intelligibility of experimental activity and the social nature of knowledge to be a condition of the intelligibility of scientific training. (The laboratory and the classroom are the two most under analysed, and yet the two most obvious, *sites* of science). I have isolated the conditions of the plausibility of the doctrine of empirical realism and shown then to be very special. A conception of both nature and our knowledge of nature as differentiated and stratified has been advanced. And I have developed two criteria for the adequacy of any account of science: viz. its capacity to sustain the possibility of a world without men and the impossibility of knowledge without

antecedents. These establish the necessity for both of what I have called the intransitive and transitive dimensions of the philosophy of science: any account of science that does not view knowledge as socially produced and the objects of knowledge as independent of men must be ruled out as a possible account of science.

If science is to be possible the world must be one of enduring and transfactually active mechanisms; and society must be a structure (or ensemble of powers) irreducible to but present only in the intentional action of men. Science must be conceived as an ongoing social activity; and knowledge as a social product which individuals must reproduce or transform, and which individuals must draw upon to use in their own critical explorations of nature. Science is a process in motion, continually on the move from manifest behaviour to essential nature, from the description of things identified at any one level of reality to the construction and testing of possible explanations and thus the discovery of the mechanisms responsible for them. This process necessitates the construction of both new concepts and new tools (or the resurrection or refinement of old ones). The aim of science is the discovery of the mechanisms of the production of phenomena in nature; and it proceeds by way of a dialectic of taxonomic (or descriptive) and explanatory knowledge, in which the conflicting principles of empiricism and rationalism can be reconciled, a dialectic which has no foreseeable end.

In order to render intelligible scientific change and to reconcile it with the idea of scientific progress we must have the concept of an ontological realm, of objects apart from our descriptions of them. We can then allow, for example, that theory T_a is preferable to theory T_b , even if in the terminology of Kuhn and Feyerabend it is 'incommensurable' with it, if theory T_a can explain under its descriptions almost all the phenomena $p_1 \dots p_n$ that T_b can explain under its descriptions $B_{p_1} \dots B_{p_n}$ plus some significant phenomena that T_b cannot explain. We can speak in this way in the meta-language of philosophy; and we must speak so if we are to retain the idea of scientific progress without falling back on the idea of certain foundations of knowledge or theory-free experience. It is the intuition of this necessity that accounts, I think, for the readiness with which some philosophers of science have embraced Tarski's

theory of truth.⁶ But this theory cannot help us to resolve the problem posed by the apprehension of the general relativity of our knowledge: viz. that whenever we speak of things or of events etc. in science we must always speak of them and know them under particular descriptions, descriptions which will always be to a greater or lesser extent theoretically determined, which are not neutral reflections of a given world. Epistemological relativism, in this sense, is the handmaiden of ontological realism and must be accepted. Now this does not mean that it is impossible to communicate between different theoretical or conceptual schemes or that a scientist cannot know the same object under two or more different descriptions. To show the difference between say Newtonian and Einsteinian dynamics and that the latter is an advance on the former a scientist must be capable of doing so. Similarly though there is no guarantee of successful communication between the adherents of two different conceptual schemas, there is no inevitability about failure. (It is difficult to understand the concept of total failure.) Epistemological relativism insists only upon the impossibility of knowing objects except under particular descriptions. And it entails the rejection of any correspondence theory of truth. A proposition is true if and only if the state of affairs that it expresses (describes) is real. But propositions cannot be compared with states of affairs; their relationship cannot be described as one of correspondence. Philosophers have wanted a theory of truth to provide a criterion or stamp of knowledge. But no such stamp is possible. For the judgement of the truth of a proposition is necessarily intrinsic to the science concerned. There is no way in which we can look at the world and then at a sentence and ask whether they fit. There is just the expression (of the world) in speech (or thought).

Transcendental idealists are fond of saying that either knowledge must conform to objects or objects conform to knowledge: that either how we speak must be a function of things or things must be a function of how we speak.⁷ But this dichotomy is bogus. Science is an activity, a process in thought and nature which attempts to express in thought the natures and

⁶ See e.g. K. R. Popper, *op. cit.*, p. 224.

⁷ See e.g. H. Schwyzer, 'Thought and Reality: The Metaphysics of Kant and Wittgenstein', *Philosophical Quarterly*, 1973, p. 205.

constitutions and ways of acting of things that exist independently of thought. Thought has a reality not to be confused or identified with the reality of its objects: knowledge may change without objects and objects change without knowledge. There is no correspondence, no conformity, no similarity between objects and thought. Thoughts are only like other thoughts, objects (including thoughts) similar to or identical with other things. Things exist and act independently of our descriptions, but we can only know them under particular descriptions. Descriptions belong to the world of society and of men; objects belong to the world of nature. We express [our understanding of] nature in thought.

Science, then, is the systematic attempt to express in thought the structures and ways of acting of things that exist and act independently of thought. The world is structured and complex and not made for men. It is entirely accidental that we exist, and understand something about our bit of it. It is important to avoid the epistemic fallacy here. This consists in confusing the ontological order with the epistemic order, priority in being with priority in deciding claims to being, the question of what has relatively underived (or independent) existence with the question of what entitles us to regard some kinds of statements as grounds for other kinds of statements, etc. In particular the question of what is capable of independent existence must be distinguished from the question of what must be the case for us to know that something is capable of independent existence. Thus electrons could exist without material things; but we could not know this proposition, let us say P, unless there were material things. The truth-conditions for our knowledge of P are not the same as the truth-conditions for P. There could be a world without men; but there could not be knowledge without antecedents.

Postscript to the Second Edition

In this postscript I wish to clarify ambiguities in two key terms in the text; comment on what I now see as its principal weaknesses; and indicate some of the ways in which I think further work is necessitated.

Two terms, viz ‘law’ and ‘cause’, are used in a systematically ambiguous way throughout the book. In general I think that the context always determines which usage is intended. But my failure to distinguish them explicitly may have been a source of gratuitous misunderstanding.

The term ‘law’ is customarily used to refer both to statements of law and to what such statements designate. A distinction between the two is, of course, implicit in the basic distinction of the work, that is between the transitive and intransitive dimensions in the philosophy of science. Yet the term ‘law’ together with the terms describing their characteristics (‘normic’, ‘transfactual’, etc.), is used indifferently in the text to refer to both. A critic determined to be captious may find confusion here where none in fact exists.¹ Were I to rewrite the book I

¹ See e.g. C. Whitbeck, ‘Review of *A Realist Theory of Science*’ (henceforth R.T.S.), *Philosophical Review* July 1977, p. 115. It is, however, going too far when this reviewer takes me to task for distinguishing a tendency from a power in two different and incompatible ways within the space of two pages (*ibid.*, p. 118n.8) when on those very pages (pp. 230–1) I explicitly distinguish two concepts of tendency (tendency₁ and tendency₂) devoting considerable attention to an elucidation precisely of their differences! Caroline Whitbeck’s captiousness is surpassed only by that of John Krige who, writing in *Radical Philosophy* 12 (Winter 1975), p. 39. represents me as attempting to deduce the development of chemistry (presumably on pp. 168–9) from one inorganic reaction, when it is clear that the reaction cited, that of hydrochloric acid and sodium, is given only as an *example* of the *kind* of reaction explained by the theory of atomic number and valency. Krige then goes on polemically to ask how I would fit alchemy and phlogiston into my schema. But my schema is designed to illustrate the

would restrict my use of the term 'law' to the concept in the intransitive dimension, always qualifying its use to denote the concept in the transitive dimension by some term such as 'statement'.

