REVIEWS

Elementary Illusions

Nancy Cartwright, How the Laws of Physics Lie, Oxford University Press, 1983, £16 hb, £7.95 pb

The classic work of C.G. Hempel established as orthodoxy the view that scientific explanation consists in subsuming phenomena under covering laws. With varying degrees of plausibility the analysis was extended to cover ordinary, everyday explanations, which were construed as abbreviated or elliptic versions of the genuine article. Whether the laws concerned are statistical or strictly universal, there is no doubt on the Hempelian view that they represent attempts to get at 'the truth' about the phenomena concerned. They fall short of this goal in so far as they fail to represent things accurately or completely, but these faults are to be repaired by subsuming low level, limited laws under laws of ever greater generality.

This picture is nicely in accord with the Official View of physics which portrays scientific progress in terms of unification of its fundamental laws - whether achieved through 'consilience of inductions' or 'revolutionary conjectures'. As fundamental physics approaches 'The Truth' so the explanatory power of its diminishing number of principles increases, and no less a theoretician than Stephen Hawking believes that The End is in sight, when we will have One Fundamental Law which explains everything. We have many historical images of this goal: the Pythagorean harmonies; the Cosmic Idea wrought in matter by Plato's demiurge; the Thoughts of Kepler's God; the Formula of Laplace's Omniscient Intelligence. The secularised modern version sees science as a Quest for The Truth, and this idea inspires both the popular and the professional imagination. But an implication of Nancy Cartwright's argument is that this Quest does not survive secularisation. Indeed, she argues, a presupposition both of the official view of scientific progress and of Hempelian orthodoxy in the philosophy of science is radically mistaken. 'The truth doesn't explain much,' she declares. The more the laws of physics are true the less they explain; the more they explain, the less they are true.

There would be nothing particularly new were this an exercise in reckless epistemological anarchy. And the view that science can do no more than 'save the appearances' and that its theoretical entities are mere convenient fictions has a venerable antiquity. A thorough-going instrumentalism seems impossible to confute - what can a realist point to but the very success in prediction and control which the instrumentalist says is the whole essence of science?

Nevertheless, despite the soothing way instrumentalism helps one to stop worrying about the paradoxes of quantum mechanics, a realist stance does seem to accord better with our strong intuitive feeling that the physical sciences, particularly as deployed in high technology, really have got onto something! Cartwright's argument cuts across this long-running dispute. She is toughly realist when it comes to theoretical entities and causal connections, and she is quite unashamed of the practical success of the sciences. But she is an anti-realist about laws - it's a lie to say the fundamental laws of physics represent The Truth, on the contrary they aren't true of anything at all.

What gives her analysis particular force is the way she deploys detailed examples from real physics. Unfortunately, this very fact may limit the readership since, even though most of the argument is directly accessible, it may be difficult for the non maths or physics graduate to exercise independent judgement on the technical illustrations. However, anyone who has had such a training will recognise the practices she describes and may then wonder why they accepted that the Hempelian account fits science any better than it fits everyday life.

Her account makes considerable use of a distinction which is drawn within physics between 'phenomenological laws' and 'fundamental laws'. The former involve the detailed, accurate description of specific, concrete physical processes. These lawlike descriptions are known with great reliability and are tested and refined with extraordinary accuracy - she cites the example of a commercial laser manufacturer who continuously runs a quarter of a million dollars' worth of lasers to destruction to check on their performance characteristics. These phenomenological laws are the closest we can get to the truth about what is going on in real situations, but they aren't explanatory. Explanation is what is provided by the fundamental laws when we try to subsume the phenomenological laws under them.

But this never works out simply and cleanly. For example, the law of gravitation gives the correct value for the force on an object only if no other forces are involved, and all relevant objects are included. But these conditions can never be satisfied, so we are faced with a dilemma: either we say the law is actually false, or we reformulate it so it remains true but counterfactual (i.e. it would apply if things were other than they actually are). However, the latter solution precludes the use of the law in precisely those situations where we make use of it, viz. to calculate the contributions of different factors to a composite 'cause'.

Nancy Cartwright concludes that all fundamental laws come with small print saying 'other things being right', but this is tantamount to admitting that the fundamental laws

are really false. What fundamental theory is able to explain is a 'simulacrum' (an idealised model of the situation). When it comes to explaining our highly accurate phenomenological laws, physicists start making approximations in an ad hoc fashion. Some approximations involve blatantly unrealistic assumptions in order to get the theory to fit the facts. Others involve piecemeal 'realistic' additions to get a better match. Either way the idea that the fundamental theory is 'true' emerges pretty battered, and that idea is made to appear still more precarious by an examination of the variety of theoretical approaches available in concrete situations. Different approaches are useful for explaining different models. Sometimes the order in which approximations are made has a critical influence on the result. All of this argues that the process of connecting fundamental laws to facts is more of a negotiation than a Hempelian deduction.

The profligacy of theoretical devices used in such negotiations stands in stark contrast to the uniqueness which physicists require of causal explanations. As Cartwright sees it there is just one situation in which you can make an inference from explanatory success to a 'best explanation' - and that is where you are citing causes. She rejects the Humean analysis of causes in terms of associations, and reinstates a causal nexus. That causality is real is presupposed by the possibility of the practice of devising effective strategies for coping with the world. Humean associations are inadequate for distinguishing effective from ineffective strategies.

Now causal connections are supposed to exist between the objects to which the fundamental theories of physics refer. Thus we can speak of them existing independently of the theories in which they are customarily embedded and hence allow different theories to be theories about the same real objects. But this claim, though appealing, is far from being unproblematic. Would it make sense to argue that the atoms of Democritus and Newton's particles of light were the same objects as Gell-Mann's quarks and Einstein's photons?

There surely comes a point when the changes in theory are such that we say we are talking of different things rather than just revising the way we talk about them. And the existence of theoretical entities hardly seems less precarious than our theories of them. Such venerable entities as Aristotle's crystalline spheres, Galen's humours, magnetic poles, austral and boreal fluids, phlogiston, electrick fluids, caloric and the luminiferous aether have all passed into oblivion despite the causal roles attributed to them. None the less a central part of Cartwright's thesis remains: a proliferation of causal explanations for the same phenomenon is not tolerable. Causal explanations compete in a way that other types of theoretical explanation do not.

Cartwright deploys her combination of 'theoretical-instrumentalism' with 'causal-realism' to dissolve one of the perennial puzzles of quantum mechanics: the so-called Measurement Problem. As is well known, when a stream of photons is sent through a pair of closely placed slits, their behaviour differs from that which occurs when the slits are opened alternately. Quantum mechanics represents this situation by associating a complex mathematical function with the photon which exhibits wave-like 'superposition' showing how the possibilities of passing through either slit 'interfere' with one another. This 'state function' evolves in a deterministic fashion, governed by Schrödinger's equation, and it would seem that if a particle is once put into a state of superposition, then so must be all systems with which it subsequently interacts.