Secondly, I use the term 'cause' to refer both to the antecedent event, condition or agent which triggers a mechanism and to the mechanism (and *a fortiori* the law it grounds) itself. Thus I talk both of men as causal agents of sequences of events and of the causal laws to which their activity may sometimes give them access, but of which they are not of course (in general) the agents. This duplex use of the concept of causality at a crucial state in the argument may have confused some readers. Thus I do not argue (on pp. 33ff), as e.g. Sharpe supposes, 'that

concept of the real stratification of the *world*. And it is clear that I can only do this by invoking some or other *scientific* ontology. Now it is a fact, unfortunate though it may seem to some romantically-inclined philosophers, that there is at present no scientific ontology that includes the three principles (salt, sulphur and mercury), phlogiston and electrons. Each new scientific breakthrough redraws, or situates the possibility of redrawing, the contours of its terrain of reality. So that from each new standpoint in theoretical time the history of the science (its positive and negative contributions to the present) looks different, is rewritten, a fact that Bachelard attempted to register with his concept of 'recurrence' (see e.g. G. Bachelard, *Le Rationalisme Appliqué*, P.U.F., Paris 1949, p. 2 & passim. Cf. also, of course, T. S. Kuhn, whom Krige cites approvingly, *The Structure of Scientific Revolutions*, p. 137 ff.). Now it follows from the syncategorematic character of philosophical discourse, stressed in the book, that I cannot give a concrete illustration of the philosophical ontology of transcendental realism, without making use of some or other scientific ontology. But that does not imply a philosophical commitment to it. A history of chemical theories representing the development of thought (a concept in the transitive dimension) not the stratification of the world (a concept in the intransitive dimension), would of course include alchemy, phlogiston and much else besides.

As I have cited Whitbeck and Krige as captious critics it is perhaps only fair to respond to their more serious points. Whitbeck finds a difficulty in the concept of the non-actual real, contending that possibilities and mechanisms 'must be actual in order to explain the behaviour which may occur' (*op. cit.*, p. 117). Now the non-actual real includes both real possibilities (and latent mechanisms), and their unfulfilled exercise. It is not the case that a tendency has to be actualised in an event to help to explain it (see above pp. 99–100). But neither is it the case that science is concerned only with the explanation of events, as distinct from possibilities. Moreover events, when they are explained, are typically explained as instances of the

the causal agency involved in the scientist's intervention cannot itself be simply reduced to sequence'.² Rather, I argue that causal *laws* cannot, if we are to render intelligible the significance of and necessity for the scientist's experimental activity, be reduced to sequence. My main criticism is of the Humean account of laws. Only in a subsidiary argument (see e.g. pp. 66, 117) do I object to the Humean analysis of the agency involved. The former implies the latter, but the converse is not the case (as

possible. Real possibilities may or may not have an actual basis: fragility for us, does; gravity, for Newton, did not (cf. p. 180 above).

Krige argues that if it is discovered that some naturally occurring pattern of events repeatedly reveals the presence of a specific tendency 'the claims made by orthodoxy philosophy of science gain respectability' (*op. cit.*, p. 38). Now while it is clear that some systems, such as biological ones, are more nearly closed (reveal a greater degree of regularity of behaviour, or recurrence of syndromes) than others, such as social ones, if the law-like statements still have to be analysed as *tendencies* and not as invariant conjunctions of events, as orthodox philosophy of science requires, then it is not a whit better off. And if, on the other hand, they do not, then we are dealing with a naturally closed system, which transcendental realism can of course allow (see p. 91 above). But orthodox philosophy of science now faces the problem of what governs phenomena, and would license the application of laws, outside such systems (see p. 65 above).

Whitbeck also claims that I do 'not sufficiently appreciate how little can be said . . . about the general character of the real world' (*op. cit.*, p. 117) declaring the task I have undertaken to be beyond human capabilities (*ibid.*, p. 118). Now, as in 1.4, I argued that every theory of science, or epistemology, whether it likes it, admits it or not, presupposes an ontology, the onus is surely on any critic to show how a theory of knowledge can do without one. It is that endeavour, though not without effects (see e.g. pp. 41–4 above), that I have tried to show is truly beyond human capabilities. Knowledge cannot be prized apart from its form, and cut loose from assumptions about what the world must be like for it to have the form that in some theory of knowledge it is claimed to possess. From now on the boot is on the other foot: there is no escape from ontological commitment.

Finally, Krige reckons (*op. cit.*, pp. 38–9) that I fail to describe the mechanisms that would make science a liberating force and not an oppressive ideology'. *Allez les rouges!* Now in this book I have attempted to sketch some of the ways in which traditional philosophical conceptions of science can function as ideologies. But *A Realist Theory of Science* does not purport to be a political economy, or sociology of science. Obviously, such work is both legitimate and necessary.

² R. A. Sharpe, 'Review of R.T.S.', *Philosophical Quarterly* July 1976, p. 284.

is shown by the possibility of Davidsonian anomalous monism).³ The importance of this is that many philosophers, particularly those influenced by hermeneutical thought, such as von Wright and Apel, have recently drawn attention to our causal activity (in experimentation) in science, taking this as a criticism of the empiricist account of causality, without recognising its significance (cf. p. 54 above): viz in yielding a definitive criticism of the empiricist account of laws, and hence of the whole wretched ontology of empirical realism.⁴

³ See e.g. D. Davidson, 'Psychology as Philosophy', *Philosophy of Psychology*, ed. S. Brown, Macmillan, London 1974, reprinted in *The Philosophy of Mind*, ed. J. Glover, Oxford University Press, Oxford 1976, pp. 102–3. (For a critique of the Davidsonian defence of Humean theory see pp. 140–1 above).

⁴ Perhaps it is this which accounts for Sharpe's failure to appreciate why 'the connection between the underlying mechanism and the observed phenomena' cannot, as the Humean supposes, 'succumb to an analysis of cause in terms of sequence' (*loc. cit.*). As most readers of the book have fully appreciated, it is precisely because of the existence in nature of two kinds of systems, viz open and closed ones, and the fact that human activity is in general necessary for the latter, that laws, if they are to be universal and operate independently of human activity, cannot be analysed as sequences at all. (See e.g. the reviews by R. Harré, *Mind* October 1976, pp. 627–30 S. Körner, *T.L.S.* 11.4.75 p. 397, W. Outhwaite, *Social Studies of Science*, February 1976, pp. 123–7). Incidentally Sharpe quite erroneously attributes acceptance of a 'like cause like effect' principle to me (*op. cit.*, p. 285). (For an explicit rejection see p. 77 above.) As I repeatedly emphasise I am committed to (relative) invariance only at the level of structures, not events (see e.g. p. 219). Still this does leave open the fair question of 'what entitles us to use . . . generality as a criterion of the existence of a causal (nomic) connection?' (*loc. cit.*). Now if two objects have the same essential nature they must behave the same way (see 3.5 above). But this condition derives from the identity of their natures. Each object must behave as it does because of its essential nature. But it is contingent that two objects have the same essential nature; just as it is contingent how many other objects do (cf. p. 226 above).

On the realist theory of universals developed here, there is no puzzle about generality. Invocation of the latter as a criterion for the existence of normic behaviour derives from the assumption of the identity of powerful particulars classified together as members of a natural kind, which in turn expresses a science's commitment to a certain line of enquiry (cf. p. 210 above). Sharpe is also worried about how 'the supposition that there is an underlying structure in virtue of which substances have the features they do . . . [turns] the relation from one of analyticity into physical necessity,' (*loc. cit.*). But if my transcendental argument for the intransitive and

In general the work has been subject to three kinds, or levels, of criticism: (I) of the transcendental method employed; (II) of the particular arguments used; and (III) of the results obtained. I shall restrict my remarks to (I) and (II), since if the arguments are valid and the method sound, the results must follow. It is, however, worth mentioning that many critics of, or sceptics about, (I) and/or (II) have been willing to concede that transcendental realism of the sort elaborated here, or something very like it, 'has been implicit in the thinking [activity] of most creative scientists from Galileo to our own time'.⁵ Now, if this is the case, and transcendental realism does successfully recapture what Bachelard has called the 'diurnal' philosophy of the scientists, i.e. the philosophy implicit in their spontaneous practice. And if it is also the case that the theses of transcendental realism depart radically from the 'nocturnal' philosophy of the philosophers, forged in the schools of empirical realism, to which scientists too tend to return when they reflect upon their practice, then the problem arises of deciphering the meaning, and explaining the mechanism, of this discrepancy.⁶ To this problem I will return.