But this has the apparently absurd consequence that the act of performing a measurement will put both our apparatus and ourselves into a state of superposition. In fact we always find our particles in specific states: it is as if the act of observation caused a sudden and indeterministic 'collapse' of a wave-packet, and this kind of evolution of the state function is governed by a different equation: von Neumann's projection postulate. This focus on measurement has led some people to make curious assertions about the role of consciousness in determining physical reality. Others, trying to preserve a realist interpretation of Schrdinger's equation, have argued that quantum mechanics shows the world is constantly splitting into an ever-increasing number of parallel universes.

The more orthodox interpretation has taken an instrumentalist stance: the state function is related to the probability of the particle being detected at a particular location. Positivistic slogans are used to prohibit us from talking about such particles having locations independently of observation. If we suppose they do have real, but unknown positions, then the two-slit experiment is rendered completely inexplicable - how can the slit the particle does not go through affect its behaviour? The tensions implicit in this account generate difficulties for either the logical or probabilistic concepts used in quantum theory.

Cartwright wants to preserve realism about entities and causal connections, but finds the conventional story about the collapse of the wave-packet unconvincing. Reductions of the wave-packet occur in all kinds of indeterministic transitions and not only in 'measurements'. She argues that where the state function makes itself felt is in calculating the probabilities of these transitions, and that if we concentrate on using this to interpret the formalism instead of thinking of positiion probabilities as fundamental, even though they have no causal role, then we will eliminate some of the logical puzzles of customary quantum theory.

Cartwright's position seems to imply that there are two kinds of evolution of a quantum system: Schrdinger evolution and von Neumann evolution. But this recreates a new form of the Measurement Problem: how does the system know which way to evolve? Her response is to argue that the evolution of quantum systems is governed by a more general quantum statistical equation in which the two kinds of situation are represented by a formal mathematical difference. This move is exactly what you would expect from someone who thinks that fundamental theory can get progressively 'nearer to the truth', except that she insists that there is no reason to suppose this difference marks a real physical property. In the end she dissolves the Measurement Problem as the product of a mathematical convention.

However, if one source of philosophical perplexity evaporates on this analysis, another hardens and crystallizes. Nearly fifty years ago, Einstein, with his co-workers Podolsky and Rosen, proposed a thought-experiment designed to show that the new quantum mechanics was 'incomplete'. They envisaged two systems in known, 'prepared' states which are allowed to interact and subsequently to separate. It appears that measurement on one of the separated systems will allow us to calculate the state of the other without interacting with it in any way.

The initial thrust of this argument was that, given certain minimal assumptions about 'physical reality', we have to acknowledge that quantum systems have well-defined properties independently of observation, even though quantum mechanics does not enable us to determine them. The only alternatives seem to be either that the system remains a composite 'whole' even when its parts have become in-

definitely separated, or that there is a kind of magical 'action at a distance', whereby fixing the state of one subsystem instantaneously fixes the state of the other. In the early 'sixties, J.S.B. showed that the predicted correlations between the states of the two subsystems differed in quantum mechanics from what you would get if you assumed these states were fixed by 'hidden variables' when the systems were in contact.

In the 'fifties, David Bohm pointed out that a version of the Einstein-Podolsky-Rosen experiment could be realised with electron spins or the polarisation of photons. And in 1982, Alain Aspect reported on the conclusion of a long series of experiments designed to discriminate between the predictions of quantum mechanics and those of hidden variable alternatives. Hume stipulated that cause and effect had to be 'contiguous'; in modern physics this has become 'the principle of local causes'. Aspect's work seems to show that this principle fails in quantum mechanics - action on one system alters the state of another even though there is no time for an 'influence' to propagate from one to the other.

Nancy Cartwright's approach to quantum mechanics avoids the puzzles which arise from taking position probabilities and Shrdinger evolution as fundamental, instead our attention is directed to transitions and their causes, but the price is that the Einstein-Podolsky-Rosen paradox becomes more vivid. We do indeed have instantaneous action at a distance in physics. As she acknowledges, it is characteristic for solutions to the Measurement Problem to leave the EPR-paradox untouched, and vice versa. Nevertheless her programme for a less mystifying interpretation of quantum mechanics deserves to be worked out in the form of a full-fledged textbook.

Nancy Cartwright's world is a rich profusion of things and properties linked together by causality. This world sometimes - amazingly - behaves in a discoverably regular way. We should be pleased we can sometimes explain things economically, even if the laws we postulate are demonstrably not true of the real world. Her argument has wider implications than merely a revision to the traditional explication of 'explanation' in the philosophy of science. If she is right, then we foster metaphysical illusions about the nature of science in thinking of its fundamental laws as getting us closer to the Truth.

Though it is not her avowed intention to do so, she strengthens the case for insisting that theoretical scientific knowledge has a socially constructed character. Yet with her firm insistence on the reality of theoretical entities and the reality of causes she indicates a clear role for the physical world to play in the determination of our scientific knowledge. What is at stake is a metaphysical picture. Pierre Duhem once contrasted the deep but narrow character of French science, insisting on the elegant mathematical formulation of fundamental principles, with the broad but shallow character of British science, filling the cosmos with ghostly confections of late Victorian engineering.

Unlike Pythagoras, Plato, Kepler, Newton or Einstein, Nancy Cartwright's God has 'the untidy mind of the English'. Of course, it may be that Stephen Hawking's awesome struggle from his wheelchair to gain intellelctual mastery of the universe by finding the One Law to rule all the laws of nature will actually succeed. It would however take an eternity to show that this Law was True. What Nancy Cartwright has done is to show that this goal indeed belongs to the eschatology of science, and that there is no certain warrant in the success of its present practices for beieving that there is any such Truth to be found.

Jonathan Powers

Physical Attractions

Jonathan Powers, Philosophy and the New Physics, Methuen, 1982, £3.95 pb

The advent of relativity and quantum theory earlier this century produced a host of philosophical problems which were engaged by some of the leading philosophers and physicists. These developments in physics led Planck, Einstein, Bohr, Heisenberg, Schlick, Reichenbach and numerous others to probe the foundations of physics and to re-assess the problems of time, space, causality, determinism and realism. Their discourse constitutes the 'grand tradition' in the philosophy of modern physics.

During the last few decades, however, this tradition has declined. Kuhn may be right in claiming that scientists only examine the fundamentals of their subject at a time of 'revolution'; it is certainly true that, with few exceptions, physicists exhibit little interest in the philosophy of their subject and the philosophy of physics - unlike the philosophy of biology - is in the doldrums. Jonathan Powers' book is to be welcomed for several reasons, not the least of which is that it aims to rekindle interest in the philosophical problems raised by modern physics.

In the opening chapter, Powers outlines a number of philosophical systems which have been deployed in science and finds them all wanting. Instead, he defends the modest claim that physical theories are not fully dictated by the structure of the natural world but possess a conventional component. The following three chapters are structured round the theme of conventionalism. The first deals with classical physics and in particular Newton's laws of motion; convention entering in the postulation of absolute space and time. The special theory of relativity is the subject of the next chapter, which shows elements of convention involved in the intepretation of space and space-time. Finally, a similar line is adopted in respect to quantum theory and especially the concepts of complementarity and indeterminacy. In a short concluding chapter, Powers draws some general lessons from these specific case studies.

There are a number of different ways of reading this book. It could, for example, be approached as a non-mathematical introduction to the new physics. In this it is very successful and Powers obviously enjoys, and is accomplished at, teaching physical theory to non-physicists. Most of his discussions and examples are succinct, although on a few occasions they are so compressed as to be difficult to comprehend. Secondly, the book is a useful primer in the philosophy of physics and succeeds in showing how the philosophical problems of physics have been discussed by the advocates of different philosophical systems and the limita-

tions to such approaches. Thirdly, the book contributes to the history of physics. Powers is well-read in the primary source material and, while he often trades on conventional wisdom, there are some places - most notably his discussion of the Michelson-Morley-Miller experiments - where he strikes out on his own. Finally, this is a consciousness-raising book, less in terms of the specific philosophical issues which Powers raises than because he frequently comments on such subjects as the role of myth and the importance of appreciating the social influences affecting physics.

Powers is to be congratulated on encompassing such a wide range of issues in 170 pages and for making a highly technical subject accessible and stimulating to the non-specialist. It is, however, hardly surprising that in such an eclectic book the main themes are not sustained. In respect to conventionalism, for example, Powers displays ambivalence; for while he often exploits the role of conventions he admits at other places that 'it can hardly be denied that the process of scientific development is, to a large extent, driven by <its internal> problems and goals' (p. 168, emphasis added), and there are sections of this book in which physics is portrayed in this very manner. In such sections the theme of conventionalism is lost from sight.

While conventionalism provides Powers with an interesting and philosophically important approach to physics, there remains some doubt as to how to characterise this central theme. The examples cited appear to illustrate very different types of convention. For example, Powers rightly calls Einstein's light postulate a convention in that it was not dictated by empirical evidence. However, it could be argued that Einstein adopted this postulate on grounds of simplicity since to assume that the velocity of light was different on the outward and return paths would have played havoc with the laws of physics. This example illustrates a type of convention very different from that involved in Powers' more general claim; that owing to the underdetermination of scientific theories, scientists adopt conventions reflecting their social, political or religious interests. This latter thesis is, however, potentially applicable to other examples of underdetermination; for example, the different interpretations of quantum theory. Thus not all cases of convention leave open the possibility of explanation in social terms (in any strong sense).

A related problem concerns the account we give of the decisions taken by scientists. At a number of places in this book Powers cites specific examples which appeal to explanation in terms of extra-scientific causes; thus Newton's advocacy of absolute space is related to his theology, cultural factors are cited as influencing the rise of indeterminacy (the Forman thesis), and some of the antipathy towards relativity is accounted for by antisemitism. In these and similar cases, Powers is appealing to the history of physics or, more precisely, the history of physics as distilled by certain historians of science. Thus he - like many other philosophers and sociologists of science - looks to history as providing an immovable point of reference on which to hang his interpretations. However, the history of science cannot fulfil this function so directly since historical interpretation is often a matter of considerable dispute, as in the case of the Forman thesis cited above. It is not that I intend to challenge any of the specific historical interpretations offered by Powers but rather to point to the disparity between his highly critical attitude towards interpretations of physical theory and his less critical acceptance of certain current interpretations in the history of physics. Part of Powers' account of conventions among physicists thus derives from the current conventions among historians of science.

Another, more fundamental, issue raised by this book is the current state of the philosophy of physics. In this country it tends to be an elusive subject with few institutional niches. Powers tells us that his book is based on lectures to STS (science, technology and society) students, humanities students and science teachers. Noticeably absent from his list are students of physics and practising physicists. Such omissions are not peculiar to the institution where Powers teaches but may also be taken to indicate a general lack of communication between physicists and philosophers of physics. There are indeed few places where physics students confront the philosophical problems raised by their discipline. Powers has the best of intentions in directing his book to the physics constituency (among others) since he believes that 'students of physics should take time between rounds of laboratory exercises and problem sheets to think about conceptual puzzles of their discipline' (p. xiv). It would indeed be laudable if they did; but, one is tempted to ask, why should they? The reason given by Powers is disappointing - 'for experience shows how easily profound misunderstanding can be masked by technical facility - important though that is'.

The importance of studying the philosophy of physics needs to be argued in stronger terms. Of the several further such arguments, let me indicate three. Firstly, philosophy should provide the physicist with a critical awareness of methods, concepts and the nature of scientific enterprise. Not only will this contribute to the individual's insight, but it can also increase the intellectual vitality of physics. Secondly, philosophy has been a major source for the critical evaluation of scientific theories and practices. A contemporary example is the realist critique of the Copenhagen interpretation, which has led to the call for experimental tests to discriminate between the Copenhagen interpretation and a family of alternative theories founded on the principle of locality. Thus, philosophical issues have reappeared at the very heart of modern physics. (See, for example, T.W. Marshall, E. Santos and F. Sellari, 'Local realism has not bee refuted by atomic cascade experiments', Physics Letters, 98A (1983), 5-9.) Finally, and most importantly, there is the need for the philosophy to provide the social and ethical perspective on science as a social activity with social consequences. For example, physicists are progressively being called upon to make public statements about the safety of nuclear reactors. All too often they possess neither the philosophical understanding nor the tools to deal with such issues. These arguments, and particularly the last, emphasise the need to include philosophy within scientific education.

While Powers seems uncertain about the role of the philosophy of physics, he appears to doubt whether the traditional techniques of the discipline will produce any conclusions of importance to the physics community. We first encounter this scepticism in the chapter on quantum mechanics: 'But a peculiar fact <sic> about quantum theory is that the formalism preceded the interpretation, and so disagreements over interpretation need not affect its experimental usefulness' (p. 138). By the end of the chapter Powers seems convinced that philosophy is irrelevant to the real ball-game of physics (pp. 163-4). Moreover, in the concluding paragraphs of the book he inverts the foundation metaphor, thus removing philosophical issues far from the concerns of the practising physicist.

What are we to make of this deconstructionist exercise? This book not only indicates that Powers has broken with the 'grand tradition' but also (when taken with other indications) that the philosophy of physics, as traditionally conceived, is lacking direction and in a far from healthy state. Is it worth reviving? How can it be revived? I leave these questions to philosophers of physics. Powers at least wants to offer some suggestions as to the way forward. At the didactic level he seems to feel that the philosophy of physics has a consciousness-raising role to play for students of physics. (Moreover, it can count on a lay audience; theologians, in particular, revel in the puzzles of quantum theory.) Again, Powers sees that the strong programme in the sociology of scientific knowledge offers a potentially insightful way of connecting philosophy with sociology and historical studies. To these suggestions we must add the need for the philosophy of physics to confront not only the intellectual, but also the social and ethical dimensions of

physics.