A good example of the kind of criticism directed against transcendental realism at levels (I) and (II) is provided in a recent book by Ruben.⁷ Ruben argues that though reality is indeed, as I claim, structured and intransitive, there cannot be any interesting non-question-begging valid deductive arguments for this conclusion. To this end he seeks to demonstrate that my argument for the structured nature of reality is

structured nature of the objects of scientific enquiry (in 1.3) is correct then Sharpe has inverted the problem of the relationship between physical and logical necessity. For whether or not a relationship of natural necessity obtains in any given case is quite independent of men, and hence prior to the question of the logical status of the propositions we use to express it (changes in which can, indeed, be shown to have a certain rationale) (cf. pp. 2001 above). I cannot give a sense to the notion of logical necessity in nature, unless it be taken as referring to that aspect of nature which consists in the spatio-temporally situated actions of men.

⁵ S. Körner, *loc. cit.*

⁶ See my forthcoming 'Philosophies as Ideologies of Science', *Essays in Marxist Philosophy* (Provisional title) eds. J. Mepham & D. Ruben, Harvester Press, Hassocks, 1978.

⁷ D. H. Ruben, *Marxism and Materialism*, Harvester Press, Hassocks, 1977.

invalid, and that my argument for its intransitive nature itself presupposes what I am trying to prove.

Ruben claims that my contention that the experimental scientist produces a sequence of events is false. For, though he produces the antecedent *a*, and thus indirectly produces the consequent *b*, it does not follow, given that *a* occurs, that he produces the sequence *a*, *b*.⁸ For it might well be the case, according to Ruben, that whenever *a* occurs (however produced) so does *b*. That is, I have not eliminated the possibility of empiricist reconstructions of causal laws as sequences.

But of course I have. For our causal activity is as much a necessary condition for the realisation of the consequent *b*, given that *a* occurs, as it is (in the context of an experiment) for the occurrence of the antecedent. I explicitly designate (on p. 53 above) the former activity as ‘experimental control’ and the latter as ‘experimental production’. Experimental production is necessary to ensure the operation of the mechanism; experimental control for the occurrence of a closure. My argument is that without our causal activity, given *a*, *b* may not, and in general will not, occur. Patently, if it is the case that our causal activity is necessary for the realisation of the consequents of laws, they just cannot be glossed, without absurdity, as empirical regularities. That is, if it is not the case that whenever *a* then *b* then in making a claim about a causal law, we cannot, if we are to sustain the intelligibility of the experimental establishment and transfactual application of our knowledge, be making a claim about a sequence of events. Instead we must be construed as making a claim about something that bears only a contingent relationship to the actual world (including that significant subset of it produced by human work). This claim, I have argued, is about the operation of a tendency or the working of a mechanism irrespective of its actualisation in any particular outcome. And if *that* is the case, then all the theories based on the flawed principle of empirical-invariance—from the consistency condition of monistic historiography of science to Feyerabend’s ‘dadaism’,⁹ from the Popper-Hempel theory of explanation to standard (empirical realist) analyses of counterfactuals (see

⁸ *ibid.*, p. 131.

⁹ See my ‘Feyerabend and Bachelard: Two Philosophies of Science’, *New Left Review* 94 (Nov.-Dec. 1975). p. 46.

p. 158 above), from the 2nd Analogy to Bachelard's cogitamus¹⁰—must all be radically wrong.

It is not clear to me why Ruben should take it as an objection to transcendental arguments, and not say mathematical proofs that in a deductively-valid argument nothing can appear in a conclusion which was not already, at least covertly, in the premisses¹¹ Still, if it is the case that philosophy is, as I have claimed it can be, a conceptual science, then, like any science, it ought to be able to tell us something we did not already know: it ought to be able to surprise us. Philosophy does so when it (for the first time) makes explicit what is already presupposed by the activities in which we engage; or when, to put it another way, it shows the condition of their possibility

Now Ruben contends that I assume in my argument (on p. 31) that scientific change consists in changing theories about the unchanging, and do not (and cannot, without circularity) prove it.¹² Here it is important to distinguish the concept 'scientific change' which can function in a neutral way between different philosophical theories from the premiss, established as a result of the *analysis* into the conditions of the possibility of change (and criticism) in science, 'scientific change (and criticism) is only possible on the condition that there are (relatively) unchanging objects, existing and acting (relatively) independently of the changing theories of which they are the objects'. From this premiss (only if Q then P) together with the minor premiss, viz that scientific change (and criticism) occurs (P), my conclusion (Q) does indeed follow. In this sense it is a touchstone of the validity of any argument, transcendental or otherwise, that the conclusion be 'implicit' in the premiss. But the interest of a transcendental argument clearly does not lie in the formal derivation of the conclusion, which is trivial; but in the production of the knowledge of the major premiss (i.e. in the analysis). It is in this that the essence of conceptual discovery in philosophy lies. Now such knowledge cannot be held to be already contained in the antecedently existing concept which figures in philosophical discourse as 'scientific change', as is shown precisely by the existence of non-realist explications of it.

¹⁰ *ibid.*, p. 54.

¹¹ D. H. Ruben, *op. cit.*, p. 101.

¹² *ibid.*, p. 100.

(It would require the drastic expedient of some absolute idealist thesis of the identity of opposites, or perhaps a Platonic theory of anamnesis to avoid this conclusion.)

But what are my grounds for the major premiss in this case? It is clear how on the assumption of the independent existence of nature the possibility of conflicting, differing, changing, clashing and inconsistent descriptions all become readily intelligible. But to leave the matter there would be to overlook just those ‘alternative neo-idealist interpretations of scientific change . . . according to which there is no neutral world “shared” by different theories or paradigms’.¹³ So, to ‘complete’ (on which more anon) the argument, let us consider them. Such theories posit either ‘incommensurability’ or ‘Kuhn-loss’. Now it is precisely a condition of the intelligibility of incommensurability (non-inter-translatability of the terms of the rival theories) that there exists a field of real objects with respect to which the rival theories are incommensurable. (As I have remarked elsewhere no-one bothers to say that the rules of cricket and football are incommensurable.) In ‘Kuhn-loss’, on the other hand, if it is total, there are no objects in common, so that Ruben’s objection is sustained. But now no sense can be given to the concept ‘scientific change (and criticism)’. For total Kuhn-loss involves neither transformation nor discursive intelligence, but an archetypal, intuitive understanding constructing its world in a single synthetic act¹⁴—a possibility from which it is significant that even Feyerabend has baulked.¹⁵

I do not think that any of the objections so far directed at the main theses of the book hold. Moreover I think the transcendental method employed is, in general, sound. It is, however,

¹³ *loc. cit.*

¹⁴ Cf. I. Kant, *Critique of Judgement*, Hafner, New York, 1972, pp. 249–58.

¹⁵ Thus note the overlapping domain marked D in the second diagram on p. 178 of his *Against Method*, New Left Books, London, 1975.

(For the record I should add that the particular argument that Ruben cites (on pp. 31–2 above) I take only as establishing the intransitivity of the objects of *experience*. The intransitivity of the objects of *knowledge* is established by the analysis of experimental activity, not simple sense-perception. For me, the objects of knowledge are disjoint from the objects of experience; and the conditions of possibility of knowledge and experience are not the same.)

certainly the case that the book contains no adequate defence, or meta-philosophical justification, of the latter.¹⁶ This is not the place to make good this omission, which I now regard as the most serious in the work. A sequel, to be published shortly, will treat the matter fully. In the meantime some brief comments on the question ‘how is philosophy possible?’ are in order.

On the conception of philosophy at work in this book both the ultimate premisses and the immediate conclusions of philosophical considerations are contingent facts, the former (but not the latter) being necessarily social and so historical. It is only in this relative or conditional sense that philosophy can establish synthetic a priori truths (truths about the world investigated by science). Philosophy operates by the use of pure reason. But it does not operate by the use of pure reason alone. For it exercises that reason always on the basis of prior conceptualisations of historical practice, of some more or less determinate social form.

Now philosophy as so conceived, can tell us that it is a condition of the possibility of scientific activities ϕ , ψ , etc. that the world is structured X and differentiated Y. But it cannot tell us *what* structures it contains or the *ways* in which it is differentiated. These are entirely matters for substantive scientific investigation. Scientific activity is a contingent, historically transient affair. And it is contingent that the world is as described by X, Y, Z. But given ϕ and ψ , X and Y *must* be the case. A ‘deduction’, or demonstration, of this necessity, (which may be termed ‘transcendental’) and which is represented by the major premiss of a transcendental argument when set out in its trivial ‘deductive’ form, will normally consist of two parts, a straightforward ‘positive’ part in which it is shown how e.g. X makes ϕ intelligible; and a ‘negative’ part in which it is shown how absurd, incoherent, counter-intuitive or counterfactual results flow from the failure to sustain X, typically expressed in the form of one or more theories that implicitly or explicitly deny it.¹⁷ (Thus if laws are identified with empirical regularities we have the absurdities that men, in their experimental activity, create or even change the laws of nature; and that either outside

¹⁶ A paper published in the same year ‘Forms of Realism’, *Philosophica* 15 (1), 1975, pp. 99–127 merely touches on the subject.