The philosophy of physics may possibly receive a new impetus from these or other directions, but one fundamental problem still remains; how to reconnect the subject with its source of vitality - physics. Certainly Powers is more aware than many of his colleagues that the philosophy of physics must confront post-war developments; he briefly mentions such innovations as Feynman diagrams and the theory of gluons. Yet such valiant attempts pale beside the larger problems of effectively combining the two subjects. Perhaps we will have to wait for another Kuhnian revolution before physicists will rush in droves to knock at the door of philosophy departments. In the meantime, stronger institutional links need to be forged between the subjects. (However, in the present climate such institutional innovations seem unlikely.) Another possibility, and a highly likely one, is that the philosophical puzzles of relativity and quantum theory will simply take their places beside other puzzles left behind by largely-defunct traditions. The clock paradox and the criticisms of the Copenhagen interpretation share much in common with the puzzles of angelology or the mystery of the trinity.

Geoffrey Cantor

Under the Spell

Charles Webster, From Paracelsus to Newton: Magic and the Making of Modern Science, Cambridge University Press, 1983, £12.50 hc

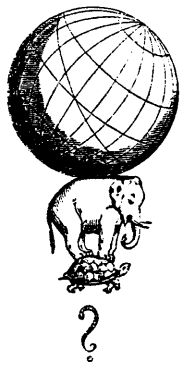
Magic poses crucial problems for the historical understanding of science. Is it simply diametrically opposed to science, as the founders of twentieth-century sociology and anthropology assumed? Max Weber, for example, saw the modern understanding and control of nature as deriving from an Entzauberung, or 'disenchantment', of the world. Magic was the mode of cognition characteristic of a 'primitive' society, in opposition to which understanding of modern society was to be constituted. But was the historical relation between magic and science in fact one of total disjuncture? Or did the transition between the two systems of knowledge-production and validation actually include elements of continuity? The period of the sixteenth and seventeenth centuries focuses these questions historically, for it was during this period in Western Europe that science assumed significant features of its modern form.

Charles Webster's Eddington Memorial Lectures, given at Cambridge in 1980, and published in this volume, take their point of departure from Lord Keynes's description of Isaac Newton as 'the last magician'. Webster claims that Paracelsus and Newton, far from occupying the completely incommensurable intellectual worlds of 'magic' and 'science', in fact show significant similarities of doctrine. He traces continuities and patterns of transmission between Reformation Germany of the early sixteenth century and late seventeenth-century Restoration England, with particular reference to three areas of occult knowledge: prophecy, spiritual magic, and demonic magic.

Discussing prophecy, Webster argues that an eschatological understanding of time and history, and a providentialist metaphysics, provided the framework for cosmological debate throughout the period. Paracelsus's ontology allowed for descending astral influences, although he criticised the actual practices of judicial astrology. Newton's cosmos, far from running like clockwork, depended on similar spiritual agencies of divine intervention. Comets, for both men, were agents of God's will, and portents of changes in the earthly realm. Millenarian expectations flourished in the turbulent political climate, both of Reformation Germany, and of the English Revolution. Furthermore, the importance of spiritual magic can be understood only be reference to this eschatological framework.

The improvement of man's earthly condition, whether by the Renaissance magus, or the experimental 'scientist', was a sign of the prophesied last age. Paracelsus's advocacy of 'natural magic', with its high valuation of the manual arts, and attention to detailed observation, pointed the way to the co-operative experimental and technical enterprises of Francis Bacon's followers in seventeenth-century England.

In the 1660s, the Royal Society took over this tradition, adapting it to the conservative climate of the Restoration, while some of its members, including Newton, retained an interest in subjects like alchemy. Finally, Webster argues against those who have assumed that it was the new natural philosophy of the late seventeenth century which disposed of the previously prevalent belief in witches and demons. He shows that several prominent Restoration scientists insisted that the existence of demons was essential to a non-materialist ontology, while the standard arguments against the existence of witchcraft, which ascribed the belief to delusions of the imagination, had already been canvassed by Paracelsus.



Webster's overview provides a very necessary corrective to those historians who have denied the importance of the magical tradition in relation to the origins of modern science, or who have too readily assumed an identity between the 'rise of science' and the 'decline of magic'. But I doubt that his emphasis on the continuities over this period can really give other than one side of the picture. This was, after all, the period when distinctively new features of a natural-philosophical theory and practice did emerge; and when the occult tradition suffered what Brian Copenhaver has called its 'disappearance of cognitive authority'.

Webster declares his intention of shifting the focus of analysis away from the 'rise of the mechanical philosophy', but in doing so he fails to offer an adequate response to those who see the new philosophy of the mid-seventeenth century as forged partly in opposition to aspects of the occult tradition. Historians such as Margaret and James Jacob have attempted to locate this dialectic socially, in the context of the English Revolution and Restoration. A full synthesis would surely require some assessment and integration of these accounts of discontinuity in the period; and given his qualifications to do so, it is a pity that Webster has declined the opportunity to present such a synthesis.

What he has presented is a skilful and lucid interweaving of some vitally important themes from early modern intellectual history, with wide-ranging reference to primary sources, and complemented by some fascinating illustrations. The result is a provocative study, certain to stimulate much re-thinking and debate among those interested in the origins of modern science.

Jan Golinski

Constructing the Unconscious

Richard Lichtman, The Production of Desire: The Integration of Psychoanalysis into Marxist Theory, The Free Press, New York, 1982, $27.95

Richard Lichtman's book, The Production of Desire, is an original contribution to the literature about the relationship between Marxist and Freudian theories. What distinguishes Lichtman's work from that of other authors in this field (Marcuse, Jacoby, Lasch, Reich, Schneider, Fromm, Lacan etc.) is that it is not eclectic. Thus, Lichtman's integration of Freudian insights into Marxist theory, especially those insights concerned with the repressed unconscious, proceeds from a clear cut view of the internal unity of both Marx's and Freud's entire corpus. His construction of the unity of Marx's work takes for granted an underlying continuity between the early and later Marx and, interestingly, Engels.

His construction of the internal unity of Freud's corpus is concerned to show that Freud's meta-psychology cannot be re-interpreted in a non-causal, phenomenological way and that his clinical practice cannot be separated from the assumptions of his meta-psychology. The point about Freud's clinical practice is made by a thorough re-examination of 'The Case of Dora'. Freud's evidence only supports his conclusion, Lichtman argues, when it is put in the context of both his theoretical and unrelective social assumptions. Lichtman is, accordingly, critical of the integration of Freud's insights into Marx's theory in the manner of Marcuse, who merely accepts a quasi-biological theory of individual drives, or Reich or Jacoby, who want to treat Freud's theory as describing an area of reality distinct from that treated in Marx's theory.

'The key to demystifying Freudian theory,' according to Lichtman, 'is the translation of its "natural" categories into their social meaning.' His critical assessment of Freud's own evidence is meant to bear out the view that the existence of drives, instincts or desires outside the realm of rational self-determination should be understood as a result of social relations rather than as a datum in the study of the human condition. This is the sense in which desire is produced. Lichtman argues further that the gap left in Freudian theory by a critique of its foundations can be made good by integrating Freud's insights into a Marxist theoretical framework.

What results is a theory of the unconscious which, unlike Freud's, see it both as social in content and as being required by particular social relations. The point here is that exploitative social relations, as a condition of their own existence, require false consciousness as well as the repressed unconscious. Thus Lichtman argues against Godelier, as a representative 'structuralist', that, on the assumption that the fetishism of commodities, as a paradigm of ideology, is an inverted reflection of reality, structuralism cannot account for the way in which the real relations are concealed from the agent merely by claiming that they are independent of the agent. 'Search as one will, it is impossible to locate "appearance", "reversals", or "mystifications" of any sort independent of the mind.'

Lichtman's solution to this problem is to enlist the concept of the repressed unconscious to show how individuals bear social relations. It is to this end that he has sought to eliminate Freud's concept of instinct from the theory of the repressed unconscious. As the bewildering variety of rival Marxisms and Marxist philosophies testifies, Marx did not adequately address the relationship between individual subjects and social structure. Thus, on Lichtman's view, Freud's theory fills a logical gap in Marx's theory as well as adding to its explanatory power.

Lichtman makes use of his integration of Marx and Freud to evaluate the possibilities and limits of 'individualised' therapeutic practice in capitalist society. What he calls for is a therapy which can recognise the social character both of the self and of the objects of our deepest needs. This therapeutic process is described as 'collective transference' and its medium is found in the budding movement for democratic socialism.

Lichtman's book, in my estimation, is pregnant with important but unsystematic suggestions about the relationship between his reconstruction of Marx's theory and contemporary political practice. From a philosophical point of view, his underlying theme that nature and society describe the same reality at different levels of abstraction is interesting and controversial. Lichtman, in effect, argues that a desire, at one level of description, involves biological energy, but also that such a fact is too general to explain the socially constituted object of desire, e.g. 'love', or the social form that desiring takes. In my opinion, it is necessary to see that biological and cultural descriptions are about the same reality in order to grasp the hypotheses of cultural anthropologists about certain distinctively human traits, e.g. the loss of a female oestrous cycle in the course of human evolution, the development of a distinctively human symbol-processing central nervous system in the course of human evolution, not to mention the opposable thumb. Lichtman's work will also be of interest to

philosophers for what amounts to a direct assault on the distinction between reason and passion which runs through the tradition of Western moral philosophy as well as through Freud's thought.

What is lacking in Lichtman's reconstruction of Marx's theory is a view of the way in which personal experience, which mediates the fetishised appearances and the underlying reality of capitalist production, is to be understood in relation to class conflict. If the hold of the relations of production depends on enforcing the false, inverted appearances of the reality of social relations, how are these relations, in turn, affected by the gradual restoration of the social dimensions of self in class struggle? How, in turn, is class struggle affected by the intractability of social relations? One wants to be convinced that, in arguing against the structuralists, Lichtman has not yielded too much to them in the way of the passivity of personal experience.

Anatole Anton

Manichean Visions

John Marks, Science and the Making of the Modern World, Heinemann, 507 pp., £9.50 pb

In the recent, and most excellent, film 'Under Fire', President Somoza's CIA sidekick asserts there are but two possibilities: either Somoza restores 'stability' to Nicaragua or 'the Commies take over the world'. Something of the same Manichean vision permeates this book, albeit more subtly. (And for 'Somoza' one should read 'Popper and von Hayek'.) The subtlety lies in introducing the book as one aiming to provide students with 'a coherent intellectual framework within which they can appraise current trends in the history, sociology and philosophy of science', and concluding it by saying: 'Because their fundamental structures are so compatible with those of science and the scientific community, liberal capitalist societies are likely to continue to be much more successful than Marxist socialist societies in developing and applying science' (p. 498).

In order to achieve the reader's agreement, the author needs to have shown that science is a good thing, and that other polities have made - and will continue to make - a worse job of it than ours has. The first of these is shown through a rapid surveyof the history of science and technology since the seventeenth century, the latter through chapters comparing the fate of Jewish scientists in Nazi Germany, Lysenkoism in the USSR, the impact of the Cultural Revolution on Chinese science, and so on. This part is rather interesting, and usefully collects together material usually dispersed through the literature. But whether these examples make John Marks's case is another matter. Evidently there have been, and are, many ghastly regimes. Equally evidently, a 'liberal society' sounds rather nice - 'underpinned by the values of tolerance, pluralism and individual freedoms' (p. 382) - especially if it had any other virtues that come to mind: justice; equality; happiness; love ... Does anyone know where one of these Utopias is?

These enviable attributes 'pluralism, tolerance, individual freedoms and the free flow of information' are also described as 'the values of the scientific community' - this is what is meant by 'their fundamental structures are so compatible' - which leads one to ponder what kind of a world John Marks lives in. We know what he means, of course: that any number of scientists will say (and believe) that these are the values of their calling and by which they live and work. And it could well be that many approach such values more closely than do, say, the members of the present Government. But evidence for his claim is not only lacking, it is positively ruled out by the limitations he has placed on his source material and approach. As he boldly - or defensively - proclaims in the introductory 'Postscript for the Academics', in this book 'The sociological sections are largely empirical and do not deal directly with approaches which speak of "the social construction of scientific knowledge". The philosophical sections emphasize the old-fashioned virtues of induction and empirical falsifiability and do not deal with topics such as the writings of "critical theorists" of the Frankfurt school or with those who believe that, methodologically speaking, "anything goes". These omissions are deliberate.
(The reader should not assume from this small list of 'omissions' that much else of an exciting or relevant nature that has happened in these areas in the last twenty years is discussed, either.) And there is a stronger statement in the 'Guide to Further Reading' (p. 499): after recommending Ben-David's The Scientist's Role in Society and de Solla Price's Little Science, Big Science - his only two suggestions in this field - Marks says: 'Both these books are more accessible to the general reader, and more in tune with the spirit of science and the scientific community, than many more recent writings by sociologists of science.'

A thought-provokling judgement, certainly; I will be content to observe that it is hardly helpful to students in whom one is claiming to encourage 'an informed and intelligent contribution to contemporary debates' (p. vi) to steer them away from any recent work on what scientists actually do, such as Latour and Woolgar's Laboratory Life or Goodfield's An Imagined World. It's even less helpful in the light of the injunction on page 81: 'how important it is to try to establish what scientists actually do rather than simply to accept what they say they do or even what philosophers of science say they do'. Very sage - but the student is given no indication of how to put this into practice.