¹⁷ Cf. W. H. Walsh, *Kant’s Criticism of Metaphysics*, Edinburgh University Press, Edinburgh, 1975, pp. 102–6.

closed systems nothing governs phenomena, so that nature becomes radically indeterministic, or that as yet science has discovered no laws!) Misunderstandings about the intentions of transcendental arguments typically stem from the failure to appreciate the critical contexts in which they are developed—*against* already existing philosophical theories. Thus it is certainly the case that there is no way of demonstrating the uniqueness of the conclusion of such an argument in advance of every possible philosophical theory.¹⁸ But the transcendental consideration is not deployed in a philosophical vacuum: it is designed to replace or situate, an existing theory; and may come, in time, to suffer a similar fate. Moreover both the acceptability and acceptance of some piece of philosophical reasoning will depend upon the acceptability and acceptance respectively of the minor premisses concerned. Someone who denies that our knowledge is experimentally established and practically applied and that science develops in time need be bound by none of the results of this book. Further it should perhaps be stressed that I have not demonstrated that transcendental realism is the only possible theory of science consistent with these activities; only that it is the only theory *at present* known to us that is consistent with them.

Some 'implications of this conception of philosophy should be noted. First, by making the possibility of philosophical discourse contingent upon the actuality of particular social practices it provides, at least schematically, a way of reconciling transcendental and historical analyses of human activities such as science.¹⁹ Secondly, philosophy cannot anticipate the form of a successful scientific practice; that is to say the minor premisses of the arguments it uses may have to be developed afresh in the case of each specific science. Does this mean, then, that transcendental realism can do no more than paint its grey on grey? No. For though I have developed my arguments in this book mainly from a consideration of the experimental sciences of physics and chemistry, ideologies, derived from defective conceptions of those sciences, weigh, like a dead hand, heavily on the shoulders of many of the other sciences, and

¹⁸ S. Körner, *Categorical Frameworks*, Blackwell, Oxford, 1970, p. 72.

¹⁹ Cf. e.g. S. B. Barnes, *Interests and the Growth of Knowledge*, Routledge and Kegan Paul, London, 1977. chap. 1.

particularly of course the proto-sciences of society and man. Philosophy can perform at least two tasks here. First, freeing these sciences from the intellectual grip of theories secreted by the flat undifferentiated ontology of empirical realism, it can set the terms for a more rational appraisal of the real problems they face. For instance, in the absence of spontaneously occurring, and given the impossibility of artificially creating, dosed systems in the human sciences, their criteria for the rational confirmation and rejection of theories cannot be predictive and so must be exclusively explanatory.²⁰ But besides this negative function, another more positive possibility opens up for philosophy too. For, it shares an affinity with social science in that they both seek, as at least part of their project, to identify and describe the conceptions of agents engaged in social practices. Given this, the possibility is bound to arise of posing for social practices other than science transcendental questions of the form 'what must be the case for ϕ to be possible?' where ' ϕ ' denotes some characteristic activity as conceptualised in experience. The conclusion of the argument would be a statement of the conditions of possibility of the particular social activity concerned. However, in opposition to a neo-Kantian stream of thought, transcendental realism allows that such conditions are real and subject to historical transformation, so that the resultant hermeneutics is contingently critical.²¹ If this formal analogy between philosophical and social scientific discourse should prove fruitful then philosophy could live down Hegel's jibe (and Bradley's that it merely produces bad reasons for what we believe on instinct) and help to act not just as the underlabourer, but as the midwife of a science, or group of sciences.

The book aimed to present a systematic account of science. I believe the account to be substantially correct. But it was an error to imply that the account could also be complete. For if philosophy is indeed, as I have been arguing, a conceptual science which takes as its premisses human activities, situated in

²⁰ See my 'On the Possibility of Social Scientific Knowledge and the Limits of Naturalism', *Journal for the Theory of Social Behaviour* 8 (1) March 1978.

²¹ See my forthcoming *The Possibility of Naturalism*, Harvester Press Hassocks, 1978.

time and subject to transformation, then there is a sense in which philosophy's work can never be completed. Philosophy is in history too.

Philosophy is not independent of the various sciences, but neither is it reducible to them. Now this book aimed to be a philosophy *for* the sciences (and *against* the ideologies that threaten them). Throughout it I have stressed the close connection that exists between the transcendental realist ontology of enduring and transfactually active structures and a conception of scientific activity as work; and between the denegation of ontology in empirical realism and the view of facts, and constant conjunctions, existing quite independently of men. But the perceptive reader will have noted an asymmetry in the development of the argument. For while the ontology of empirical realism has been explained in terms of a certain conception of man, no explanation of the latter has been given. It is in this area that I think the most pressing problems for the further development of the Copernican Revolution in the philosophy of science lie.

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Index of Names

- Achinstein, P., 184n
Anscombe, G. E. M., 33, 34n, 110
Apel, K. O., 254
Aristotle, 21n
Armstrong, D., 237n, 238n
Aronson, J. R., 81n, 90n
Ayer, A. J., 37n, 70n, 180n
Ayers, M. R., 50n, 64n
- Bacon, F., 89, 146, 161
Bachelard, G., 252n, 255, 257
Barnes, S. B., 260n
Black, M., 216
Blanshard, B., 200n
Boscovitch, R. J., 86
Boyle, R., 89
Bradley, F., 261
Braithwaite, R. B., 96, 149n, 216
Bridgeman, P. W., 128n
Brodbeck M., 68n, 162
Buchdahl G., 7n, 13n, 149n, 242n
Bunge, M., 72n, 84n, 114n, 150n, 169n
- Campbell, K., 216n
Campbell, N. R., 152-5, 156, 157, 159
Čapek, M., 59n, 85n, 86n
Carnap, R., 128, 217
Chisholm, R., 149n, 158
Chomsky, N., 188n, 245n
Clavius, C., 150n
Conant, J. B., 7n
Copernicus, N., 150n
Craig, W., 152n
Crosland, M., 211n
- Danto, A. C., 240
Davidson, D., 140-1, 201n, 254n
Dijksterhuis, E. J., 55n
Dingle, H., 60n
Dirac, I., 86
Donagan, A., 133n
Dray, W., 72n, 123
Dretske, F., 35n
Duhem, P., 133n, 153, 157, 162, 190n
- Eddington, A. S., 59
Edwards, P., 217
Eve, A. S., 23n
Ewing, A. C., 200n
- Feyerabend, P. K., 9, 59n, 86n, 115n
118n, 131, 155n, 156n, 161n, 162
188n, 191, 248, 256, 258
Fisk, M., 200n, 201n
Flew, A., 175n, 213n
Foucault, M., 10n
Friedman, M., 138
- Gardiner, P., 72n
Gasking, D., 122
Geach, P. T 50n, 99-100
Gibbs, J., 64n
Gibson, J. J., 32n, 221n
Goodman, N., 150n, 152, 162, 212n,
218, 222n
Grunbaum, A., 137n
Grunberg, E., 94
- Hanson, N. R., 9, 31n, 55n, 64n, 167n
Hare, P., 215n
Harman, G., 219n
Harré, R., 9, 21n, 50n, 87n, 159, 164n,
168n, 175n, 177n, 181n, 202n, 211n,
237n, 254n
Harrod, R. F., 120n
Hart, H. L. A., 121n
Hegel, G. W. F., 7, 261
Heisenberg, W., 82n, 138
Hempel, C. G., 63n, 68n, 75, 127n,
130, 141-2, 149n, 153, 162, 222n,
225-6
Hesse, M. B., 9, 43n, 150n, 153n, 156n
Hobbes, T., 83, 87n
Hollis, M., 32n, 174n, 218n
Honoré, A. M., 121n
Hume, D., 24, 26, 37 40-2, 67n, 73n,
75, 90, 97, 143n, 149, 151, 157, 160,
161, 217, 218, 238, 244
Hutchison, T. W., 162n