But let us turn from what isn't in this book to what is. In reading the pages devoted to the history of science - about two-thirds of the book - I was struck by the appropriateness and reflexivity of a remark quoted (Kuhn's judgement on Descartes, in fact, p. 63): 'His vision was inspired, and its scope was tremendous, but the amount of critical thinking devoted to any one of its parts was negligibly small.' This book covers a lot of ground, and in clear and simple prose; the latter an old-fashioned virtue too often neglected nowadays and so to be welcomed here. But this makes the simplicity of the author's approach to history all the more glaring: you feel it ought to be serialised in the Boy's Own Paper. There are lots of great men and discoveries and pictures and anecdotes, but ultimately a sanitized blandness which must stem from Marks's view

of science. An example will illustrate the problem. We learn this about Francis Bacon: 'For many years Bacon's ideas were neglected but, in time, his views on empirical observation, on the usefulness of science and on the necessity of organising scientific activity came to be quoted again and again as authoritative statements on how and why science should be fostered' (p. 81). Now the problem is not that this is untrue, but what it's doing in a book attempting to foster 'informed and intelligent contributions'. We may conjecture that the reason the reader is just left here, with an illustration of truth eventually triumphant, is that (a) Bacon's views are Marks's views - which is fair enough, were it not that (b) this prevents the recognition that to encourage a historical/critical attitude in students the important questions are: why were Bacon's views resuscitated and plugged? In whose interests was this? Whose views were being undermined or attacked by invoking Bacon? What was the social/political/institutional context? Without some such rationale for making the observation, the impression is left that things just happen or are discovered because they are true, which is misleading and mystifying to students at any level.

But John Marks is Lord Acton himself by comparison with Lady Caroline Cox, who contributes four chapters to the book. Under a photograph of the Olympic stadium, the Hippocratic Oath appears, then after Galen we do a bit of time-travelling: 'Europe had descended into the Dark Ages. Then the universities emerged, Europe experienced the Renaissance, and the natural sciences started to burgeon' (p. 296). And with one bound, Jack was free.

(The Hippocratic Oath is fascinating, actually, though we're given no clue as to what we're supposed to do with it: learn it? use it to make inferences about something? Obviously doctors can't hold to it any more, as it contains 'I will not cut, even for the stone, but I will leave such procedures to the practitioners of that craft', which presumably refers to some trade union squabble of long ago. Or does it? This is the recurrent frustration of being deluged with non-contextualised, unmotivated historical information; without knowing why things are told us, the principle of selection and so forth, we're no further advanced in understanding or critical awareness.)

The history in this book is unsatisfactory not because it is inaccurate sentence by sentence (except occasionally, which is bound to happen in a book this length, though I'm surprised that Lady Cox gives quite such a misleading impression of Pasteur's rabies work), but because it is so innocent of any sense of the difficulty of the enterprise, and so unself-critical as to subvert entirely its value as a student text for developing skills and competences and critical faculties in a young audience. But there should be no hesitation about recommending this book to alert and critical students of more experience: there has long been a need for a text such as this, that clearly and unself-consciously demonstrates in practice the impossibility of writing value-free history of science.

John Fauvel

Understanding Understanding

Josef Bleicher, The Hermeneutic Imagination, Routledge and Kegan Paul, 1982, £5.95 pb

It is Bleicher's contention that none of the contemporary theoretical approaches to sociology can be understood without reference to the 'hermeneutic paradigm', and that only a dialectically understood hermeneutic social science can engage properly with the social world at a level which can understand the meanings and structures which it generates, and adequately stimulate a practical apprehension of the requirement to further social emancipation as an internal and essential element of social theory itself.

A central premise in Bleicher's argument is that the social world is irredeemably normative, moral and dialogical, and that social science must accept that it cannot be detached from the processes, contexts and structures of the social world it reflects on. Social theory is a clarificatory practice within the social world and one valued for its critical force and self-awareness. Social theory cannot then just be scientific, structured in the usual subject/-object paradigm, rather it has to be fully reflexive in that it attempts to bring to full awareness not only the prejudices and principles which guide its investigations, but also those determining factors - labour and domination - which structure perception and action. Critical dialectical hermeneutic social science is thus an extension of Hegel's insight - recently excavated by Gillian Rose - that social and historical preconditions underlie cognition and thus limit the possibilities of acting reciprocally.

In Bleicher's view, social theory has three elements. It is historical and shaped by cultural and linguistic traditions. It is dialogical - essentially revisable and subject to clarification, and dialectical since it is committed to relating self-understandings to the objective forces which limit their scope. And, thirdly, it includes 'ideal' moments; inscribed in its project is the notion of the 'good life', of values engendered by historical development but not actually realised in social practice. It is Bleicher's hope that a properly constituted critical social theory could foreclose on the gap, now so evident, between rapid technological change, and moral sensibility - and thus 'progress towards a lessening of the dangers of international confrontation and possible self-extinction' (p. 153).

Bleicher first outlines the universality of the hermeneutic claim whereby it 'aims at uncovering the conditions of science and its truth claim by considering it as a "project": a mode of mastering and using objectifiable processes which is linked to a particular way of viewing the world and of knowledge-acquisition' (p. 3). Hermeneutic reflection disavows the scientistic claim that only a scientific methodology can provide us with knowledge, and claims to show that 'theoretical Reason has itself a normative basis'.

The central argument which Bleicher deploys, through an elucidation of the nature of scientism as it found expression in the Vienna Circle, is that the hermeneutic dimension of intersubjective agreement and given understandings are central to the growth of science, and its mystified self-understandings. Every scientism involves hermeneutic dimensions, whether concealed as in Carnap and Schlick, or given a pragmatic emphasis as in Quine and Peirce. Disavowing the hermeneutic dimension not only con-

ceals dimensions of knowledge from participants in the social world, but, by cancelling practical knowledge as a form of technical expertise, it shrinks the ground upon which socially responsible and rational choices can be made.

The central chapters provide the reader with an analytical discussion of the rise of scientistic sociology in its empiricist, functionalist, structuralist and interpretative forms, as well as providing an evaluation of the evolution of a form of sociological reflection - in Dilthey, Betti and Gadamer - which re-instated practical and normative considerations into the body of social theory. These chapters also evince interesting discussions of symbolic interactionism, Schutz's critique of sociology and Garfinkel's ethnomethodology. Bleicher also recognises the strengths which late Wittgensteinian philosophy, with its emphasis on language and social convention and on intersubjective forms of social existence, grafted onto the purely empirical philosophy of science characteristic of much Anglo-American philosophical expression.