274 *Index of Names*

- Johnson, W. E., 149
Joske, W. D., 50n
Joynt, C. B., 72n
- Kant, I., 8, 24n, 25, 37, 42, 84n, 86,
145, 258n
Kat, J. J., 150n
Kneale, W., 59–60, 70n
Kolakowski, L., 175n
Körner, S., 84n, 176n, 254n, 255n, 260n
Koyré, A. 55n, 81n, 228n
Krieger, J., 251–3n
Kuhh, 4n, T. S., 9, 42, 86n, 131, 191, 192n,
248, 252n
- Lakatos, I., 9, 132n, 152n, 160n, 162,
176n, 191n
Laplace, P. S. de, 68, 161n
Leibniz, G. W., 36, 67
Lewis, C. I., 212n
Locke, J., 10n, 36, 59–61, 79, 88–9,
213
Losee, J., 150n
- Mach E., 42, 128, 157
Mackie, J. L., 238
Madden, E. H., 181n, 203n, 211n, 215n
Mandelbaum M., 137n
Marshall, A. 244
Martin, W., 64n
Maxwell, N., 200n
Mellor, D. H., 39n
Michotte, A., 90
Mill, J. S., 29, 63n, 70n, 72, 92, 99–100,
103, 151, 153, 217, 244
- Nagel, E., 26, 71n, 100n, 130n, 149n,
152n, 158n 175n
Nell, E. J., 174n
Neurath, O. 128n
Newton, I., 42n, 73n, 79, 85–6, 90
- Osiander, A., 138, 150n
Outhwaite, W., 254n
- Pelling, H., 123n
Piaget, J., 90
Pierce, C. S., 128n
Poincaré, H., 101–2, 131n
- Polanyi M., 9, 111, 114n
Popper K. R., 9, 44n, 55n, 63n, 64n,
128, 130, 133n, 138–9, 162n, 176n,
188n, 190n, 213, 216, 217–18, 249n
Priestley, J., 6–7
Putnam, H., 202
- Quine, W. V. O., 39n, 162
Quinton, A., 38n
- Ramsey, F. P., 128n
Ravetz, J. R., 9, 21n, 34n, 188n
Reichenbach, H., 217
Rescher, N., 72n
Ronchi, V., 55n
Ruben, D. H., 255–8
Rudner, R., 68n
Russell, B., 128n
Ryan, A., 99
Ryle, G., 59n, 97, 128, 129
- Salmon, W., 217
Schaffner, K., 170n
Schlesinger, G., 169n
Schwyzer, H., 249n
Scriven, M., 9, 51n, 72n, 101n, 122n,
137n, 142, 157n
Sellars, W., 26, 39n, 59n
Shapere, D., 191
Sharpe, R. A., 252–4
Skinner, B. F., 138, 244
Smart, J. C., 31n, 44n
Stebbing, L. S., 59n
Strawson, P., 216n, 217, 224, 228n
- Thomson, W. (Lord Kelvin), 22
Toulmin, S. E., 7n, 9, 128n, 148, 158n,
214n
Tyndall, J., 154–5, 157
- Vernon, M., 221n
- Walsh, W. H., 84n, 259n
Weber, M., 101n
Weld, C. R., 118n
Whitbeck, C., 251–3n
Winch, P., 7
Wittgenstein, L., 8, 36n, 48n, 145
Wright, G. H. von, 33, 35n, 54, 240n,
254

Index of Subjects

- abstractions, 100, 237
- accidental sequences, distinguished from necessary sequences, 19, 28, 46, 149, 151, 157-8, 164-5, 183, 200, 225
- action
 - by-contact, 61, 79, 124; critique of, 86-7
 - classical paradigm of, 79ff, 198
 - human, 239; conditions for, 117
 - mechanistic conception of, 17; see also: classical paradigm of
 - subjects of, distinguished from conditions of, 78 107
- actual, domain of 13, 17, 56, 119, 229, 252n, 256
- actualism, 64
 - consequences of, 142
 - critique of, 92ff,
 - dilemmas of, 65-6, 92-6
 - strong actualism, 92, 141; refutation of, 114-17
 - weak actualism, 92, 142; critique of, 93-6
- actualities, 35, 226
- additivity, as a requirement for closure, 76
- adequacy, judgements of, in science, 167, 249-50
- agents (agency), causal, 45, 49, 77, 105, 109, 114, 122, 205, 215
- alternatives, elimination of, in explanation, 125
- analogical grammar, 242, 244
- analogies, Kantian, 257
- analogy, role of, in explanation, 153-4, 159-60, 167, 194
- analytic truths discovered a posteriori, 174
- anamnesis, 258
- anomalies, 161
- anthropomorphism (anthropocentricity), in philosophy, 16, 34, 44-5, 48, 58, 61, 158, 215, 243
- a posteriori knowledge of natural necessity, 19, 209-10, 214-15, 238
- astrology, 44n
- asymmetry, of causes and effects, 107
- atomicity
 - as a requirement for closure, 75-6
 - of events, consequences of, 38, 64, 143
 - of individuals, consequences of, 79
 - physical v. epistemological interpretations of, 82-3
- auxiliary (background) theories, 55, 179
- behaviourist science, positivist concept of, 189
- boundary conditions, 110, 112
 - v. initial conditions, 236-9
- branches of science, 181-2
- categories, Kantian, 37
- causal agents; see: agents
- causal analysis, 125
- causal connection, nature of, 18, 226
- causal laws, see: laws
- causal powers, 49-50; see also: powers
 - no direct awareness of, 215
- causal processes, irreversibility in time, 107
- causality
 - dynamic realist principle of, 20, 205, 207, 221
 - Humean theory of, 18, 129; critique of, 77, 139-40; critique of Davidsonian revision of, 140-1
 - mechanical view of, 79
 - problem of, 32
 - transitive verb model of, significance of, 90, 21-2
- cause
 - antecedent v. mechanism, 252
 - concepts of, 70-1
 - notion of, 121-2

- causes, countervailing (offsetting),
46, 98, 161, 233–4
- ceteris paribus (CP) clause, 95–7, 160,
162
- classification in science
as commitment to a line of enquiry,
210
theory of arbitrariness of, 211–12,
227–8
- classroom, significance of, 247
- closed systems, 14, 33, 35, 253n, 254n
definition of, 69–70
- closure, 17, 73, 132
artificial v. spontaneous, 91
celestial, 61, 68, 198, 242
conditions for, 74–6, 78
Laplacean, 92
paradoxes of quest for, 77
universal v. restricted, 91
- co-determination, 165–6
- cogitamus, Bachelardian, 257
- complex determination, theory of, 111
- compounds, 227, 237
- computational determinism, 71
- conceptual breaks, in science, 155
- conceptual schemes, in science, 192–3
- condensation, 122–3
- conjunctures, 119, 165, 227
- connection, as an ontological category,
158
- connections, and conjunctions, 144
- consistency condition, 131
- constant conjunction-analysis of laws
see: Humean theory of causal laws
- constant conjunctions
as artefacts, 54, 246–7
as social products, 45, 58
- contingency
of the causal connection, 143
of events, 173
of the manifestation of tendencies
and powers, 212
- continuous series, 123
- conventionalism, 128, 131
- corpuscularian world-view, 79, 83–6
- correspondence, isomorphic, in em-
piricism, 24–5, 41, 241
- correspondence rules, 170
- counterfactual statements, 149, 158
166n, 225, 256
- covering-law requirement, 130, 132–3
- dadaism, 256
- darstellung, 154
- deducibility
not a necessary condition for
explanation, 119, 137 & n, 160
not a sufficient condition for
explanation, 71, 150 & n, 152–4
requirement, 130, 132–3
of tendencies, as a criterion for
knowledge of natural necessity
201, 202
- deductivism, 128, 129–32
critique of, 132ff, 141–2
- definitions
nominal; see: nominal definitions
operational, 128
ostensive, 128
real; see: real definitions
- demonstrative criterion, 178
- description
incomplete, 74, 105, 114
qualitative, conditions for, 178;
limits of, 179–80
- descriptivism, 42, 128
- determinism, 18, 67
error underpinning, 107
sense in which science presupposes,
108
varieties of 69ff
- differentiation
of reality, 18, 19, 66–7, 144, 163, 247
of the sciences, 119, 247
- disjuncture, possible, between domains
of real, actual and empirical, 49,
186, 241, 252n
- disorder, notion of, 71
- displacement, 122–3
- dogma, problem of functions of, 162
- dogmatism, 43
- dual (and multiple) control, 110ff
- economics, 44n
- education, 216, 219
conditions under which justified, 219
- emergence, 113
- empirical, domain of, 13, 17, 56, 229
- empirical-invariance, principle of, 127,
129
critique of, 132
- empirical realism, 15, 17, 18, 26, 241–2
critique of, 56ff, 198, 241–3
dilemma generated by, 163
effects of, 41–2, 211–12, 243
genesis of, 198, 242–3