Bleicher's strategy is to show how each of these theoretical approaches to social theory either ignores the 'hermeneutic' dimension foundational to their very enterprise, or how it is not fully realised in non-scientistic theories due to the distorting conceptual framework established within the subject/object dichotomy. Bleicher thus raises a canon of theoretical competence against which social theories can be judged.

Although Bleicher's argument is sketched rather than concisely argued, it is nevertheless fairly successful in bringing out the weaknesses and contradictions evident in much theoretical social science. And Bleicher does make an effective case for the 'hermeneutic imagination' as an indispensable element in social theorising, and it will, when given greater theoretical expansion and direct illustration, contend strongly as a philosophical position able to begin to solve certain of those problems which stand currently in the way of the full investigation of social objects, and of those approaches to the subject which invariably screen out the practical and moral implications of sociological reflection.

To penetrate to those elements which are presently bearing in on, and rapidly undermining, the social cohesion and political consensus which has marked the social and cultural landscape of Britain since the war, to make clear their interlinking and overwhelming force in enacting changes in social consciousness, seems an increasingly difficult, if not impossible, task. Not only do we seem to lack a coherent system of ideas or the moral imagination to confront those overlapping crises, but the changes themselves, operating at structural levels, seem to starve the community of the intellectual capacity to think through the problems clearly and in the universal mode which match their universal impact.

It is fairly certain, however, that it will be from within the debates that currently engage around the construction of an adequate critical social theory that any progress will be made, and, incontestably, hermeneutic argument reinforced by critique will find its force and essential place.

Although Bleicher has only written an introductory text, it will also prove useful to those wishing to survey the field of contemporary theoretical work in the social sciences, and to those wishing to see how critical hermeneutics functions as a form of argument which aims to clarify and to develop in dialogical fashion the space for public awareness, of and control, over those technocratic imperatives which now govern so much of social and economic life.

Vernon Martel

The Numbers Game

Philip Kitcher, The Nature of Mathematical Knowledge, Oxford University Press, 1983, £15 hb

Fresh from his forays against Creationists (see review of Abusing Science, above), Philip Kitcher has turned his attention to a more subtle and widespread, if less news-catching, group of opponents, the Apriorists. These people lurk in every mathematics department and school common-room, and believe in teaching to the innocent young that mathematical knowledge is independent of experience, but resides in some Platonic realm of eternal truths.

Unlikely as this belief may sound, it is even more difficult to see what alternative view could account for the relative stability of cumulative mathematical development down the centuries, and the feeling of other-worldly certainty which attends mathematical discovery. And it is an influential, and convenient, belief politically, in asserting that mathematics thrives pure and unsullied above the mundane struggle, its success a paradigm for what any self-respecting science should aspire to, untainted by ideology or human abuse.

But there are problems: most notably, how can a world of non-empirical, mind-independent truths possibly say anything about our physical world? This is what Wigner has called 'the unreasonable effectiveness of mathematics'. And as our century has shown, mathematics and its applications are all too terrifyingly effective. So a convincing alternative account of how mathematical truth has an empirical basis is to be welcomed. Philip Kitcher has now supplied the most thorough discussion to date. He is not the first to attempt such an enterprise, but is to some extent amplifying and making more rigorous the views of John Stuart Mill that mathematics is an empirical science. (Mill's own, less sophisticated, arguments having retired with egg all over their face, from the witty ridicule of Gottlob Frege.)

Kitcher goes far beyond Mill in demonstrating his claim for all of mathematics, including its most abstract achievements. He rightly observes that two obvious facts have generally been neglected in philosophical discussion: that mathematical knowledge is learned and passed on within a social community; and that mathematical knowledge has developed historically. For Kitcher, knowledge is sufficiently explained by analysing how it has come to be passed along and developed by a chain of knowers starting from 'rudimentary knowledge acquired by perception', several millenia ago.

This last assertion poses a problem for Kitcher's argument that he doesn't altogether guard against. It is a historical claim he is making that mathematics started by ord-

inary sense perception. 'In this way our remote predecessors acquired the first items of mathematical knowledge' (p. 96). Now this may of course be true, but it's quite unsubstantiated here. (There's one thing to be said for Plato, that he knew when he was purveying myth.) If another scenario should turn out to be more plausible - for instance, Seidenberg's arguments that early mathematics stems from religious ritual - then Kitcher's impressive edifice seems left without a foundation stone. I think in fact his later arguments, once the baton of mathematical activity gets started, depend less importantly than Kitcher believes on any particular hypothesis about the firing of the starting pistol. Perhaps he couldn't then call his thesis 'mathematical empiricism' any longer, but that might not matter.

The last part of the book describes some episodes in the history of mathematics when informed by Kitcher's philosophical position. He is quite right to draw attention to the way the aprioristic beliefs of past historians constrained the history they wrote and the questions they asked. But Kitcher's historical account, of the development of analysis, doesn't in the event look very different from anyone else's; and indeed retreats from the implications of his earlier recognition that mathematics is taught and developed within a social context. So this is not the place to look for - or at any rate, to find - a radical historiography. For he is essaying historical discussion in order to illustrate something about his account of mathematical knowledge: that mathematical development is rational.

The emphasis on this point is perplexing. It is almost as though during the course of writing Kitcher became increasingly aware of a bogey-man over his shoulder getting ready to shout 'irrationalist' or 'relativist' or 'Feyerabendian' or some such term of abuse, who had to be appeased or guarded against. Which is a pity. This defensive concentration on whether mathematical development is 'rational' precludes discussion of more interesting questions; for instance, the extent to which some of Wittgenstein's remarks are consonant with Kitcher's approach, or whether the history of mathematics has not a more serious and illuminating role than it is allowed here. Doubtless in a future edition Kitcher will take account of more of the work of scholars on this side of the Atlantic, such as David Bloor and Luke Hodgkin.

Still, this is a rich, deeply argued and thought-provoking book. It is especially gratifying that with no fuss or awkwardness the author uses 'she' as third person throughout. So it can be done.

John Fauvel

Transcendent Aspirations

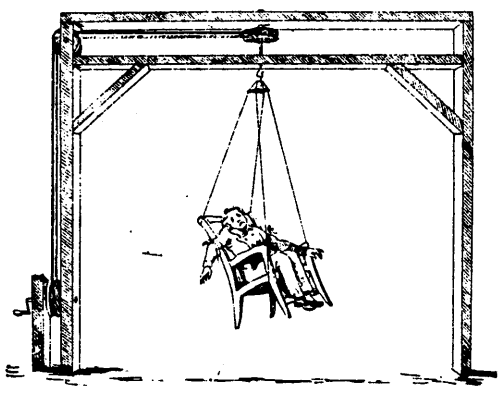
Lawrence LeShan and Henry Margenau, Einstein's Space and Van Gogh's Sky, Harvester Press, 1983, £12.95 hc

'Einstein's space is no closer to reality than Van Gogh's sky.' Thus Arthur Koestler in typically suggestive - and quotable - form, with a remark which seems to question many common sensical notions of matter, truth and mind. The aim of this book is to show the way in which developments in physics and psychology now render such notions, and the classical conceptions of quantification, determinism and mechanical models, untenable - as it were, neither physically common nor philosophically sensible.

The idea that quantum and relativity physics entail great philosophical and psychological readjustments is hardly new. Ever since Einstein, Heisenberg and Bohr reflected on the wider implications of their work, the whole realm of waves, particles, matter and anti-matter, not to speak of 1001 strange entities, has been colonised by positivists, operationalist, organicists, determinists, dialecticians, Catholics, Buddhist and Taoists, all determined to plant their banners in firm scientific soil. So perhaps the first question to ask of another work dealing with the relations of the new physics and philosophy is whether it is more coherent and original than its predecessors.

The answer, so far as I understand the book, is no. Though it is claimed that the dialogue between a physicist (Margenau) and a psychologist (LeShan) will break new ground, indeed, that it will provide the elusive synthesis of physics and psychology as well as of ethics, sociology, psychiatry, economics and parapsychology, what is actually offered is startlingly banal. There are intermittent insights into the springs of artistic creativity and an interesting section is given to research into ESP, but what is at first impressive are the new or redefined terms used and the analytical models which are intended to account for the scientific production process.

The authors deploy concepts such as the 'protocol experience' and the 'construct', and these are meant to capture what underlies all verifiable knowledge whatever realm it treats. Yet, despite the fresh gloss, what is being appealed to is a simplistic notion of empirical verification, a process which by correspondence rules linking experience and concept is designed to connect physical observable entities and psychical unquantifiable terms. This at any rate seems to be the message, though at root it is difficult to gauge whether the authors wish to offer a grand synthesis of knowledge (e.g. p. 39) or instead, and in the name of anti-reductionism ('transcendent elaboration with continuity') to argue that different realms require distinct investigative procedures (e.g. p. 215). The thrust of the book culminates in a notion of mind as sui generis, which somehow transcends the body but interacts with it (p. 239), a defensible position no doubt, but one which the disparate sections of the book hardly support.



The arguments in the text are further hampered by a mass of trivial and seemingly irrelevant examples of the 'just suppose' and 'let's imagine' variety - the case of the businessman who hears his child crying upstairs, then takes his wife out dancing, and then dreams about a kangaroo, described in some detail over four pages would be taken as a parody of the genre in any other book; here however it is meant to illustrate the day in the life of a consciousness. It may be that some readers will be impressed by this book, but, before succumbing to its charms, they will have to negotiate mountains of alarmingly bad grammar and errors of fact (Hume, for instance, is referred to as a 19th century philosopher), the kind of obstructions any competent editor should have cleared away.

Following this, they should be warned that the historical sections are, almost without exception, naive, crude, or simply false. Readers of Radical Philosophy will not need to be told that Comte was not a Renaissance figure (p. 30), or that Marx was not a rigid determinist (p. 147); they should however know that the Copernican Revolution so-called had neither the characteristics nor the immediate consequences the authors declare (pp. 26, 65-66). It is laudable that Harvester should be publishing texts on the history and philosophy of the sciences, but they would have done better to make available Milic Capek's Philosophical Impact of Contemporary Physics (New York, 1961) or Thomas Goldstein's Dawn of Modern Science (Boston, 1980) - both more original and more coherent than what is on offer in Einstein's Space and Van Gogh's Sky.

Mike Shortland

Science and Survival

Gonzalo Munévar, Radical Knowledge: A philosophical inquiry into the nature and limits of science, Avebury Publishing Company, 1981, £16 hb, £8 pb

This book is a discussion of problems of epistemology within a scientific and, in particular, a biological and evolutionary framework. Its case for radical knowledge starts with the 'simple idea' that 'at an elementary level the experiences of an organism are the result of an interaction between its biology and its environment' (p. 20). Perception, and, ultimately, intelligence and science also, depend on a 'frame of reference', and, according to the author's principle of Relativity, 'no matter how good a perceptual or conceptual frame of reference is, many others may be just as good (there are no "preferred" frames)' (p. 18).

The appropriate model for knowledge in this situation is, simply, 'performance': 'a theory provides knowledge insofar as it enables the species that holds it to "get along" in its universe' (p. 52). Rationality is based, not upon standards, but on our preparedness to change' (p. 7), and rational changes are those that improve performance: 'if the practice of science is set up in such a way, that it not only permits but promotes "getting along" in the universe, then it is rational' (p. 120). Against this background science appears as a social or communal enterprise to be judged in terms of its survival value for the species that practises it.

Munévar has written a short (125 pp.) book which tackles the most central and complex issues in the philosophy of science. Misgivings about it arise chiefly from the fact that he has not given himself room to deal adequately with these issues, and on some it is not left entirely clear what his position is. The problem may be illustrated by his treatment of rationality. On his 'official' view, rationality is a kind of blanketing concept applicable to whatever changes promote 'getting along', so that, methodologically, 'anything goes'. But we are also given more traditional formulations: 'rationality lies in how we proceed and change, not in whether our growth is cumulative', and whether changes are rational 'will depend on the manner in which the alternative we choose leads to the growth of our knowledge' (pp. 103-04). If it is a question of the 'how' and the 'manner', we are surely returning to the discarded idea of rationality as a matter of methodological standards.

Moreover, we are now entitled to ask which ways of choosing and which sorts of connection between choices and the growth of knowledge are the rational ones. But these are questions which Munévar, in view of his official commitments, cannot begin to answer. Matters are not helped by the fact that the key notion of 'getting along' is seriously undertheorized. The fullest characterisation is as follows: 'what we have in mind by "getting along in the universe" is that a species be able to survive, avoid great hardships, better the lot of its members and so forth' (p. 71). What this seems to amount to is the claim that science is rational knowledge by virtue of yielding a powerful technology. But doubters may feel that this does not so much solve the problem of the rationality of science as restate it in a way that brings home its urgency for our culture.

Limitations of space do not prevent Munévar's handling of some questions from being entirely convincing and successful. This is true, for instance, of his challenge to the conventional division between philosophy and science, with its reminder that 'philosophers make crucial and often implausible, empirical assumptions in what they think are pure conceptual investigations' (p.3). There is also his demolition of the main pillar of traditional empiricism, the assumption of a theory-neutral observation language. Munévar's conclusions about this are, of course, now widely accepted, but it is valuable to have the case presented as trenchantly as it is here.

The entire book is written in elegant, vigorous, jargon-free prose that captures something of the manner of Paul Feyerabend, who contributes the Foreword. Indeed, readers may well feel that, although Munévar is indisputably his own man, there is something Feyerabendian about the general conception of his project and the spirit of its execution. As such, his book will be seen as one of the surprisingly rare, successful attempts to do independent creative work in that vein, and it deserves to be read by everyone interested in contemporary philosophy of science.

Joe McCarney