- mistakes of, 16, 36
- trilemma faced with, 243
- empirical-testing, in science, 145–6
172, 192–3; see also:
 - experimental activity; perception
- empirical world, critique of concept of
16, 28, 35, 37, 58
- empiricism, 9, 19, 65, 128
 - classical; see: philosophy of science.
 - classical empiricist tradition in
 - ontological legacy of, 41, 147
 - radical, 149, 157; dilemma pro-
duced by, 151
- empiricist ontology, effects of, 41–4,
138, 157, 172
- engineering, 119
- ‘engineer’s dilemma’ (problem of
applied scientists’ excuse), 226
- entrenchment, 152, 162
- enumeration, incomplete, 74
- epistemic bias within philosophy, 44,
61, 243
- epistemic fallacy, 16, 30, 36–8, 200,
250
- epistemological individualism, 16, 148
187, 242
- epistemological relativism, 249
- equilibria, 232, 234
- essences 88; see also: nominal essence
 - real essence
 - not human creations, 213
- essentialism, 213
- events
 - conceived as conjunctures, 119, 227
 - independence from experiences, 13,
27, 31–2
 - pattern of explanation in open
systems, 125
 - unknowable 32
 - unperceivable, 32
- everyday life, and laws, 111
- experience, 13, 17; see also: experi-
mental activity; perception
 - analysis of, 30–6
 - conditions of significance of, 16, 28,
 - social production of 58
- experiences, scientifically insignificant,
8, 117–18
- experimental activity, 12, 13, 65, 144
 - analysis of, 33–5, 53, 256
 - limits of, 55–6, 244
 - rationale, 46, 53
 - role of, 158
 - significance of, 185, 226, 241, 253–4
256, 260
 - theoretical component of, 54–5
- experimental control, 53, 256
- experimental design, 35, 53
- experimental production, 53, 256
- experiments
 - crucial, 161
 - thought, 55
- explanation
 - deductivist theories of, 129, 130ff,
147, 233; see also: Popper-Hempel
theory of explanation
 - models, theories of, 152–4, 156–8,
159
 - in open systems, 125
 - scientific v. lay, 120
- explanatory problems, in science, 233
- external/extrinsic, 76
- facts
 - as social products, 42, 57–8, 189, 190
196, 242
 - correction (transformation) of, 160,
161
 - objectivity of, 27
- fallibilism, 43
- objections to theory of, 197–8, 199
- falsification
 - dyadic theories of, 197
 - genuine v. pseudo-, 160–1
- falsificationism, 128
 - critique of, 133ff
 - paradoxes of, 225
- fictionalism, 42
- freedom, human, 112, 117
- frontiers of knowledge, 180–1, 184–5
- generality (order), in nature, 144, 227,
233
- generative mechanisms; see: mech-
anisms
- Goodman’s paradox 18, 222
 - resolution of, 223–4
- gravity, 85
- Hempel’s paradox, 18, 222
 - resolution of, 224
- hermeneutics, 254, 261
- historical explanation, role of causal
notions in, 123
- historicism, critique of and critique of
Popper’s critique of 138–9 & nn

- human sciences, absurdity of orthodox philosophy of science in, 104–5
- Humean level of knowledge of natural necessity, 19, 171, 172, 174
- Humean theory of causal laws, 12ff, critique of, 33–5, 64–6, 149ff, 161, 164, 253–4
- genesis of, 64, 143–4
- Humean theory of causality; see: causality, Humean theory of
- hypothetical (imagined) entities and mechanisms, 166, 169
- criteria for the ascription of reality, 179–80, 182
- meaning of terms designating, 170
- iconic models and ideas, 152, 167, 239
- idealizations, 100, 237
- idealism; see: transcendental idealism; neo-Kantianism
- ideologies, generation of conservative, 42–3, 53, 131, 243
- idols Baconian, 61
- imagination, role of, in science, 155, 166, 185, 241
- imagined/imaginary, distinguished, 15, 145–6
- incommensurability, 258
- resolution of problem of, 248–9
- inconsistency
- and incommensurability, 191 & n
- and meaning-change, 190–1
- indifference
- principles of, 108; limits of, 247
- dynamic principle of, 202
- individualism, epistemological; see: epistemological individualism
- individuality, 227
- individuals
- Aristotelian, 182
- Strawsonian, 182, 212
- induction, 215
- conditions under which justified 218–20
- induction, problem of, 18, 20, 32, 163, 198–9, 206, 214–5
- ambiguity in, 216, 219
- conditions of intelligibility, 220–2
- critique of dissolution, 218
- critique of Popper's solution, 217–18
- resolution of, 221, 223
- responses to, 216–17
- inductive scepticism, 202ff
- distinguished from other forms of scepticism, 206ff
- refutation of, 204ff
- innovation, theoretical, 194
- instance-confirmation, principle of, 18, 127, 131
- critique of, 133–4
- instrumentalism, 42, 128
- intelligibility determinism, 70
- interactionism, 77
- interference in nature, 33, 53, 56, 240–1
- internal/intrinsic, 76–7
- intransitive dimension in philosophy of science, 17, 242, 251, 251–3n
- intransitive objects of knowledge, 21–2
- as structured, 35, 52
- intransitivity of objects of knowledge, significance of concept of, 159, 171, 200, 242, 248
- invariances
- as social products, 246–7
- realist theory of, 20, 91
- isolation, as a requirement for closure, 74–5, 76
- justificatory argument, and philosophy, 44
- knowledge
- and being, 36, 38, 39, 162, 169, 181, 217, 228, 249–50
- as a produced means of production, 17, 23, 24, 167–8 & n, 185
- as a social product, 16, 21, 62, 148
- as structured (layered), 162, 193
- bipolarity (double-articulation) of, 167
- correction (transformation) of, 186–7, 190–1 & n, 192
- creation v. transformation, 148, 187, 197
- explanatory and taxonomic, 20, 66, 173–4
- incorrigible foundations of, 16, 40; non-existence of, 189
- intransitive objects of, 17, 21
- (two) kinds of objects of, 17, 21
- (three) levels of, 19, 171
- necessity of, 46, 143, 228
- practical application of, 14, 65, 118; significance of, 256, 260
- reduction to sense-experience, 41; impossibility of, 148, 187–8

- reproduction and transformation of
 - 192–8, esp. 195
- transitive objects of, 17, 21
- universality of, 46, 67, 143, 164 & n, 228, 236
- Kuhn-loss, 258
- laboratory, significance of, 46, 225, 247
- language, 239
- law-like statements
 - analysis of, 92ff, 132–4
 - concepts in them not given in experience, 81–2, 120
 - mode of application of, 98
 - surplus-element in analysis of, 28, 149ff, 158, 183–4
- laws, scientific and causal 12, 17, 59 66, 105, 164 & n, 184, 222, 226–7
 - analysis of, 45ff, 92ff, 184
 - and statements of law, 251–2
 - as consistent with dual (and multiple) control, 109, 110
 - as empirical regularities, 164, 259–60
 - as expressing tendencies, 164, 253n
 - as imposing constraints, 106, 111
 - as unconditional sequences (strong actualism), 92, 151
 - backed by models, 164
 - establishment of, 163–4
 - necessity of, 14, 46, 164, 225
 - non-empirical character of, 109–10 112, 123–4, 256
 - ontological status of, 45, 49–52
 - ontologically distinct from events, 12, 17, 33–5, 54, 59, 65, 144, 164 236, 253n, 254n, 256
 - resistance to pseudo-falsification, 46
 - satisfaction of CP clause as a condition for the application of (weak actualism), 92, 96
 - transfactual character of, 46, 144, 164
 - universality of, 14, 46, 164, 225
 - unknowable, 59
 - with perceivable but unrealized consequents, 59
 - with unperceivable instances, 59
- Leibnizian level of knowledge of natural necessity, 19, 171, 172, 173, 174
- Lockean level of knowledge of natural necessity, 19, 171, 173, 174
- logic, laws of, 200
- machines, 67–8, 112
- magnetism, 85
- man, model of, underpinning empirical realism, 16, 17, 198, 243, 262
- material causes of knowledge, 17, 21 58, 61, 148, 185, 194
- material continuity, principle of, 84, 205
- matter
 - corpuscularian view of, 79ff
 - rationalist theory of, 80
- meaning-change, 170, 177; see also: inconsistency
- meaning-invariance condition, 131
- mechanics, Newtonian, 61, 68, 85–6
- Lockean interpretation of, 88–9
- mechanisms, generative, 14, 47, 48–50, 51–2, 53, 58, 62, 158, 184, 202, 233, 246–7; see also: structures
 - analysis of, 49ff
 - as objects of enquiry, 17, 241–2
 - reality of, 46
 - transfactual character of, 49, 66, 98, 164, 225, 236
 - transformation of, 236 247
- metaphor, in science, 194
- metaphysics, 36; see also: philosophy
- meteorology, 119
- microphysics, 155
- middle dimensions zone of. 59
- model-building, 15 145, 172
- paramorphic, 194
- models, 164, 192
 - bipolarity of, 160, 166–8
 - Campbell's theory of, 152–9
 - dynamic realist theory of, 159–60
 - interpretations of, 161–2
 - source of, 152–4, 160, 166–7
 - subject of, 154, 160, 166–7
- monads, pre-established harmony 67n
- monism, anomalous, 254
- monistic theory of scientific development, 130, 256
- multiple causation, 72
- natural accident, concept of, 158
- natural kinds, 171, 174
 - consistent with evolution, 214
- natural necessity, 14, 43, 183
 - advantage of transcendental realist account of 183–5
 - concepts of, in science, 171

- conditions for non-Humean theory
 - of, 152, 153, 157
- conflation with logical necessity, 201, 209, 254–5n
- deducibility as criterion for knowledge of, 201, 202
- levels of knowledge of, 171–4
- not traceable to sense-experience, 215
- objections discussed, 199ff
- truth-functional concept of, 128
- natural possibility statements, 78
- naturalistic fallacy, 37
- necessary connections between matters of fact
 - interpretation of, 199–201
 - necessity for, 144, 201–2, 203–4
- necessary sequences, distinguished from accidental sequences, 19, 46, 149, 157–8, 164–5, 183, 200
- neo-Kantianism, 9 26, 261
- Nicod's criterion; see: instance-confirmation, principle of
- nominal necessity, 157; see also:
 - natural necessity
 - v. logical necessity, 38, 200
- nominal statements, 14, 51, 157
- nominal definition, 211
- nominal essence, 202, 209, 211
- normal science, 42, 131, 192–3
- normic statements, 14, 16, 18, 51, 92, 125, 166n, 219, 226, 231, 236, 251
 - analysis of, 97ff
 - distinct from powers statements, 51
 - not counterfactuals, 102
 - not elliptical probability statements, 102
 - not empirical statements, 100, 109
 - not guarded generalizations, 101
 - not idealizations, 100
- objects of knowledge, structured and intransitive, 25, 35, 37, 52, 255
 - objections discussed, 47–9, 255–8
 - see also: intransitive objects of knowledge; transitive objects of knowledge
- Occam's razor, 154
- ontological commitments, nature of, 29–30
- ontological distinction between laws and patterns of events, 12, 17, 33, 46, 54, 144, 229, 236
- ontological independence of domains
 - of real, actual and empirical, 186, 228, 241
- ontological neutrality, impossibility of, 28–9, 252n
- ontological preference, 230
- ontological propositions, conditional nature of, 29
- ontological tension, 41, 147
- ontology, 23
 - and epistemology, 36, 38, 39, 181, 199, 253n
 - dissolution of, 37, 42, 45; and denial of social character of science, 58, 262
 - generation of implicit, 16, 17–18, 40–1, 0
 - irreducibility of, 40, 253n
 - nature of, 36, 39
 - need for revised, 9, 147
 - philosophical v. scientific, 26, 39, 251–2n
 - status of, 29–30
 - subject matter of, 36
 - transcendental realist, of transcendently active structures, 15, 47, 222, 262
 - vindication of, 52
- open systems, 14, 33, 35, 67, 73, 254n
 - explanation in, 125
 - responses to, 93
 - significance of concept of, 117–18, 164
 - significance of ubiquity of, 103–4
- opposites, theory of identity of, 258
- organization, principles of, 76
- paradigm, 192–4
- perception (sense-experience)
 - analysis of, 31–2
 - as skilled, 56–7, 60, 240
 - in empiricism, 41
 - role in science, 158, 240
 - significance of, 185, 226, 241
 - v. detection, 179–80
- persons
 - as causal agents, 33–4, 53, 65, 117, 122
 - as material things, 182, 228, 239
 - powers of, 239–40
- phenomenalism, 25, 26, 81
- philosophical argument, scope of, 52, 146, 247
- philosophical ontology, 36, 40; see also: ontology

- philosophical problems, 6
 - and transcendental realism, 10
- philosophical sociology; see: sociology
- philosophical
 - philosophy
- philosophy
 - as a conceptual science, 36–257
 - implications of, 259–62
 - classical, 43; anthropocentric and epistemic bias of 44
 - critical role of, 40, 43–4, 260–1
 - dislocation from science, 41–2, 45, 255
 - diurnal v. nocturnal, 255
 - function (tasks) of, 24, 52
 - involution in, 45
 - relation to science 7–8, 10, 36, 39 43, 45, 52, 146, 166, 176, 243, 244 255, 259–61
- philosophy for science, 10, 262
- philosophy of science
 - classical empiricist tradition in 14–15, 24–5, 128, 145, 164, 166, 172
 - Copernican revolution(s) in, 9, 61–2, 144, 148, 262
 - need for two dimensions in, 17, 242, 248
 - orthodox, critique of, 129ff, 237, 256
 - recent critical, limitations of, 8–9 147, 162, 164 & n, 222–3
 - three traditions in, 24ff
 - transcendental idealist tradition in, 14–15, 25, 128, 145–6, 147, 166, 172
 - typology, justification of, 26
- physicalism, 128
- Platonism, 38
- plausibility, as a criterion for explanation, 156, 166
- plurality of causes and effects, 72
- Poincaré’s problem, resolution of 101–2
- Popper-Hempel theory of explanation 18, 66, 129–30, 256
 - critique of, 132ff, 233
- positivism 8, 128
- positivists logical, 37
- possibilities (potentialities)
 - natural v. epistemic, 38, 77–8, 87–8 102, 232
 - types of, 18, 50
- power ascriptions
 - analysis of, 88, 231ff
 - and falsification, 177
 - and meaning-change, 177
- powers
 - and their exercise, 78, 180, 231
 - and liabilities, 87, 221
 - and tendencies, 18, 50, 172n, 229–32, 235
 - and ultimate entities, 182
 - conceptual system, advantages of, 183–5
 - conditions for concept, 88
 - ontological basis of, 176–8, 180, 229
 - role in scientific discovery, 175 176–8, 237–8
 - role in scientific explanation, 175 237–8
- pragmatism, 9
- predicates
 - artificial, 150n, 168n, 223
 - intensional relations between, 152
 - place- and time-dependent, 150n
 - theory of independence of, 211–12 222, 227–8
- prediction
 - deductivist theory of, 130
 - types of, 135
- predictive power, as a criterion for explanation, 151
- processes, assumption of linearity of, 82
- protective-belt, for theories, 160
- protolaws, 164, 166, 167, 172, 176 & n
- Prout’s hypothesis, 160
- psephology, 44n
- pseudo-falsification, 46, 160–1, 162
- psychological sciences, problem of confirmation in, 244–6
- pure v. applied science, 118, 120
- puzzles, 193
- quantum mechanics, 109
- radical empiricism; see: empiricism, radical
- rationalism, 19, 38, 173, 248
- reading, metaphor of, 196–7
- real, domain of, 13, 17, 56, 229
- real/actual, distinguished, 46, 119, 252n
- real definitions, 19, 171, 173–4, 211, 246
- real essences, 171, 209, 213, 214
 - and scientific enquiry, 209–10, 211
 - discovered empirically, 173
 - expressed in real definitions, 173
 - scepticism over, 60

- realism
 - empirical; see: empirical realism
 - generation of implicit, 16, 40
 - transcendental; see: transcendental realism
- realist philosophy of science, 9–10, 12, 20, 24, 100, 205, 221, 222, 226
 - connection with conception of science as a social activity, 57, 186–7, 262
- reality, domains of, 56, 58
- reasons
 - and regularities, 163–4
 - and things, 20, 212
 - ideals of, 205
- recognitive criterion, 178
- recurrence, 252n
- redescription, 124, 125
- reduction
 - limits of, 181
 - problems of, 89–90
 - senses of, 114–5
 - theory of, 130
- reductionism, 77
 - critique of, 115–16
- refutations
 - normally replacements, 190, 197
 - presuppose acceptances, 197–8
 - presuppose necessary connections, 201–2
 - presuppose resolution of problem of induction, 217–18
 - presuppose uniformity of nature, 199
- regresses, generated by drive to closure, 77
- regularity determinism, 69, 106, 141
 - definition of, 73
 - distinguished from computational determinism, 71–2
 - ontological presupposition of, 106
 - refutation of, 106
 - response to open systems, 74–5
- regulative principles, refutation of, 70
- repeatability, 232
- research programmes, 176, 192–3
- resolution, 125
- resonance, 243–4
- result/regularity, distinguished, 15, 91, 145
- retrodictability, criterion of, 73
- retrodiction
 - in explanation 125
 - significance of 135–6
- scepticism, 43; see also: inductive scepticism
 - about particular knowledge, 206–7
 - Heraclitean, 208–9
- science
 - accounts of, criteria for the adequacy of, 17, 24, 26, 148, 157, 197, 247–8
 - aim of, 17, 24, 47, 51, 148, 163, 189, 248, 258
 - and its uses, 118
 - and nature, 25, 185, 228
 - Aristotelian schematization of, 194
 - as explanatory, not simply descriptive, 186
 - as explanatory, not predictive, 222
 - as historical, 60–1, 189, 194–5
 - as a process, 16, 146–7, 159, 179, 183, 185, 248
 - as a social activity, 17, 189, 222
 - as work (transformation), 57, 167–8, 185
 - conditions of the possibility of, 26, 228, 239–42, 248
 - shoddy, Stalinist, etc., 188
 - social character of, 9, 182, 188; denegation of, 58, 262
 - stratification of, 9, 162, 169, 247; significance of, 170
- scientific activity
 - continuous nature of, 190; consequence of, 197
 - dialectic of 14–15, 145–7
 - phases of, 192
- scientific certainty, collapse of, 44
- scientific change
 - and criticism, significance of, 31, 35, 57, 187, 241, 257
 - and development, 9, 15, 57, 147, 190, 258
- scientific discovery process of, 18, 145, 166, 168–9, 176, 192
- scientific education (training), significance of, 31, 35, 57, 187, 241, 247
- scientific knowledge, under particular descriptions, 52, 190, 249, 251–2n
- scientific ontologies, transformations of, 153
- scientific progress, relativity of, 186, 190, 191n, 249
- scientific research, as detection, 39
- secondary qualities, 83, 90

- self-determination, possibility of, 111
114, 235
- sensationalism, 128
- simplicity, as a criterion for explanation, 150 & n
- skills, 47, 56, 60, 240
- social psychology, 44n, 196
- social sciences, 195–7, 244–6, 261
- social transmits, 148, 195
- sociology. 195–7
 - philosophical, 148, 195; generation of implicit, 16, 57–8
- solipsism, 24
- statements, classification of, in development of science, 201
- stratification of nature, 18, 19, 67, 120, 144, 162–3, 247, 254n
 - and reduction, 181–2
 - consequences of, 204, 214–15, 219
 - significance of concept of, 170, 189
 - unbounded, 170–1
- stratification of science, 9, 162, 169, 247
- significance of, 170
- structure
 - deep, 110
 - internal, 75
 - intrinsic, consequences of absence of concept of, 79
- structures; see also: mechanisms
 - distinct from events, 56–7, 118, 219
- subjunctive conditionals, problem of 18, 222
 - resolution of, 224–5
- substance
 - dynamic realist principle of, 20, 205, 207, 221
 - sciences of, effects of underdevelopment, 61
- symmetry between explanation and prediction, theory of, 18, 130
 - breakdown of, 125–6, 136–7
- syncategorematic nature of philosophical discourse, 52, 124, 190, 252n
- synchronic/diachronic, 143
- synthetic a priori truths, 216, 259
- taxonomic knowledge and explanatory knowledge, 20, 66, 173–4, 227–8
 - dialectic of, 211–12
- taxonomy, importance of, 212–13
- tendencies, 14, 16, 50, 68, 92, 174, 184, 214, 236, 253n; see also: powers
 - analysis of, 97–100
 - fulfilment not a condition of a tendency's exercise, 99–100, 252n
 - primary v. secondary concepts of 229–30, 251n
 - secondary concept (tendencies₂), analysis of, 231–5
- theoretical, senses of, 179
- 'theoretician's dilemma', 225
- theories, effects of interpretations of, 42–3
- theory
 - and experiments, 191 2
 - and its applications, 118
 - irreducibility to sense-experience, 148, 187–8
 - nature of, 187, 192
 - resistance to pseudo-falsification, 119
 - significance of, 152, 158
- things
 - autonomy of, 108, see also: agents as comprehensive entities, 106
 - criteria for the identification of, 203
 - in science, 98, 226
 - ontology of, 51, 223
 - ordinary, 227, 237
 - positivist theory of, 80
 - scientifically insignificant, 212
 - stratification of the properties of, 87
 - three worlds, Popper's doctrine of, 195
 - tools, 240
 - 'Tractatus-world', 54
- transcendental arguments, 30, 36, 106, 244, 257, 258, 260
- transcendental idealism
 - distinguished from classical empiricism, 27
 - distinguished from transcendental realism, 27
 - false dichotomy of, 249–50
- transcendental idealist tradition in the philosophy of science, 14, 25, 128, 144–5
 - restricted to Humean level of knowledge of natural necessity, 172
- transcendental realism, 15, 26, 56, 78–9 145, 147, 158, 225, 237, 246, 255, 261
 - criticisms of, 255, 261
 - v. actualism, 92ff
 - v. empirical realism, 26–7, 183–5, 221–2
 - v. transcendental idealism, 27–8, 164 & n, 166, 249–50

284 *Index of Subjects*

- transfactual statements, 14, 51, 92, 132, 166n, 251
- transitive dimension in philosophy of science, 17, 242, 251, 251–2n
- transitive objects of knowledge, 21–3
- transitive verb model of explanation, 121–2
- transitivity of process of knowledge, 195, 248
- truth, theories of, 249
- two worlds, Eddington's problem of, 59
- Tyndall's criterion, 154–5

- ubiquity determinism, 70
- ultimate entities, 182
- ultimate levels of knowledge, 171
- understanding, archetypal, 258
- uniformity of nature, 219
- uniqueness, 227

- universals
 - problem of, 6
 - real, 212, 227
 - realist theory of, 20, 227, 0
 - resemblance theory of, limits of, 212, 227
- universals of law v. universals of fact, 149

- verificationism, 37
- voluntarism, 44, 57

- ways of acting; see: mechanisms
- wholes and parts, 85
- work (labour), 57, 117, 178, 262
 - logical structure of, 185, 194–5
- world, view of as open v. view of as closed, 78–9, 117, 142, 225
- world order, objective, 171–4, 180–2
- worlds, counter-inductive and capricious, 218



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